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SCIENCE PROGRESS

AN EXTINCT MALAYAN FLORA IN ENGLAND

By A. C. SEWARD, Sc.D., F.R.S.
Professor of Botany in the University of Cambridge

THE title chosen for this article is not a mere fancy of my own; it is the expression of a remarkable conclusion, based on ample evidence and elaborated in a volume—the *London Clay Flora*—by Mrs. E. M. Reid and Miss M. E. J. Chandler, recently published by order of the Trustees of the British Museum. The outstanding merits of this most welcome contribution to the strange history of the British Flora has been officially recognised by its publication in the largest form of Museum "Catalogue" hitherto devoted to fossil plants; the size of the volume and the high standard of the printing and illustrations are worthy of the contents, which represent the results of seven years' intensive study of several thousand specimens of fruits and seeds by the two leading experts in a fruitful, very difficult, and highly specialised branch of botanical research.

THE FLORA OF THE LONDON CLAY

We will first confine attention to the nature and affinities of the Lower Tertiary London Clay flora and then briefly consider it in relation to other floras; some of earlier date and others which have left records in sediments of different ages connecting the flora of the London Clay with the present plant-population of Britain.

The table on page 2 includes the various geological series to which reference is subsequently made and shows the position of the London Clay in relation to other plant-bearing beds · the plants with which we are mainly concerned lived in the earlier part of the Tertiary era, at a time separated from the present by, let us say, rather less than 100 million years. Underlying London there is a considerable thickness of marine, blue-grey clay through which we travel in the underground railway:

> There where the long street roars, hath been
> The stillness of the central sea.

28138 | '36

The same formation extends over a wide area in south-eastern England; in many districts it determines the surface features; in others it lies hidden below later deposits of sand and gravel. In Essex the clay reaches a maximum thickness of about 500 feet; in Wiltshire, nearer its western boundary, it is only a few feet in depth: the same clay occurs in Hampshire and the Isle of Wight, and is represented by sedimentary material of the same age in Belgium and northern France. On the north coast of Kent in the island of Sheppey the clay forms cliffs 200 feet high, which have yielded the richest collections of fruits and seeds. It is noteworthy that the fossils were not actually found in the clay but picked up on the foreshore at Sheppey, Herne Bay and other places where they occur in company with brown nodules of iron pyrites and white flints, the refuse that remains after the ceaseless eroding action of the sea. The clay is a sea-mud deposited on the floor of a large estuary and in comparatively shallow water which covered a considerable portion of the land that is now south-eastern England. A large river, flowing from a source which cannot be definitely located, carried in suspension far beyond its mouth the finer scourings from the valley-rocks and on its surface drifted logs of wood —showing signs of long floatation in the holes bored by *Teredo*— broken twigs of trees and innumerable fruits and seeds, which with bones of crocodiles, sea-snakes and the hard parts of many other animals were eventually entombed in the water-borne sediment. The wood and other flotsam from the forests serve as samples of the vegetation which grew on the borders of the sea and along the mud-banks. The London Clay, unlike many other plant-bearing sedimentary rocks, has not furnished impressions of recognisable

QUATERNARY PERIOD	The present stage passing imperceptibly into
	Post-Glacial stage
	Glacial stage
TERTIARY PERIOD	Pliocene stage
	Cromerian flora
	Teglian flora
	Castle Eden flora
	Reuverian flora
	Miocene stage
	not represented in England
	Oligocene stage
	Bembridge and Bovey Tracey floras
	Eocene stage
	Hordle flora
	LONDON CLAY FLORA
CRETACEOUS PERIOD	Upper Cretaceous
	(very few plants in English rocks)
	Lower Cretaceous
	Wealden flora
JURASSIC PERIOD	Rich plant-beds in Yorkshire

leaves ; it provides a rich store of reproductive organs which, with pieces of wood, were carried as floating rafts to a greater distance than the foliage Mrs. Reid and Miss Chandler quote a description given by H. N. Moseley, the naturalist of the *Challenger* Expedition, of driftwood and fruits off the coast of New Guinea where the great Ambernoh River runs into the sea ; a description which reads "as though it were the authentic record of what had existed in the London Clay sea millions of years ago." The sea was connected with the Tethys ocean farther south, which for many ages stretched across the world from west to east in a deep and slowly subsiding trough, the collecting-ground of sediments of successive geological periods that were destined to be upheaved, not long after the period of the London Clay, into the Alps, Carpathians, Balkans, and Himalayan mountains. When the clay was being spread over the sea-floor there were no transverse barriers of alpine chains in the path of wandering plants ; routes of migration between north and south were still open. Later in the Tertiary era, when the greater part of the Tethys had been transformed into ranges of mountains, limits were set to the free interchange of plants from one region to another ; and, as we shall see later, this geological revolution was a determining factor in reducing the European floras of the Tertiary period to a position far inferior to those of North America and the Far East. The present relatively poor botanical province, of which the British Flora is a humble component, owes its inferiority to geological causes. The history of the plant-world is closely bound up with the history of the rocks . the geologist sets the scenic background ; the botanist reconstructs part of the drama.

The fossils of the London Clay attracted the attention of naturalists as long ago as the early part of the eighteenth century, but it was not until well into the nineteenth century that a serious attempt was made scientifically to study the fossil plants. In 1840 Dr. J. S. Bowerbank (1797–1877), a city merchant, whose business demanded "constant and careful attention for at least twelve hours a day," published a volume entitled *A History of the Fossil Fruits and Seeds of the London Clay*, the only instalment of a projected series of five parts which was issued. Bowerbank's work is described as a model of what a palæobotanical study should be : he made many mistakes ; but when we remember that he wrote nearly a century ago and that his scientific work was done after a hard twelve-hours business day, we can only admire his determination and his successful cultivation of a hobby. Lists of Sheppey plants published by Baron Ettingshausen and other professional authors are practically valueless. The Bowerbank

Collection, acquired by the British Museum in 1865, also a large collection made by Mrs. Reid and Miss Chandler, and presented by them to the Museum, are the chief sources from which the latest information has been obtained. It is interesting to note that in the course of two short visits to Sheppey and Herne Bay the authors gathered more than a thousand specimens representing 122 species, 67 genera, and 34 families. All the fossils were found on the foreshore and it was only after a good deal of practice that fruits and seeds could be detected among the nodules of pyrites and other pebbles on the beach. Fruits and seeds are themselves impregnated with pyrites which fills cavities and spaces, and it needs no little care and experience to distinguish between plant-structure and the introduced mineral. Apart from difficulties caused by methods of preservation and the partial destruction of external features by maceration in sea-water and subsequent abrasion, there is the serious difficulty of obtaining fruits and seeds of living plants with which to compare the fossils. Very little attention has been paid by systematists to the structure of fruits and seeds as diagnostic characters: no standard collections were available. The authors had to make their own collection from herbarium material in Europe and through the co-operation of botanists in many parts of the world. Now that the authors have demonstrated after many years of laborious work that fruits and seeds have a very high value as guides to affinity, botanists are beginning to realise that herbarium material is insufficient and inadequate for palæobotanical purposes unless it includes mature reproductive organs. The determinations are based upon many characters in addition to external form, which is often misleading even when the specimens are intact; they rely chiefly upon internal structure, the number of carpels in a fruit and their arrangement, the methods of dehiscence and of germination. It is only an optimist who would expect to find in battered fruits evidence of methods of germination, but fortunately the authors are optimists and to their persistence and ingenuity in devising a suitable technique, the fossils have yielded up their secrets. From a comparison of many extinct types of fruit with their living representatives light has been thrown upon the nature of evolutionary changes in constructional plans. In some instances the London Clay fruits differ from those of recent species or genera in having a larger number of loculi: in the genus *Nyssa*, for example, a North American, Himalayan and Malayan tree, the fruit has a single loculus, whereas fruits of extinct Tertiary species have three to four loculi. Other genera furnish similar evidence of reduction in the course of evolution.

Excluding Diatoms, which are represented by several forms, 234 named species are recorded: 2 species of *Chara*, 5 species of conifers and the rest flowering plants. The flora of the London Clay as represented by the available samples is unquestionably very far from complete: when we speak of the composition of an extinct flora we usually mean lists compiled from the waifs and strays that were swept by rivers into deltas and other areas of deposition. The London Clay flora was certainly not composed almost entirely of dicotyledons as the published lists would seem to suggest; there must have been many more conifers than the few that have left traces in the clay, and it is equally certain that ferns and their allies also played a part in the vegetation. At the best a fossil flora is only a selection of certain types of plants which grew in localities near enough to flowing water to furnish a few examples for us the "afterthoughts of Creation." Moreover, as we have seen, the debris preserved in the mud of the Londinian sea is very much the same as that now met with in the drift off the mouth of a strongly flowing river. Most of the species of the London Clay flora are referred to present-day families, but not to living genera; there is not a single plant specifically identical with a living one. The families are for the most part unfamiliar to those botanists whose chief interest is in the British flora; and many families that bulk largely in our present flora are unrepresented. Extinct species and genera present many problems to the systematic botanist; the question constantly recurs, whether or not certain peculiarities in the fossil—differences in the structure of fruit, seed or embryo—are of sufficient magnitude or importance to justify the establishment of a new genus, or whether the definition of a genus based on living species might reasonably be extended to include the extinct form. There is no golden rule; common sense and individual judgment are the only guides. The authors, so far as a layman can judge, seem to have used these guides with judicial wisdom. The proportion of extinct genera in the flora is approximately 75 per cent.

A selection of plants, chosen at haphazard, will serve to illustrate the nature of the data: one of the few genera named from fragments of twigs and not seeds is an *Araucarites*, a species of conifer closely allied to some living Araucarias, as, for example, *Araucaria excelsa*, the Norfolk Island Pine indigenous to the small island in the Pacific Ocean. This is one of several records of the occurrence of an Araucarian conifer in Tertiary Britain: the genus is now confined to Brazil, Chile and the Argentine in the west, and to the Australasian region in the east: here we have one of many illustrations of the importance of taking into account the facts of geographical distribution obtained from an examination of the records of the rocks. It is only from a knowledge of floras of the past that we fully appreciate the efficiency of plants as travellers over the world's surface. Another conifer discovered in the London Clay, is *Cupressinites curtus*, founded on cones and seeds; it is compared with the north African genus *Tetraclinis* of the Atlas Mountains and with the tropical and South African *Widdringtonia*.

The genus *Cephalotaxus*, now confined to the mountains of China and Japan, once grew by the banks of the Eocene river in western Europe. Monocotyledons are represented by several Palms, including a single species of *Nipa* the "stemless Palm." The only Palm that is still European is the dwarf Palm (*Chamærops humilis*) of the Mediterranean region; the genus *Nipa* is widely spread in brackish water of Old World tropical estuaries. In the London Clay flora species of five living genera have been recognised and seven forms of seed are referred to an extinct genus, *Palmospermum*, a name implying an undoubted Palm alliance but not referable with certainty to a generic position within the family. The most abundant fossils in the whole collection are the woody single-seeded fruits of *Nipa Burtini*, a species closely allied to the recent *N. fruticans* but with rather smaller fruits: as the nuts are now carried on the water of the Ganges, and other tropical rivers, so we can picture them falling from the fruit-heads among the palm-like leaves of the *Nipa*, which grew on the shores of the Eocene sea. The London Clay species has been found in other Tertiary beds in England, also in Belgium, France, Italy, Russia, and in the upraised bed of the Tethys sea that now forms the Mokattam hills on the outskirts of Cairo.

. By far the greater part of the fruits and seeds belong to dicotyledons and are distributed among rather more than forty families, a few of which will serve to emphasise the contrast between the Eocene and the present flora. There are five species of Juglandaceæ, a family represented in Britain only by the Walnut and a few other cultivated trees: one species is referred to a living genus, *Platycarya*, a native of China and Japan. In the opinion of the authors the name *Platycarya* must now give place to *Petrophiloides*, a generic designation which, they maintain, has prior claim because it was adopted by Bowerbank for fossils, generically the same as the Far Eastern plant described three years later than the date of Bowerbank's account of the London Clay specimens. The other four species are placed in a new genus—*Juglandicarya*, a name indicative of relationship with the Walnut (*Juglans*) and the Hickory (*Carya*). The family Nymphæaceæ is represented by a single species assigned to a new genus and believed to be closely related to *Barclaya*, the species of which grow in fresh water in the tropics of eastern Asia. It is interesting to find that the London Clay plant is more nearly allied to a Far Eastern genus than to the water lilies of Europe. Of the tropical family Menispermaceæ there are five genera and seven species: the fruits of this family have well-defined characters in which it is easy to trace generic relationship: four of the five fossil species differ too much from the fruits of any living plant to be placed in a recent genus and thus afford a good example of the preponderance of extinct over recent genera in the London Clay flora. One of the extinct species is said to be nearest to the living genus *Tiliacora*, a genus of climbing plants inhabiting India, Ceylon, Further India, the Malay Peninsula and Islands, and tropical Africa. Another species of Menispermaceæ is also compared with a tropical climber, *Tinospora*, a Chinese, Malayan, Australian and west African genus; a third type of fruit of the same family bears a closer resemblance to that of *Hypserpa*, an Indo-Malayan genus. The genus *Magnolia*, native of Asia and North America but no longer European, is represented by several species The family Anonaceæ, characteristic of the tropics of the Old World, had representatives in the flora of the London Clay. The tropical and subtropical family Lauraceæ had several members in Tertiary England and is second only to the Nipaceæ in the number of species: among living genera of the Lauraceæ, to which fossils have been referred, are: *Endiandra*, a Malayan and Australian genus, the Indo-Malayan *Cinnamomum* (Cinnamon and Camphor), *Litsea*, a tropical Asiatic and Australian genus, and another tropical genus *Beilschmiedia*. Other species are placed in extinct genera of the Lauraceæ. It is surprising that only a single fruit has been found which can be definitely assigned to the Leguminosæ, a family that is now prominent in tropical floras. Many fruits are referred to extinct genera of the tropical family Burseraceæ: the characters of two of the genera indicate that "a recombination of characters has occurred in the family during the long lapse of time since the period of the London Clay." Similarly the Euphorbiaceæ are represented only by species included in two extinct genera. Eleven species are referred to a section of the family Anacardiaceæ: two are placed in the genus *Dracontomelon*, a tree of tropical jungles in eastern Asia, another is referred to *Spondias*, a tropical genus which now occurs in both the Old and the New World; two species are assigned to *Odina*, a tropical African and Asiatic genus; other species are placed in extinct genera of the Anacardiaceæ. Another family which occupied a prominent position in the Tertiary flora is the Icacinaceæ, now exclusively tropical in its range: there are six genera and about twenty species. The Lythraceæ, one of the few families with species native to Britain, are represented by two extinct genera, one of which is compared with *Lagerstrœmia*, a genus that is now abundant in India, China, East Indies, Australia and other regions. Another family, Cornaceæ, one of the few with members that are still native in Britain, is represented by three species of *Mastixia*, an Indo-Malayan genus, which is the only living member of the section Mastixoideæ, and by four extinct genera, three of which are considered to belong to the Mastixoideæ. These families suffice to emphasise the enormous contrast between the London Clay flora and the present flora of western Europe; most of the others tell the same story.

All the families recognised in the fossil flora are now represented in the tropics; five are exclusively tropical—11 per cent. of the whole. Fourteen families are almost confined to the tropics—32 per cent. Twenty-one are equally tropical and extra-tropical in their present geographical range, that is 46 per cent. Only five families—11 per cent—are chiefly temperate. There are seventy genera, the living representatives of which have been traced (that is genera which are elements in modern floras), and a greater number of genera which no longer exist. From a careful analysis of the present geographical distribution of the plants to which the fossil fruits and seeds show the nearest affinity, the conclusion is drawn that the closest relationship of the London Clay flora is with the "very heart of the East Asian tropics, mainly with the Malay Islands, where 73 per cent. of the genera are found at the present day." Next in order are: the Malay Peninsula with 67 per cent., Further India with 66 per cent., India and Ceylon with 61 per cent. The relationships of the British Eocene flora are very definitely with floras separated in space by some thousands of miles from western Europe; there is hardly any affinity to the present floras of Europe and western Asia; with America the relationship is also slight. As we shall see later, this surprising conclusion based on the critical work of Mrs. Reid and Miss Chandler is not inconsistent with the history of the western European floras as recorded in the plant-bearing beds of more recent geological formations. Of the seventy London Clay genera thirty-three (47 per cent.) of the most nearly related living plants occur in tropical lowlands; twenty-nine genera (41·5 per cent.) are lowland tropical "and, as well, either montane in the tropics or extra-tropical in latitudinal distribution" ; eight genera (11·5 per cent.) are "either exclusively montane in the tropics or extra-tropical." It would therefore seem that the old conception of the London Clay flora as tropical receives confirmation from the latest and by far the most thorough analysis to which it has been submitted. The majority of the plants are most closely allied to genera now living in tropical rain-forests in the Indo-Malayan region. The authors quote a description by Beccari of the primeval rain-forests of Borneo which, in their opinion, despite certain differences, affords the truest picture of the vegetation fringing the estuary where the fruits and seeds, collected on the foreshore of Kent, had been embedded in sediment deposited by the Eocene river in an area which now comprises south-eastern England.

WHAT WAS THE CLIMATE IN WHICH THE PLANTS OF THE LONDON CLAY FLOURISHED ?

The flora of the London Clay is described as tropical because the plants determined on the evidence of fruits and seeds, which there is good reason to believe are trustworthy criteria of systematic position in the hands of experienced investigators, are more nearly related to genera now living in the eastern tropics than to temperate genera. May we then draw the conclusion that the climate of south-western Europe in the early part of the Tertiary era was comparable with that of a tropical rain-forest ? May we place confidence in fossil plants as true indicators of the climate of ages separated from the present by many millions of years ? These questions are fully considered by the authors and answered in the affirmative ; they believe that the London Clay flora can only be regarded as tropical and that plants are true recorders of climate, "only it must be the bulk of the flora that interprets the climate, not a few exceptions." This is the considered opinion of two botanists ; it is also consistent with the fancy of a modern poetaster, who wrote of the warm Eocene sea :

> And with its tides laid down the London Clay,
> When, genial, a lost Alaska grew
> Broad-blossomed trees, and the Magnolia stole
> Warm-scented to the Pole.

Dr. Bowerbank, who was equally impressed by the tropical character of the flora, suggested that the plant debris and the sediment in which fruits and pieces of *Teredo*-bored driftwood were embedded had been transported by a river from some southern source probably near the equator. He evidently hesitated, and well he might, to assume tropical conditions on the latitude of the London Clay.

His explanation is examined by Mrs. Reid and Miss Chandler, who give cogent reasons against its acceptance: when the London Clay was being deposited over the area which it now occupies, southern Europe was on the edge of a great trough occupied by the transmundane Tethys ocean or Nummulitic sea—so-called from the abundance in the Lower Tertiary limestones that were upraised in the Alpine revolution, of the foraminifer *Nummulites*: this well-established fact is in itself a fatal objection to Bowerbank's supposed equatorial river. It is true that vegetable detritus is often carried far from a forest-area and ultimately finds a resting-place many miles from the original source, but the drift-material now characteristic of tropical beaches consists for the most part of fruits and other remains of strand-plants within reach of oceanic currents. If the fruits from the London Clay had been carried long distances by currents we should expect to find a preponderance of examples having some of the structural features that are known to be characteristic of fruits able to withstand long journeys in sea water. Among the Sheppey species there are very few which show any "special adaptation for floating." There is a further point: drifted material brought by the Gulf Stream to the shores of Europe is always relatively insignificant in amount in comparison with debris of local origin. If some of the Sheppey fruits belonged to plants alien to the locality where they were preserved, we should expect to find in association with them a still greater number of fruits derived from native plants. But it has been shown that the London Clay flora is homogeneous and is not made up of elements of two distinct types of plant communities. Though the fruits and seeds are not preserved on the actual site where the plants grew, the evidence is clearly in favour of the conclusion that they fell from trees growing within reach of the river which built up the delta where they were deposited. Granting that the forests grew at no great distance from the London Clay area, is it necessary to assume that in the early part of the Tertiary period southern England enjoyed a climate comparable with that of the Indo-Malayan region which, in the numerical composition of the flora, furnishes the closest parallel to that of the London Clay?

Mrs. Reid and Miss Chandler, with many other authors who have faith in fossil plants as guides to climatic conditions, naturally treat the records preserved in the London Clay as they would treat a collection of living plants. It would seem logical to employ knowledge based upon known relationship between assemblages of genera and species and climatic conditions, in the world as it is now, to the climatic interpretation of groups of extinct plants closely allied to existing genera and species. The present is a guide to the past if we hold the view that a certain type of flora, whether its component elements are still living, or whether the flora has no elements identical with existing plants though a close relationship is clearly established, connotes a certain environment and a mean temperature that can be determined within narrow limits. There are few more attractive and baffling problems with which the student of ancient floras is confronted than those relating to past climates. The botanist may be convinced, from his knowledge of plants in relation to present external conditions, that he can use botanical documents in the rocks as trustworthy guides and deduce from them a reasonably accurate estimate of climatic conditions; but his conclusion may meet with serious opposition from experts in other branches of knowledge, from astronomers, meteorologists, geologists and geophysicists

Let us first consider the problem from a botanical standpoint, not, it must be admitted, without reference to non-botanical considerations, but with the knowledge that there are strong objections from certain sources to postulating a tropical climate in a temperate country at a period which, in a geological sense, is relatively recent. The authors of the *London Clay Flora* have carefully considered the general question of the trustworthiness of extinct plants, or rather of floras composed wholly of extinct species and in great part of genera which no longer exist. Their considered opinion is that an early Tertiary flora must be regarded as strictly comparable to an existing flora in its validity as a standard reference in reconstructing climatic conditions. Attention may be called to a suggestion that has not been overlooked by Mrs. Reid and Miss Chandler, namely the possibility that in the course of ages plants may have changed in their reactions to external factors. The correctness of this view, which the authors quote from published statements made by myself, is impossible to prove and not easy to disprove. They admit a certain power of adaptability to unaccustomed climate among present-day plants, but they add "such slight evidence . . . can count for very little against the general evidence of plant distribution." They also say, and with justice, that the weight of evidence from the living world "does not indicate any very general power of adaptability among flowering plants as a whole. And it would be rash to attribute any greater power of adaptability in the past without very definite evidence to support it." The fairness of their statements is not disputed, but they do not in the writer's opinion dispose of the suggestion that plants in the course of ages—during a period of many million years—may undergo some constitutional change, may, in fact, suffer from the effects of senility. A suggested solution, unsupported by definite evidence, of a difficult problem, has, perhaps, little value; none the less, it need not on that account be discarded as inadmissible. The point which it is worth making is this: there is now a flora of a certain type attuned to the conditions provided in the Malayan region; the ancestors of many plants in that flora lived in what is now south-eastern Europe in the earlier part of the Tertiary era. May we not hazard the opinion that Malaya is a refuge where the enfeebled descendants of a race which was composed of elements hardier and, in an evolutionary sense, much younger, are able to flourish because their constitution is no longer such as to permit of continued existence in an environment which had been appropriate to their remote and more vigorous forbears? This possibility was in the first instance suggested by contrasts, much more amazing than that between southern England and Malaya, furnished by fossil arctic floras and existing tropical floras which include elements closely allied to the extinct plants which once flourished 300 miles north of the Arctic Circle. It is impossible in a single article, primarily concerned with questions that are non-controversial, adequately to discuss the wellnigh insuperable difficulties involved in the assumption that fossil plants and extinct floras should rank equally with assemblages of living plants as indicators of climate.

Some reference must, however, be made to other aspects of the subject which are ably dealt with by the authors. The science of Geophysics offers another line of enquiry which may lead to the discovery of at least a partial solution of problems presented by the occurrence of fossil floras in places latitudinally remote from the homes of the most nearly allied recent floras. The mobility of the earth's crust is manifested in many different ways: its importance as a contributory cause of the present and past distribution of organisms cannot as yet be estimated. Wegener's hypothesis of continental drift is an old idea, restated and more fully developed, which despite strong opposition is often welcomed with avidity as a possible way out of a dilemma. Wegener believed that Europe and North America were once part of a single continental mass; this was severed by a gradually widening rift into two separate blocks which drifted apart, and the rift became the sundering Atlantic ocean. This particular form of continental drift is inapplicable to the facts revealed by the London Clay flora.

An important point made by Wegener is that America and Europe were still in actual contact as recently as the end of the Tertiary era; they had been partially separated at an earlier date as the rift, which began in the south, spread towards the north. This idea involves a continuity of land between Europe and North America at the stage of geological history when the plants of the London Clay were living; we should therefore expect to find evidence of a clearly marked relationship between the floras of the Old and New Worlds, but it has been definitely established that the American affinities are insignificant in proportion to those with existing floras in the Far East. Mrs. Reid and Miss Chandler are obviously right in saying that Wegener's hypothesis "will not hold." And yet the failure of Wegener's hypothesis as an aid to the solution of the phytogeographical problem presented by the Tertiary flora is not a fatal objection to the general idea of drifting continents. There remains the possibility of movements of the upper layers of the earth's crust in other directions than those assumed by Wegener. An entirely different form of a drifting crust—continental spreading—advocated by the German physicist, Gutenberg (described by Lake in the *Geological Magazine* of 1933) serves to show that the subject is by no means exhausted and is indeed in its infancy. We have as yet no proof of continental drift, and no form of it so far described is applicable as an explanation of a Malayan flora in Eocene Europe; but this is not a valid reason for ruling out the possibility of geophysical assistance in our search for light on the phytogeography of past ages.

There remains another line of approach to the problem of climatic fluctuations which is more in accordance with definite geological evidence. It is well known that land and sea have frequently changed places: even if the great ocean-basins and continental blocks are regarded as permanent features in a general sense, their boundaries have changed and the distribution of land and water has varied within wide limits from one age to another. It is for example indisputable that the Tethys sea occupied a large part of south central Europe, north Africa and regions farther east. The meteorologist, Dr. C. E. P. Brooks, believes that such changes in the inter-relationship of land and water, as it is reasonable to assume from geological data, and the consequential changes in the direction and temperature of ocean currents, would afford at least a partial explanation of the higher temperature in western Europe deduced from the composition of the London Clay flora. Mrs. Reid and Miss Chandler say: "Dr. Brooks in his letter makes it plain that in his opinion his hypothesis will not, alone, account for a climate able to support the London Clay flora, but that, by invoking the aid of Dr. Simpson's hypothesis of change in solar radiation, the requirements would be more nearly met. At least the two hypotheses together have the merit of postulating the kind of climate that would be required by the Eocene flora, if we may judge by the needs of its living representatives—an intense degree of precipitation, a soil always loaded with moisture, a climate always equable with warm winters." The authors, not fully convinced that they had solved the problem, determined to enquire more closely into the climatic conditions in the tropical rain-forests of Malaya and other regions. They obtained much valuable information from different sources and found, for example, that botanists with tropical experience are agreed that "tropical rain-forests extend their range upward in moist and sheltered ravines considerably beyond the range in the surrounding open country." Their conclusion is that "given suitable conditions of precipitation, of the balance between precipitation and evaporation, of the seasonal distribution of the rainfall, of a very equable climate, and a frostless winter, the London Clay flora might have lived under a mean annual temperature of about 70° F. Dr. Brooks, in reply to a request for his opinion, wrote: "It is, of course, impossible to give exact figures for past temperatures, and though I think a mean annual temperature of 70° rather high for latitude 52°, I cannot say it is impossible." The mean annual temperature of London is approximately 51° F. Enough has been said on this fascinating problem of climate to show that the authors have spared no pains to deal with it as thoroughly as possible. While fully appreciating the judicial balancing of evidence, there still remain in my mind an element of doubt and scepticism and a disinclination to abandon the suspicion that the value of the plants as thermometers of past ages is not so great as is usually supposed.

Comparison with Other Floras

If space permitted it would be interesting to make comparison of the flora of the London Clay and the floras of the latter part of the Mesozoic era: a few general statements must suffice. In the latter part of the Cretaceous period the sea covered a much greater area in Britain than it did in the early stages of the Tertiary era: we know from collections of plants obtained from sediments in other countries where Upper Cretaceous records are available, that in general composition the vegetation in the northern hemisphere was very similar to that of the Tertiary period, at least in the preponderance of Angiosperms closely related to living genera. Plants discovered in the highest Cretaceous strata represented in England, particularly fossil wood from the Lower Greensand, show that dicotyledonous trees of several kinds were elements in the vegetation.

On the other hand the Wealden Flora, obtained from an older Cretaceous series, differs fundamentally from that of the London Clay in affording no evidence of the existence of Angiosperms of the modern type. The flora is essentially Jurassic and represents the latest phase in a sequence of floras, differing from one another mainly in minor features, which lasted from the latter part of the Triassic period to the earliest stage of the Cretaceous period. The plant-world was transformed shortly after the Wealden stage; the older Mesozoic type of flora was replaced by one in which flowering plants similar to existing forms occupied the chief place. There is evidence in favour of the view that this new element came from a home within the Arctic Circle whence it spread into Europe, America, and Asia. Some of the families and genera represented in the Eocene flora of the London Clay are known to have existed in western Greenland in the Cretaceous period. Further research into the component elements of Cretaceous floras should be stimulated by the publication of the British Museum volume: we should like to ascertain to what extent it is possible to connect the London Clay genera with ancestors in more northern regions.

The London Clay flora may be described as marking a culminating point in the development of a vegetation in western Europe which was mainly composed of Angiosperms having an Indo-Malayan relationship. Let us briefly review the procession of floras in the later stages of the Tertiary era and in the Quaternary period.

There is still much work to be done on the Tertiary floras of Britain but enough is known to enable us to make comparisons with the Lower Eocene flora of the London Clay. We will first glance at an Upper Eocene flora described by Miss Chandler from Hordle, Hampshire: the plant-bearing beds were deposited by a river in an estuary which extended over part of Hampshire and the Isle of Wight. The flora is "closely allied to the living flora of south-eastern Asia," while there is a marked relationship with that of east North America. Many of the genera are the same as those in the London Clay, but there are significant differences in general composition that are worthy of note. In the London Clay flora about 97 per cent. of the plants were trees or shrubs, not herbaceous; in the Hordle flora woody plants form 85 per cent.: in modern Britain the proportion is 17 per cent. Other differences are apparent when a comparison is made of the percentages of genera in the two Eocene floras based on geographical relationships: the proportion of Malay Peninsula plants, which is 67 per cent. in the London Clay flora, is 60 per cent. in the Hordle flora, and a more striking contrast is shown by the substantial increase in the younger flora of European genera. These percentages are taken from a table drawn up by Mrs. Reid and Miss Chandler, part of which in a simplified and less complete form is here reproduced.

	London Clay.	Hordle	Bembridge.	Reuver.	Tegelen	Cromer	Britain (Living)
N. Europe .	1	20	23	37	64	71	46
N. North America	1	14	20	33	53	62	35
N.-W. Europe	2	26	37	55	80	98	100
S. Europe	4	37	51	62	82	94	94
W. Asia	4	43	54	64	80	95	85
Himalayas . .	31	54	57	70	79	83	70
Japan and N. China	36	71	71	83	85	82	67
S. China and Burma	53	80	69	60	64	55	42
India and Ceylon	61	60	57	54	62	62	48
Further India	66	74	60	55	56	50	32
Malay Peninsula	67	54	60	42	47	44	27
Malay Islands . .	73	60	63	47	51	49	32
Australia, New Zealand, etc	40	46	60	39	50	47	35
E. U.S.A. . . .	16	46	49	64	76	79	59
W. U.S.A.	7	43	40	51	68	78	60

Table abridged and compiled from the Tables and Diagrams given by Mrs. Reid and Miss Chandler "showing the percentage distribution of genera in certain fossil floras of West Europe and in the living British flora." Mrs. Reid tells me that by the terms "North Europe and north North America in the left-hand column were intended the arctic and sub-arctic regions of the continents."

A few years ago Mrs. Reid and Miss Chandler made an intensive study of an Oligocene fossil flora from Bembridge on the north-west coast of the Isle of Wight. The Bembridge beds were probably deposited in a delta, and the leaves, fruits and seeds were derived from neighbouring vegetation. The proportion of woody plants is 57 per cent., considerably less than in the older Hordle flora. In the Bembridge flora there are a few more wide-ranging genera than in the Hordle flora and a much greater number than in the London Clay flora; the percentages are—29 per cent., 26 per cent. and 1 per cent. Instead of a preponderance of Malayan elements, we find that the closest relationship is with north China and Japan, south China and Burma being not far behind: in the Hordle flora the closest connection is with south China and Japan. The division of the Tertiary formation next in age to the Oligocene series, in ascending order, is the Miocene, a stage unrepresented in Britain. Miocene floras from Switzerland, Germany and other European countries, so far as they have been critically examined, supply information consistent with the gradual transformation of the older Tertiary floras related in different degrees to the Malayan region into floras exhibiting a closer affinity to those of China, North

America, and Europe. It would seem that the extermination of the Chinese-North American element began in the Miocene.

Three Pliocene floras will now be briefly considered: the youngest of them, referred to the upper part of the Pliocene series, is from plant-beds on the Norfolk coast, the Cromerian flora. Another and older British Pliocene flora, described by Mrs. Reid from Castle Eden on the Durham coast, differs from the Cromerian in having a much higher percentage of exotic and extinct species, 64 per cent. as compared with 5 per cent., as well as of Chinese-North American species, 31 per cent. as compared with 0·74 per cent. in the Cromerian flora. Two Pliocene floras from the Dutch-Prussian border, which are richer in species than the smaller Castle Eden flora, supply interesting data and fill in gaps in the sequence represented in Britain. The older of the two Dutch floras is the Reuverian, from Reuver, and the younger is the Teglian from Tegelen, a neighbouring locality: the three localities, Reuver and Tegelen on the east side of the North Sea and Cromer on the west, are situated within the area of the delta of the old Rhine which impinged upon what is now the Norfolk coast. England was united to the Continent across the southern half of the North Sea, the northern part of which was occupied by a gulf of the Atlantic Ocean. Taking the Lower Pliocene Reuverian flora first, we find that the percentage of woody plants is the same as in the Bembridge flora, 57 per cent.; the proportion of wide-ranging genera in the Reuverian flora is 32 per cent., slightly greater than that in the Bembridge flora. The relationship with north China and Japan is still more pronounced than in the Oligocene floras; the importance of the west European, western Asian, and Himalayan relationships has increased and the Indo-Malayan affinity has decreased. Ascending the series to the Teglian flora the following distinguishing features may be noted: the percentage of woody plants has fallen to 28; wide-ranging genera form 48 per cent. of the whole; the west Asian Himalayan and west European relationships almost equal those with Japan and north China; in the Teglian as in the Reuverian flora "the temperate alliance with America has quite outstripped the tropical," and now for the first time "the European alliance begins to overtop the American." At the still higher Cromerian stage there is a further fall in the percentage of woody plants; on the other hand the percentage of wide-ranging genera is approximately the same as in the Teglian flora; the dominant relationship "has definitely passed to western Europe." Another point which emerges from a comparison of earlier and later Tertiary floras is a marked increase in the proportion of wide-ranging genera from the London Clay to the Cromerian. Attention is drawn by Mrs. Reid and Miss Chandler to a curious anomaly, namely the greater proportion of northern genera in the Teglian and Cromerian floras than in the present British flora—they offer a geographical explanation based on the fact that in the latter part of the Tertiary era Britain was part of the continent of Europe and " would therefore have shared its flora more completely than it did later."

A comparative analysis of the Tertiary and Quaternary floras reveals many facts of considerable importance which throw light upon evolutionary changes in the successive phases of the development of the British flora. The gradual decrease in the percentage of woody plants during the long period separating the London Clay from the present day is in accordance with the view that the evolution of trees largely preceded that of herbaceous plants ; it is suggested by the authors, whose results are freely quoted in this article, that another cause may be climatic : tropical floras, among which they include the London Clay flora, are characterised by a preponderance of woody plants, and " in passing from the tropics to the cold regions the proportion of woody plants diminishes." We know practically nothing of the origin of herbaceous forms: neither the place nor the reason of their production. The Cretaceous floras of the arctic regions, so far as we know, did not differ from the Cretaceous and early Tertiary floras of more southern lands in having a smaller proportion of woody plants.

An outstanding feature of the London Clay flora is the almost complete absence of wide-ranging genera ; the representative living genera are restricted in their geographical range and most of them are Indo-Malayan ; there are very few links with the New World. Before the end of the Eocene period, at the stage represented by the Hordle flora, the dominance of Indo-Malayan elements had ceased, and a glance at the table (p. 16) shows that as we ascend from the older to the younger floras the relationships of genera have a wider geographical range ; there is a gradual increase in the number of wide-ranging genera from 1 per cent. in the London Clay to 46 per cent. in the Cromerian flora. A comparison of the Cromerian and present floras reveals an unexpected contrast ; the wide-ranging genera are only 26 per cent. in the living British flora. This is attributed in part to the destructive effects on the vegetation of the arctic climate in the early part of the Quaternary period shortly after the end of the Pliocene period, and to the severance of Britain from the Continent, which reduced the possibilities of immigration. The changes in composition of the successive floras from the London Clay upwards are the expression of a transformation of a flora with

Malayan relationships to floras which became gradually Europeanised; a change from tropical and subtropical to temperate floras. There is also an increasing alliance of western European and temperate North American floras as we pass upwards through the geological series. On the whole there is an increase in the proportion of northern Eurasian and American genera from the London Clay, with *Cornus* as the only representative, to the Cromerian.

We have so far noticed certain features characteristic of selected floras beginning with a Lower Eocene flora and passing upwards through several million years to the last stage of the Pliocene period, when the aspect of the plant-world in Britain was very much as it is now. Soon after the beginning of the Quaternary period the temperate climate became increasingly arctic, glaciers began to form on the hills and invaded the lower parts of valleys, neighbouring streams of ice coalesced to form ice-sheets, and eventually most of the country was clothed in white. Some of the hardier pre-glacial Pliocene plants survived in Britain, on ice-free mountain-peaks and in districts which there is good reason to believe were never overridden by glaciers or ice-sheets: we know that in Greenland at the present day plants are able to exist on rocky islands protruding from the ice-sheet. At a later stage the climate again became temperate. We are not now concerned with interglacial periods, long-continued intervals between successive glacial invasions: it is enough for present purposes to think of the glacial period as a single episode, a factor which had a devastating effect upon the flora of Britain. Eventually the pre-glacial conditions were restored, and the partially depleted land was ready to receive invaders from the Continent; these with the hardier elements which had lived through the Ice Age inaugurated the present flora. For a very clear account, by a recognised authority, of the story of the changing vegetation in Post-glacial Britain readers are referred to an article by Dr. T. W. Woodhead, "Post-glacial succession of Forests in Europe," published in SCIENCE PROGRESS, No. 102, October 1931.

Whatever our views may be on the mean temperature of southern Britain at different times within the section of geological history covered by this retrospect, it is generally admitted that the conditions were most genial when the London Clay was being deposited in the early Tertiary sea. In the later stages of the Eocene, in the Oligocene and Miocene periods the climate continued to be favourable, though the mean temperature was gradually falling and reached an arctic climax in the early part of the Quaternary period. It would seem that by the end of the Quaternary period (Cromerian) the conditions on the borders of the Rhine delta were very much the same as they are now in East Anglia. Then came the Ice Age, the incursion of an arctic flora from the Far North and the migration of pre-glacial species to more southern regions where the displaced plants were able to colonise new ground. Fluctuating climates, the wandering of arctic and temperate species to and fro as the glaciers grew, waned and vanished, the inability of some fugitives to cross physical barriers athwart their path, the survival of the hardier kinds ; these and other incidents in the long years separating the Ice Age from the present day are well known.

There is, however, one important consideration to which reference should be made : the London Clay flora flourished on the northern shores of the Tethys ocean on the borders of a sea in direct communication with the Tethys. By the time the Bembridge flora had become established the great trough of the Tethys was being upraised into gigantic folds and overfolds of the earth's crust ; the " alpine storm " was bringing into being the transverse mountain-chains of the Alps and Himalayas, the barriers which at a later date, when the incidence of arctic conditions in northern Europe compelled the Pliocene floras to seek more southern homes, closed the road to safety. The result was the destruction of numerous genera and species which had once been British and the reduction of the European plant population to a position much inferior to that of North America and the Far East, where access to refuges had been preserved, and there were no transverse obstacles to free migration. Different views are held on the effect of the Ice Age upon the Pre-glacial flora : the question is, to what extent the country was reduced to barrenness, was the plant-population almost completely destroyed, or is it possible that a fairly large number was able to retain possession of unglaciated areas. Botanists best qualified to express an opinion appear to think that the disastrous effect of an arctic Britain has been greatly exaggerated. As Professor Salisbury says—" In the reaction from the view that the whole of the flora of these islands was destroyed by the Pleistocene [Quaternary] glaciation we must avoid too facile an acceptance of the theory of persistence."

THE PROBLEM OF A MALAYAN FLORA IN EOCENE EUROPE

We have strayed far from the London Clay and its Malay genera. What, it may be asked in conclusion, is the explanation of the close relationship of the flora of the London Clay to that of Malaya ? Mrs. Reid and Miss Chandler express the opinion that " Dr. Brooks' hypothesis affords an admirable explanation of the dominance of the Indo-Malayan element." He correlates the hot Eocene climate with the maximum transgression of the Tethys sea when a warm current flowed towards Europe in the uninterrupted waterway from Malaya which stretched from east to west across the north of the Indian Peninsula, which was then an island, at a period antedating the upheaval of the Himalayan ranges. As Dr. Brooks finds in the Tethys sea the primary cause of the hot Eocene climate indicated by the fossil flora, so the botanists " find, in the same sea, the cause which decided that the flora should be predominantly of Indo-Malayan type ; whilst, at the same time, its waters barred the way for any great inflow of African genera, and the Atlantic presented an even more efficient barrier against the entry of American genera." The conclusion is that in the early days of the Eocene period the plants of the London Clay came to western Europe from the region where their modern representatives are now living: migration was from the south-east to the north-west. On the other hand, the later European floras, allied more closely to existing floras in China and North America, are spoken of as spreading in successive waves of migration from northern homes along divergent routes to the south. Before referring to the arguments advanced by Mrs. Reid and Miss Chandler in support of their view that the Malayan elements in the fossil flora were immigrants from Malaya, it is permissible to suggest the possibility that the present Malayan flora was derived from Europe, that the genera which we now call Malayan existed in the Eocene period in the London Clay area. Their home was then on the forest-clad margin of the London sea ; they had no connection with Malaya In course of time as the climate became unsuitable they wandered farther afield and after a long interval reached the refuge where their descendants play a prominent part in Malayan forests. Malaya is the home of many plants, ferns as well as seed-bearing plants, which in past ages were widely spread in the northern hemisphere. A Malayan forest might be described as a link with Cretaceous and Tertiary Europe, a forest —remote in space and in time from the vanished forests of England —where a botanist familiar with the floras of the past would feel that he had been transported to a scene reminiscent of a much older land, a land that became England.

Mrs. Reid and Miss Chandler believe that the Eocene floras preceding that of the London Clay indicate a warm temperate rather than a tropical climate. Before the London Clay period " the climate was warming," whereas after that period it was cooling. When the temperature in western Europe was rising to the maximum, represented by the flora of the London Clay, plants would tend to migrate northward ; on the other hand, when the temperature fell, as the contrasts between the older and younger Eocene floras suggest that it did, migration would be towards the south. The more tropical elements of the London Clay flora may represent the incoming flora, immigrants from the south ; the more montane and extra-tropical elements may in part at least represent an older Eocene flora. Genera of the latter class, the extra-tropical plants, lived through the tropical period and survived as members of later Tertiary floras. The tropical elements, with the exception of three genera, do not appear to have been able to withstand the cooler climate of the later Eocene period : they have not been found in floras older or younger than the London Clay. *Nipa* is one of the three exceptions ; it persisted as an element of a later Eocene flora. It was at or about the time of the London Clay that *Nipa* lived in several parts of Europe on the northern shore of the Tethys sea, and the inference is that localities where the Tertiary examples have been found mark the northernmost outposts of a tropical genus. This brings us to an important step in the argument : a fossil species of *Nipa* was described by Dr. Kräusel in 1923 from a locality in Borneo, within the extensive geographical area where the living species abounds in brackish estuaries. Mrs. Reid and Miss Chandler speak of the Bornean *Nipa* as an Eocene species : their view is that *Nipa* " must have travelled to Britain along the shores of the Tethys sea." But there is no definite evidence of the Eocene age of the Bornean species ; it may be Eocene as Dr. Kräusel says, but he admits that the precise age of the beds in which it was found has not been determined. The English authors add, " It is making no great assumption, therefore, to suggest that along with *Nipa* came the remainder of the lowland tropical Indo-Malayan-African element in the London Clay flora." Evidence is quoted from authorities on Tertiary invertebrates in support of a similar migration from Malaya to Europe. It has previously been stated that Mrs. Reid and Miss Chandler have made out a convincing case for a series of migrations in the course of the Tertiary period from the north to the latitude of southern England, and this was a natural consequence of the gradual lowering of temperature subsequent to the London Clay period. But in their view the London Clay plants, or at least many of them, must be regarded as immigrants from a remote region in the south-east. This necessarily implies the existence of a flora in the Indo-Malay region contemporary with the flora of the London Clay or—to allow time for migration—at a slightly earlier period, whence representative elements travelled to Europe. The fossil *Nipa* is one piece of evidence that is not by any means convincing. Mrs. Reid, in a letter written in answer to a request for some further light on the question of the original home of the Malayan element in the English delta, reminded me that Dr. Kryshtofovich stated in an article in the *New Phytologist* (1929) that in the south of Asia " the flora has developed unmolested ever since its first descent from its Cretaceous ancestors." " This is proved," wrote the Russian author, " by fossil documents found in Borneo, Sumatra, Java, the Philippines and other tropical countries." A statement such as this, if based on accurate knowledge and well established evidence, would be of great value ; but as Kryshtofovich says, the Tertiary flora of the countries named " is unfortunately still but little known." My point is this : Mrs. Reid and Miss Chandler, Dr. Brooks and other authors may be right in picturing currents in the warm Tethys sea carrying marine animals, and the slow march of terrestrial plants on its shores from Malaya to western Europe ; but we have no proof of the existence in Malaya of an Eocene or Pre-Eocene flora of the type necessitated by the botanical conclusions. We know that the Eocene flora of the London Clay is related to the living Indo-Malayan flora, the two separated in time by at least fifty million years. We know practically nothing of the geographical range of Indo-Malayan genera in the Eocene period ; they may have been established on the south-eastern shores of the Tethys sea, but assuming—and it is an assumption, not as yet a fact based on published evidence—that they were there, it is possible that the same genera may have been living in many other parts of the world where they left no traces. Extinct floras furnish many instances of far-flung genera inhabiting regions widely separated latitudinally, a distributional range which suggests that the plants were tolerant of different climatic conditions.

There would seem to be three possibilities : (i) the derivation in the Eocene period of the Malayan genera of the London Clay from Malaya, the view held by the authors of the fossil flora , (ii) the genera of the London Clay may be regarded as members of a widespread flora which in the course of the Tertiary period suffered progressive reduction and is now represented by enfeebled relicts in Indo-Malaya ; (iii) that the London Clay flora, like the later floras, did not reach Europe from a south-eastern home but from the north.

It is not possible in the space at my disposal to do more than mention these possibilities. The value attached to fossil plants as trustworthy criteria of climate is one of the most important considerations, and it is no doubt true to say that most students of palæobotany would agree with Mrs. Reid and Miss Chandler on this point. Their conclusions to a large extent depend upon the truth of the assumption that plants do not alter their constitution in the course of ages; that the present relation between genera and species and climate is a safe guide to the relation of closely allied plants to climate in the past. Disinclination wholly to accept the conclusions still remains despite the arguments advanced by the two authors, largely because of the demand for a tropical climate in the region occupied by the London Clay. My view is that we are almost compelled to have recourse to some form of continental drift, or to admit the possibility that plants are not as conservative and as fixed in their reactions to climate as is generally supposed in order to account for the facts of phytogeography revealed by fossils.

After reading the volume, which inspired this article, I am at least convinced that Mrs. Reid and Miss Chandler have earned the gratitude and admiration of all students of ancient floras: their work is not merely important as a contribution to knowledge; it is a notable demonstration of the possibilities of intensive study of a fossil flora by botanists equipped with the necessary knowledge of living plants.

THE RIVERS OF WALES AND THEIR CONNECTION WITH THE THAMES

BY PHILIP LAKE, M A.

THE aim of a river is to reach the sea and attain nirvana in its waters. Our rivers in the end succeed, but for some the journey seems needlessly long and troubled. One of the head-streams of the Yorkshire Derwent rises a little more than a mile from the North Sea. It flows away from the sea to join the Derwent and at the junction the Derwent itself assumes a westerly course. About sixteen miles from the coast it seems to realise that it is going in the wrong direction and turns to the south, cuts through a ridge which is higher than the source of this head-stream and at last enters the Ouse. The water that rose about a mile from the sea must travel more than seventy miles to reach the head of the Humber estuary. There are other rivers in England and Wales that seem equally reluctant to lose their individuality in the ocean.

Between 1860 and 1875 English geologists devoted a considerable amount of attention to the development of our river-systems. To that period belong the papers by Jukes [1] on the rivers of Cork and Waterford, and by Foster and Topley on those of the Weald, and the more general paper by Ramsay on the river-courses of England and Wales; but this activity ceased with the publication, in 1876, of another paper by Ramsay on the history of the Welsh Dee The subject fell into disrepute at the Geological Society and for the rest of the century was practically ignored in its Journal.

It was during this period of neglect in England that the laws of river-development were worked out in America and a new science arose. G. K. Gilbert laid the foundations, and the superstructure was built by W. M. Davis. It is not surprising that in this matter we owe so much to America. In our islands the physical features are seen through the veil of a hazy atmosphere and through a carpet of grass and other vegetation; and, to add to our difficulties, the Glacial period introduced so many complications that some of our river-systems seem to have obeyed no laws at all. Many English geologists were unwilling to accept the teachings of a region which, in these respects, is more favoured than our own; but, in spite of this, it was a paper by Davis on English rivers that revived interest in the subject.

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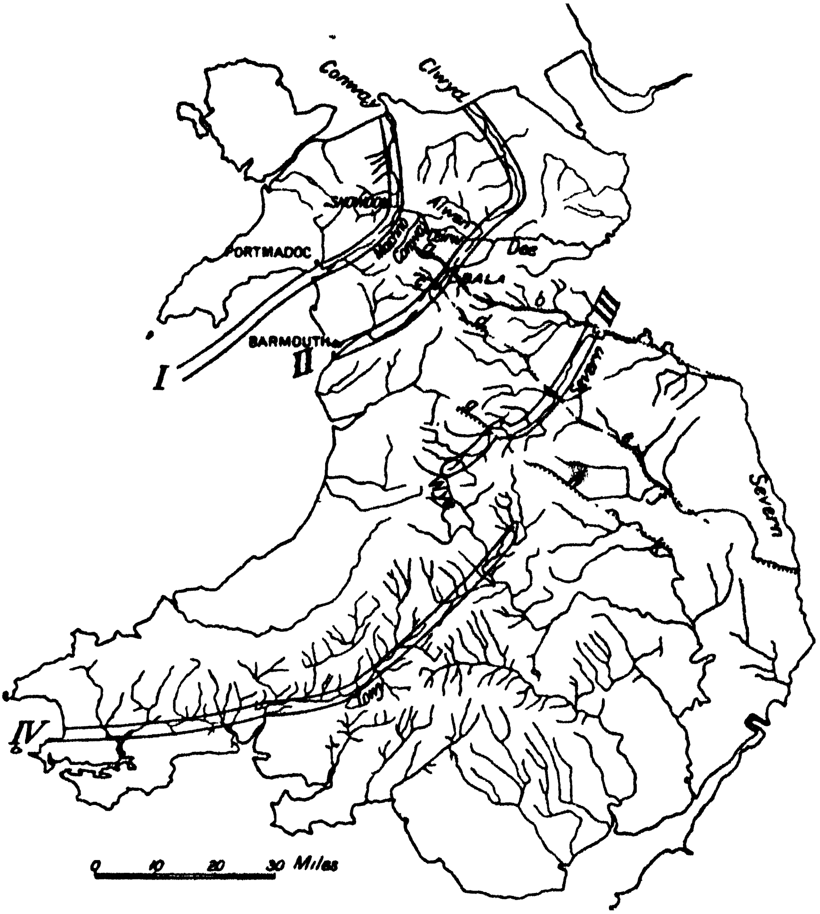
[1] The references are given at the end of the paper.



A glance at the accompanying map will show that in Wales there are many rivers flowing to the south-east or south-south-east but there are few that keep to this direction for more than ten or fifteen miles. Most of them turn abruptly to the north-east or to the south-west, some of them resuming their original direction farther on. To appreciate the significance of these sharp bends the reader should bear in mind that except near their sources the valleys of these Welsh rivers are cut deeply into solid rock. In an alluvial plain, where the rivers are only a few feet below the general level, a very small obstacle may divert a stream and a single flood may cut a new channel. But to change the direction of a river that is flowing along the bottom of a deep valley is a very different matter. How this happened is one of the problems set before us.

The island of Anglesey and the peninsulas north and south of Cardigan Bay are comparatively low-lying, though with isolated hills rising to a much greater height. Excluding these three areas the main mountain mass of Wales is roughly a rectangle with its longer sides running from north to south and its greatest height in the north-west corner, where all the peaks above 3,000 feet are grouped together. But the mass is not continuous. It is broken up by a series of valleys or depressions which cross it obliquely. Their general trend is from south-west to north-east, with a northward curvature towards their eastern end. Approximately they form arcs of ellipses, the longer axis of which runs from south-west to north-east through Snowdon. Some of these depressions are continuous throughout the mass, others cut deeply into it but die out in the highest ground ; many are short, too short to show any curvature.

Four of these depressions have so profound an influence upon the drainage of the area that they require special notice, and for convenience may be called the major depressions. On the map they are lettered *I–IV*, and in order to direct attention to them without obscuring other detail they are distinguished by heavy lines drawn upon each side of the streams that flow along them. It should, however, be remarked that these heavy lines have no other purpose and they do not mark the lateral boundaries of the depressions. The shorter depressions with the same general trend are sufficiently indicated by the direction of the streams that occupy them.

The first of these major depressions runs from the Portmadoc estuary, at the north-east corner of Cardigan Bay, past Ffestiniog and by the river Machno into the Conway valley. For more than two-thirds of its course its floor is less than a hundred feet above the sea, but across the high ridge between Ffestiniog and the Machno it reaches an altitude of about 1,550 feet. Even here, however, the depression is well defined, for north-west of it the ridge rises to more than 2,000 feet and south-east of it to more than 1,900 feet. At the Portmadoc end the depression continues south-westward beneath the sea for a distance of nearly thirty miles, and its floor reaches a depth of more than twenty fathoms.

The second depression, perhaps the most striking of all, runs from the Barmouth estuary to Bala Lake. Beyond the lake its floor rises and the depression shallows, but it still continues as a definite valley in the same direction to the head of the vale of Clwyd. The highest point of the depression lies about three-and-a-half miles north-east of Bala, where its floor is about 900 feet above the sea.

The third depression is the valley of the Upper Severn and does not cross the whole width of the mountain mass. It enters on the eastern side from the lowlands of Shropshire and passes south-westward through Welshpool and Newtown to Llanidloes, losing itself on the eastern slopes of Plynlimmon. Comparison with the second depression suggests that perhaps the low land of western Cheshire and the estuary of the Dee may constitute a northerly prolongation of the third.

The fourth depression is also incomplete. It enters from the west at St. Bride's Bay, passes through Haverfordwest and Carmarthen and along the valley of the Towy to Llandovery. Beyond Llandovery it continues in the same direction up the valley of the Bran, crosses the watershed by the same gap as the road A.481 and opens out into the broad basin of Builth. From there it proceeds, still preserving the same direction, up the valley of the Ithon to Llanbadarn Fawr.

The first of these four depressions separates the north-west corner of the mountain mass from the rest of the rectangle; the other three define the outer limits of three broad and roughly concentric strips. The first depression receives most of the drainage from the eastern and southern flanks of the Snowdonian massif but little enters it from its other side. Similarly each of the other depressions receives most of the drainage from the strip upon its inner side and little from that upon its outer side. The watershed of each strip is very near its inner edge and the strip as a whole slopes to its outer border. To prevent the possibility of misunderstanding here or elsewhere it should be noted that the terms inner and outer are used throughout with reference to Snowdon as a centre. The inner side is the side nearest to Snowdon.

As a broad generalisation which corresponds closely with these facts we may imagine the mountain mass of Wales to be the south-eastern part of an elliptical dome. The longer axis of the dome runs approximately from S.W. to N.E. through Snowdon, and its centre lies somewhere in the Irish Sea south-west of the Lleyn peninsula. We are not yet concerned with the question whether any such dome ever existed; the conception is introduced here simply to facilitate the description of the general drainage of the actual mountain mass.

The streams which originate upon such a dome will not radiate from a point but from a line, or from an elongated ellipse of which we may take Snowdon as the north-eastern end. But the dome is trenched by the four major depressions. Each strip into which it is thus divided is highest on its inner edge and slopes down towards its outer edge. Most of the rain that falls upon a strip will find its way into the trench along its outer edge and will be diverted along the trench to the N.E. or S.W.; only a small proportion will descend the steep inner edge into the inner trench. This is the general scheme of the actual drainage system, but here and there it is modified in various ways.

Where a gully is worn in a steep slope any water that flows over the neighbouring surface during heavy rain will converge towards it, and the head of the gully will gradually be cut farther back into the slope. In time the gully may be cut through the watershed and capture the head of a stream that was flowing down the other slope. This, no doubt, is the explanation of the remarkable course of the upper Conway. For six or seven miles it flows towards our second depression and then turns abruptly and descends a steep and narrow valley into the first. Wherever the slope on one side of a watershed is much steeper than on the other, changes of this kind are so common that they may be considered as normal developments of the general scheme and call for no further remark here.

But there are other departures from that scheme that are of more importance and interest. Bala Lake lies in the second depression, in an elongated basin at the head of the deep valley that runs up from Barmouth. This valley seems to be the natural outlet of the basin and the floor of the lake slopes down towards it; but the water of the lake flows from the other end and escapes through a narrow rocky gorge cut through the ridge enclosing the basin. Here, accordingly, a considerable part of the drainage from the strip inside the second depression passes into the strip beyond it. This was a change brought about during the Glacial period. The original outlet towards Barmouth was blocked by ice, and is still blocked by glacial debris. The Bala basin filled with water to the level of the lowest ice-free col across the surrounding hills, and the overflow began to cut the gorge through which the water now escapes. As the gorge was deepened the level of the water-surface fell until it reached its present height. Near Rhiwlas, north of the town of Bala, there are well-marked terraces formed at the edge of the lake when it was much larger than it is to-day.

There are a few other places where a part of the drainage from a strip on the inner side of a major depression crosses to the strip beyond. North-east of Bala the Ceirw and the Alwen drain eighty or ninety square miles of the strip on the inner side of the second depression. The water of the Ceirw is quite unaffected by the depression. Without any change of direction it flows across it and joins the Dee at Corwen; and the Dee itself, from Corwen to the plains, follows the same general direction. The Alwen is deflected along the depression for rather less than two miles and then joins the Ceirw. Since the Alwen is the larger of the two it gives its name to the united stream, but it is the Ceirw that maintains its general direction unchanged as far as the Welsh border.

Farther south the Wye rises in Plynlimmon and flows to the south-south-east, crossing the fourth depression near its head.

Finally, all the drainage that enters the fourth depression from the head of the Towy nearly to St Bride's Bay breaks out from it southwards into the sea. But where it escapes the strip on the outer side of the depression is very narrow and is deeply indented by the sea.

With these exceptions no water finds its way from the strip on the inner side of any of the four major depressions to the strip on its outer side. There are, however, many shorter valleys with the same trend as the major depressions. Some streams cross these without any change of course, others are diverted along them; some of the latter break out again and resume their former direction.

Returning to the general scheme we exclude from it the slope from the watershed to Cardigan Bay, and for the rest we still retain the conception of a dome in order to define direction, using the term radial for directions down the dome and transverse for directions along its flanks. In the northern part of Wales the strictly radial direction is nearly due east, in the central part it is south-east or south-south-east; everywhere the transverse direction is at right angles to the radial direction at the same point.

With this terminology the river valleys fall naturally into two groups, *viz.* those that are more or less radial in direction and those that are transverse The transverse valleys are usually free from irregular kinks and bends, and they receive many tributaries from their inner side but few from their outer side. The radial valleys are less steady in direction and they receive tributaries equally from both sides. It should be noted, however, that where a transverse valley is wide the river within it may meander over a broad flat floor; and the third major depression is exceptional, for the valley itself shows a great bend above Newtown.

Many short streams are radial throughout their course, ending in the sea or in a transverse valley. A few longer rivers retain the radial direction for twenty or thirty miles. The Ceirw, with its natural continuation, the Dee below Corwen, is radial for about twenty-five miles; the Wye is a radial river for nearly forty miles from its source. But by far the greater number of streams that start in a radial direction are sooner or later deflected into a transverse valley. They may follow it to the border of the mountain mass, or they may escape and resume a radial course. The Wye itself, although it crosses the fourth major depression without deflection, turns into a transverse valley about fourteen miles lower down and does not leave it till it reaches the Welsh border.

In the normal evolution of a river system upon a uniform slope composed of uniform material a number of rivers are developed running down the slope, and these rivers receive tributaries equally from each side; both rivers and tributaries erode valleys. The radial valleys of Wales are erosion valleys cut by the streams that occupy them. In direction they agree with the streams that would be developed on such a dome as we have imagined, and in general they are quite independent of the geological structure of the ancient rocks in which they are cut. The transverse valleys do not follow the general slope of the country through which they pass, but their directions are closely related to its geological structure. They lie along faults, folds or other lines of disturbance.

According to the sense in which the terms are used here the radial and transverse directions are everywhere at right angles to each other, and it may be thought that if the transverse valleys are related to the geological structure the radial valleys must be related also. This is true where transverse valleys exist; but there are large areas where the geological trend-lines are not in the direction that we have defined as transverse. Here there are no transverse valleys, but radial valleys are as numerous and well-defined as elsewhere. The natural conclusion is that in their origin the radial valleys are unaffected by the geological structure, but that sometimes the geological trend-lines happen to be in the transverse direction. It is only where this is the case that transverse valleys are developed.

It seems clear, therefore, that the radial valleys differ from the transverse valleys in their origin, and it is probable accordingly that they differ also in their age. The transverse valleys divide the mountain mass more or less completely into a number of strips. In each strip the watershed is near its inner border and the general slope is towards its outer edge. If the transverse valleys were formed first the drainage of each strip would originate independently and there would be no reason why the radial streams in one strip should have any relation to those in any other. If the radial valleys were formed first the transverse valleys must have cut across an already developed radial system, and the radial valleys of one strip would be continued into the next. The drainage of each strip into which the area is now divided will naturally be concentrated into the valleys which already exist; and consequently, at the present day, the radial streams of neighbouring strips will be in line with one another.

Even the small map here provided is enough to show that the drainage of each strip is not independent of its neighbours. The radial streams of one strip often fall into line with those of the next, as if they were originally continuous across the intervening transverse valley.

It will now be necessary for the reader to look more closely at this map. There is a danger that the eye, lost in a multiplicity of lines, may not perceive the essential simplicity of the general scheme; and therefore towards the borders of the map only the more important streams are shown. This is the case especially on the eastern and southern sides, and also on the slope towards Cardigan Bay, which does not fall within the scheme. Over the rest of the map most of the streams that it is practicable to show on so small a scale are inserted; but in order to keep the map as clear as possible few names are given, and most of the streams to which reference is made hereafter are indicated by letters only.

For a more complete representation of the whole river-system the reader may refer to the Physical Map of England and Wales on the scale of 1/1,000,000 issued by the Ordnance Survey, in which the general relief and the rivers are beautifully shown. The smaller features which I have occasionally to mention, may be seen on the North Wales and South Wales sheets (4*a* and 7*a*) of the quarter-inch map, in which the relief is shown in greater detail.

Upon my own map the probable courses of the three radial rivers that I now proceed to trace, are indicated by dotted lines running alongside existing streams and over existing cols.

The Tryweryn (*a*) enters the second major depression near the town of Bala, and is there deflected. If, however, we continue in the original direction of the river across the depression we pass up the Hirnant valley and across a col into the headwaters of the Tanat (*b*), a radial stream like the Tryweryn itself. The Tanat retains its direction unchanged till it joins the Severn, and the part of the Severn below the junction has the direction of the Tanat as far as the Shropshire lowland.

The Llafar (*c*) enters the same depression from the north-west and falls into Bala Lake. But on the other side of the depression the valley of the Vyrnwy (*d*) runs in the same direction as the Llafar, and in line with it, to a point about a mile below Lake Vyrnwy. Here the river is deflected towards the south-west along a transverse valley, but about two miles farther on it escapes and resumes a radial course. Lower down it is again deflected, but this time towards the north-east, and when it comes into line with its original course it again becomes radial. Farther on it enters a more important transverse valley and again turns to the north-east, retaining this direction till it joins the Tanat.

But upon the other side of this transverse valley a radial valley in line with that occupied by the Upper Vyrnwy cuts across the high land between the Vyrnwy and the Severn, and on the other side of the Severn another valley, still in the same line, leads across a col into the Onny (*e*), which runs in the same direction and in the same line as the Upper Vyrnwy. The floors of the valleys uniting the Upper Vyrnwy with the Onny are nowhere so much as 600 feet above the sea, though the hills on both sides rise to more than 1,000 feet. It seems probable, therefore, that the valleys now occupied by the Llafar, the Upper Vyrnwy and the Onny are sections of a single radial valley which was once continuous but has since been broken up by the transverse valleys that now cross it. It is easy to trace this radial valley still farther. The Onny joins the Teme (*f*), and the united stream, though called the Teme, is the direct continuation of the Onny. After about eight miles it swerves aside in a wide curve but returns to the same direction and the same line before it reaches the Lower Severn.

The Carno (*g*) enters the third major depression from the north-west. On the other side of that depression and in line with the Carno is the Upper Teme (*h*), which preserves the same direction as far as Knighton. Here it is deflected eastwards to join the Onny, but the original line is carried on by a tributary of the Lugg (*k*) and the lower part of the Lugg itself.

Many other examples of a similar kind might be given They are, indeed, so numerous that it is impossible to attribute them to accidental coincidence; and there is nothing in the geological structure to account for them. It seems certain that in North and central Wales a radial system of rivers had been established before the formation of the transverse valleys. Probably it arose upon a cover of later beds concealing the old rocks of the region. This is the explanation that has been suggested by several writers for the more complete radial system of the Lake District.

In South Wales the development of the river-system has been discussed by Strahan, Trueman and R. O. Jones. There is also a very interesting paper by O. T. Jones upon the Upper Towy drainage system, but he is concerned with its later history rather than with its initiation. Strahan, Trueman and R. O. Jones all recognise the essential difference between the radial and the transverse valleys, although they do not use these terms. The transverse valleys follow lines of disturbance in the old rocks of the area, while the radial valleys pay no regard to geological structure. Strahan supposed that the river-system of South Wales was initiated upon a slope of Upper Cretaceous beds by which all the features of the Palæozoic rocks were blanketed, and the rivers still kept their original courses even when the Cretaceous covering was entirely worn away. He did not realise that there was any difference in age between the radial and the transverse valleys, and attributed the latter to uneven upheaval along the lines of disturbance which already affected the older rocks. Trueman and Jones allow the possibility of a Cretaceous covering, but they recognise that the transverse valleys are of later date than the others, and Jones finds evidence that they were not formed till after the Cretaceous beds had been removed. Trueman thought that the transverse valleys were eroded along the belts of disturbance because they were belts of weakness. Jones shows that, in one case at least, there has been elevation on the outer side since the radial valleys were in existence.

During the later part of the Cretaceous period, while the Chalk was being deposited, the English lowland was beneath the sea, and the sea must also have spread far up the slopes of the highland areas. It was once supposed that the Chalk was a deep-sea formation, like the Globigerina ooze of the present day, but it is now clear that the analogy is fallacious. More recent estimates place the depth of the Chalk sea at about 200 fathoms (1,200 feet), for some of the beds and at still less for others. Even with this more moderate estimate it would require an elevation of 1,500–2,000 feet to bring the top of the Chiltern Hills, which are made of chalk, to their present altitude of over 700 feet. Moreover, the Chalk rises towards the west, and in that direction the elevation must have been still greater. The westward rise is enough to carry the base of the Chalk over the top of Snowdon. It seems almost certain, therefore, that the deposits of the Chalk sea must have covered a considerable part, if not the whole, of the Welsh mountain mass. No direct evidence, however, is left; the deposits were too soft to resist the glaciers of the Ice Age.

If there was once a covering of Cretaceous beds over the whole, or the greater part, of Wales, the formation of the radial valleys is easily explained. The whole area was elevated into a dome such as we have imagined, and the rivers were initiated upon its surface. Their valleys were already deep before they reached the older rocks beneath, and therefore they kept to the same courses when they cut into those rocks, even after the original cover had been entirely worn away. Their directions are consequently independent of the geological structure of the rocks over which they now flow.

It is probable that before the Upper Cretaceous beds were deposited, long-continued subærial denudation had worn down the old rocks of Wales to a gently sloping plain, on which, perhaps, rose rounded hills formed of the most resistant rocks. Even if the sea did not spread over it, this plain might be raised into a dome, and radial rivers would begin upon it. If, however, there were no cover of later beds, the complicated geological structure of the old rocks would make its influence felt as soon as the rivers began to flow. There would be greater erosion along the softer beds and along the weaker belts, and the system would quickly become irregular. The radial streams that we have traced ignore so completely the structure of the rocks over which they now flow that their courses must have been determined before they reached those rocks. They must, in short, have originated upon a cover of more uniform composition.

It was not till the radial rivers had cut their valleys down to the older rocks that the transverse valleys were developed, and there are two different causes that may have led to their formation. The lines of disturbance along which they lie are belts of weakness which would be easily eroded. As soon as these weaker belts were exposed, by the removal of the overlying cover, streams would begin to develop along them, running either into the sea or into some radial valley. The valleys of these streams would deepen and lengthen until the whole belt was hollowed out and became a continuous depression. The waters of the radial streams running into the depression would be deflected along it to the sea or into a stronger radial river which for the time had kept its course. It is possible, for instance, that the second major depression has gradually been gouged out from both ends, but the excavation has not yet proceeded far enough to deflect the Ceirw, which therefore continues to flow across the weak belt in its original direction.

But the disturbances along this depression, along the fourth major depression, and along some of the shorter transverse valleys seem to have had a more direct influence than this. The Bala and Clwyd faults, which lie along the second depression, both have their upthrow upon the outer side. After their formation the strip upon the outer side of the present line of depression must have stood far above the level of the strip upon its inner side. It still stands higher; the hills outside the depression, Cader Idris, Aran Mawddy, the Berwyns and the Clwydian range, are considerably higher than those that face them on the inner side. The main movements took place long ago and it is perhaps unlikely that the resulting difference of level has survived all subsequent vicissitudes; but the faults are still lines of weakness, and more recent movement along them seems to have helped in the deflection of the radial rivers. In a dome built by man the joints between the stones or bricks are all planes of weakness, whether they run radially or transversely. If the abutments yield so that the dome begins to collapse, it is chiefly the transverse joints that give way. There is a tendency for roughly concentric fractures to form, and in general the strip on the inner side of a fracture sinks farther than the strip on its outer side. But often a transverse fracture will curve inwards till it meets another; and there may be some strips that lie lower than the strips on each side of them.

We can now understand the history of the Welsh river-system. While the Chalk was being deposited in England the greater part, if not the whole, of Wales lay beneath the sea. In Tertiary times there was a general upheaval, increasing in amount towards an axis which passed from N.E. to S.W. through Snowdon. An elliptical dome was raised which stretched across St. George's Channel into Ireland, and upon this arose a radial system of rivers. The dome collapsed and much of it sank beneath the sea, but the south-eastern part still stands and forms the mountain mass of Wales. Even here, however, the effects of the collapse are conspicuous. Old transverse fractures along the sides of the dome re-opened and the dome subsided in roughly concentric strips, and in general each strip settled down farther than the strip outside it. Partly in this way and partly by erosion along the fractures the transverse valleys were formed; the radial rivers were deflected into them, and the present complex drainage system was developed. There were irregularities such as might be expected upon a fallen dome; but space does not allow us to deal with them or with the part that now forms the slope towards Cardigan Bay.

A new question now arises. What happened to the radial rivers when they reached the English lowland? Did they continue in the same direction or were they intercepted, as they are at present, by the Lower Dee and the Lower Severn? In the former case some of them must have joined the Thames.

Several of the streams which unite to form the Thames rise in the Cotteswold range. At the head of the valley of each there is a gap in the ridge which was evidently cut by running water but which is now dry. The explanation is simple. The Oolitic beds that form the range are now cut off abruptly along its western scarp and at one time must have spread far over the Trias of the Severn plain. Upon this extended surface the rivers took their rise, and their present valleys, together with the gaps at their heads, are the truncated remains of the valleys that were then begun.

Generalised section from Snowdon to the Kennet near Reading.
Length of section about 210 miles.

A Palæozoic rocks
B Trias and Lias of the Severn Valley
C Lower Oolites of the Cotteswold Hills.
D Chalk
I, II, III Major depressions

So much will readily be allowed by most observers. It requires only a moderate extension of the same idea to carry the original sources of the Cotteswold tributaries of the Thames to the hills on the west side of the Severn As long ago as 1882, in a paper that I have not been able to see, T. S Ellis of Gloucester made a suggestion of this kind. He thought that before the Vale of Gloucester had been hollowed out the Severn did not turn at Tewkesbury as it does now but went on across the line of the present Cotteswolds into the valley of the Coln The Leadon, which now enters the Severn at Gloucester, continued without change of direction and crossed the Cotteswold line about near Birdlip.

A similar suggestion was made by Davis in 1895 and was more fully worked out by Buckman, who draws attention to Ellis's paper. Buckman himself goes farther than Ellis and boldly traces the Evenlode, Windrush, Coln and other tributaries of the Thames back to distant sources in Wales. In some of its details his map is highly imaginative but it shows very clearly that some of the Welsh rivers and these tributaries of the Thames arose on the same general slope.

There is nothing improbable in this. The floor of the Vale of Gloucester consists of Trias and Lias, softer material than the old rock of the hills on the western side and softer also than the later beds that rest upon the Lias on the eastern side. Every geologist recognises that these later beds once spread over the valley to the hills on the other side, and indeed there is still a remnant left at Bredon Hill. It was not till after the Tertiary elevation that the valley was excavated through this overlying cover, and it was this elevation that gave to the beds their present south-easterly dip.

We may, in fact, extend our dome over the Midlands as far as the Thames; but not beyond, for south of the Thames the axes of uplift run from west to east.

On this view the Cotteswold tributaries of the Thames fall into place as radial streams and the Lower Severn is a transverse river of later date. The Trias was a land formation and probably never extended into the highland much beyond its present limits. The succeeding beds spread farther and the Chalk may have covered the whole mass. When the dome was raised above the sea its flanks were gradually worn away and in time the Trias and Lias resting upon the hard old rocks were uncovered. In this soft belt a stream began to run into the Bristol Channel and erode its bed. It was by the gradual extension of this stream backwards along the soft belt that the valley of the Lower Severn was formed. The radial rivers from Wales were deflected into it and their former continuations remain as tributaries of the Thames. It would lead us too far to deal with the origin of the Bristol Channel or of the Thames itself, for here we come within the influence of the southern uplifts.

Buckman himself does not postulate a dome, but in effect starts with its worn south-eastern flank. He assumes an approximately plane surface reaching from North Wales to the Thames and sloping towards the south-east. Its altitude in the Malvern area was about 1,300 feet and in the Cotteswolds about 700 feet. Upon this surface the rivers flowed towards the south-east until the Severn developed in the manner described and captured their upper portions.

Ramsay and Strahan both look to special earth-movements to determine the course of the Lower Severn. Both imagine an Upper Cretaceous cover extending far into Wales. Ramsay thinks that when Britain rose above the sea in Tertiary times the Cretaceous cover was given a tilt towards the western mountains and the Severn arose in the hollow thus formed along the foot of the mountain mass. It was a later movement that reversed the tilt and gave to the beds the south-easterly dip that we actually see. The weak point in this explanation is that there is no evidence that the Cretaceous beds ever had a westerly dip.

Strahan supposes that somewhere west of the present outcrop of the Chalk there was an anticlinal axis, and from this axis the Cretaceous cover sloped towards the west. The Severn originated in the syncline between this axis and the hills. Even though the Chalk has been removed from the anticline that he imagines, there should be evidence of it in the beds beneath. No such evidence is to be seen, and the explanation is quite as conjectural as Buckman's.

Ramsay's paper was published long before the American studies of river-development. Strahan wrote while there was still a prejudice against them. It was not to be expected that happenings in the Wild West should provide a clue to the past of a well-regulated country like England. Time has brought a change of outlook.

NOTE ON THE SECTION

Owing to its small scale the section is necessarily much generalised and no attempt has been made to show the bedding or folding of the Palæozoic rocks. The general shape of the dome is indicated by continuing the base-line of the Chalk over the top of Snowdon. But it must be remembered that the dome has since collapsed. Before the collapse the mountain mass of Wales must have stood higher than it does now ; perhaps a part of it may have risen above this line and may not have been covered even by the Chalk sea. The radial rivers arose upon the Cretaceous surface before the collapse of the dome, the transverse valleys I–III after the Cretaceous cover had been removed and during the collapse. The valley of the Lower Severn was excavated in the soft belt of Trias and Lias when the beds overlying it had been worn away.

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MODERN SEISMOLOGY

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Introduction

As the main branches of science expand and overlap each other new subdivisions become recognised. Geophysics is a conspicuous instance of such a subdivision and it is one in which there has been a considerable development of interest in recent years. In view of this development the National Research Council at Washington has thought it desirable to prepare a series of Bulletins on the Physics of the Earth with the object of giving scientists who have no special knowledge of the subject some idea of the present position of geophysics and its main problems. Several Bulletins of this series have already appeared, the most recent being one on Seismology [1] The authors of this volume, *viz* J. B. Macelwane, H. O. Wood, H. F. Reid, J. A. Anderson and P Byerly, are all well-known seismologists, and their symposium forms a clear and comprehensive survey, mainly from the physical standpoint, of modern ideas on the subject. In the present article it is not intended to give more than a brief outline of these ideas; those readers who feel encouraged to enter more deeply into the subject will find the Research Council's National Bulletin a useful guide to further study of the physics of earthquakes.

The Growth of Modern Seismology

Before the latter part of the last century seismology did not offer a very attractive field to scientific men, mainly because the data then available were crude and unreliable. That the subject has now taken its place as a quantitative physical science is due, in no small measure, to the pioneer work of John Milne, whose interest in the subject was roused by his experience of earthquakes during the period 1876 to 1895 when he was Professor of Mining and Mineralogy in Japan. The subject was also taken up by workers in Germany and Italy about the same time, and very soon various types of apparatus were devised to record earthquakes at great distances. On his return to England in 1895 Milne set up his own earthquake observatory at Shide in the Isle of Wight, and in 1897 he inaugurated a scheme whereby recording-instruments were installed at a number of stations distributed all over the earth. The observatory at Shide became the central office of a world-wide seismic survey, and the comparative data thus obtained soon laid bare the main facts regarding the propagation of earth tremors through and round the world. A great advance was made at the beginning of the present century when Wiechert and his pupils at Göttingen took up the question of the interpretation of seismograms both from the observational and the theoretical side, and showed how the results lead to a knowledge of the physical properties of the earth. When Milne died in 1913 the organisation which he had built up passed into the hands of the late Professor H. H. Turner, who had shared Milne's enthusiasm in the work for many years. In addition to his other duties Turner found time for much research in seismology, notably on earthquake periodicities and deep earthquakes, and his interest in the subject lasted right up to the last conscious minutes of his life, for the collapse which led to his death occurred whilst he was in the presidential chair of the International Seismological Association at the Stockholm Congress in August 1930.

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[1] *Bulletin of the National Research Council*, No. 90. *Physics of the Earth, 6: Seismology*. Pp. viii + 233 (Washington, D.C.: National Academy of Sciences, 1933). Paper, 2 dollars; cloth, 2.50 dollars.

Progress in seismology is dependent to a very large extent on international co-operation, and for this reason alone the subject is deserving of all the support we can give it. Since the war the number of observing stations in the world-wide network has increased nearly threefold ; at the present time there are nearly 400. Practically every country in the world takes a share in the observations. The work, started by Milne and continued by Turner, of collecting the data and issuing a summary of the results is still undertaken in this country, at the University Observatory, Oxford, under the auspices of the British Association. In addition to the routine work of recording and measuring earthquakes a large amount of seismological research is now being done in many countries, and this is leading to an increased knowledge of the properties of the earth.

How Earthquakes Occur

The earth's crust has by no means reached a stable state ; it is continually undergoing deformation under the influence of stresses which develop within it. The deformation frequently takes the form of a sudden fracture in which the energy stored up by the strain is converted into the energy of internal motion. At the surface of fracture a displacement occurs which sets up vibrations that spread throughout the earth. The majority of severe earthquakes are produced in this way; such earthquakes are said to be tectonic in origin to distinguish them from shocks caused by stresses arising from volcanic activity. The class of tectonic earthquakes is by far the larger and more important one; most of the shocks that occur in volcanic regions are small in total energy. Sometimes, however, shocks of greater violence appear to be associated with volcanic eruptions, but it is doubtful whether these earthquakes are truly volcanic in origin.

Whatever its origin an earthquake sets up a disturbance of the ground which is communicated to other parts of the earth in the form of waves. As the waves spread out the motion of the ground at places distant from the origin of the shock is in general very small. With suitable recording apparatus, however, it can be detected even at the opposite end of the earth. In general, any earth movement may be considered as made up of three linear displacements along directions at right angles to each other. A fully equipped observing station therefore requires three seismographs, and the components of the displacement which these are set to record are usually the north-south, the east-west and the vertical. Many stations, however, only record the horizontal components since vertical seismographs are generally more troublesome to operate.

THE SEISMOGRAPH

The main principle of the seismograph may be explained briefly as follows. A body attached rigidly to the ground will undergo the same displacements as the ground itself. If, however, the attachment is not rigid there will be some relative motion between the body and the ground, and it is this relative motion which enables us to determine how the ground itself is moving. Thus the essential part of a seismograph is like a pendulum, the bob of which tends to remain stationary in space while everything to which it is attached is moving. Actually the bob does not remain stationary because it is impossible to eliminate entirely the effect of the attachment, but in certain circumstances it is possible to make allowance for this and so obtain the true motion of the ground. The modern development of seismographs has been in the direction of providing increased magnification of the relative motion and of ensuring adequate damping of the moving system. The latter is necessary since any undamped oscillatory system has a period of its own irrespective of the period of the vibrations which are forced upon it, and in such a case the motion recorded bears no definite relation to the true earth motion. For magnifying the movement of the pendulum mechanical lever arrangements are tending to be displaced by optical or electromagnetic methods of recording which have the advantage of eliminating friction. In some of the older types of seismographs the weight of the main mass of the pendulum often ran into hundredweights or even tons ; it is now realised that such heavy systems are not necessary, and the tendency now is to go to the other extreme. The moving mass of the Wood-Anderson seismograph, for instance, weighs no more than a few grams, and whereas most other horizontal instruments use gravity as the restoring force this instrument employs the torsion of a stretched fibre. In vertical seismographs a coiled spring is generally used to control the motion of the pendulum, and the chief difficulty associated with these instruments is the effect of temperature on the elastic constants of the spring. This has been overcome to some extent by the use of elinvar alloy, but there is still plenty of room for improvement in the design of vertical seismographs. A point of very great importance in the recording of earthquakes is the accuracy of the timing of the records. There is no difficulty in getting sufficiently accurate time-marks from a control clock which can be checked by wireless time-signals, but unfortunately some seismograms suffer from the lack of uniformity of rotation of the recording drums.

EARTHQUAKE WAVES

The record of a strong earthquake obtained on a modern seismograph at fairly large distances from the epicentre[1] usually presents a characteristic appearance. Three distinct portions can be recognised ; the first has a sharp commencement followed by rapid but irregular oscillations. After a certain interval there is a second abrupt pulse and rather larger but still irregular vibrations. In the last phase the maximum movement is reached, and it is characterised by smoother oscillations of longer period and greater amplitudes. The disturbance then gradually dies away after lasting, in many cases, for several hours. Now in the theory of elastic vibrations in a homogeneous solid it had long been known that two types of waves could be transmitted through the medium with different velocities. One of these is known as the compressional or longitudinal wave, in recognition of the fact that the displacement of the medium occurs in the direction of propagation, as in the case of sound waves. The other is called the distortional or transverse wave since the displacement is at right angles to the direction of propagation, as is the case with light waves. For each type of wave the speed of propagation depends on the elastic constants and the density of the material. Seismologists are now agreed that on the seismogram of an earthquake at a fairly large distance the primary pulse, usually denoted by P, marks the arrival of the compressional wave, whilst the commencement of the second phase, denoted by S, indicates the arrival of the distortional wave. Professor Turner very appropriately described these waves as the "push" and the "shake" waves. In addition to these body waves which travel through the medium, there is another type of wave which can travel along the surface in much the same way as water waves are propagated. These surface waves were discovered by Lord Rayleigh and are usually named after him. They play an important part in the third portion of the seismogram; the commencement of this portion is usually called the long wave or L phase.

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[1] The point within the earth at which the shock originates is called the focus ; the epicentre is the point on the surface vertically above the focus.

With the improvement of instrumental records it was soon found that the times of travel of the P and S waves gave a smooth curve when plotted against the distance from the epicentre. In 1907 Wiechert and Zoeppritz constructed travel-time graphs from a careful examination of the records of three well-defined earthquakes, and the tables derived by Turner from these graphs have served as a basis for all important seismological work until quite recently These tables have been used, for example, in the determination of epicentres and times of origin of earthquakes which are published regularly in the International Seismological Summary From the time interval between the P and the S phases at any station both the epicentral distance and the time of origin can be estimated by means of the tables, and if the time intervals for three well-distributed stations are known the position of the epicentre can be located. A rough estimate of the position of the epicentre can often be obtained at a single station provided that good records of all three components are available, for a comparison of the amplitudes of the components of the initial longitudinal pulse indicates the direction from which the wave has arrived; this information together with an estimate of the distance obtained from the S–P interval gives the locality of the epicentre.

It has been realised for some years now that the Zoeppritz-

Turner tables have appreciable errors, amounting at some distances to 20 seconds, and much work has been done recently on the construction of more accurate tables. H. Jeffreys and K. E. Bullen have obtained revised times of travel of P and S from a statistical analysis of data taken from a large number of the most fully observed earthquakes treated in the International Seismological Summary; these average times are probably accurate to within one or two seconds, but a point that remains doubtful is to what focal depth they correspond. The next step is to compare the average times with the data obtained from detailed studies of the records of individual earthquakes; in this way local variations in the velocities of the waves and differences in the structure of the earth may be revealed.

Waves in the Earth's Crust

Some information as to the constitution of the upper layers of the earth's crust in certain continental regions has been obtained from the study of near earthquakes. The records of shocks at distances of less than about 800 km. often show not only the normal P and S pulses, but also other pairs of compressional and distortional waves that have travelled in the crustal layers. According to Jeffreys' interpretation of these waves there are at least three, perhaps five, layers concerned; the upper layer of granite (which is generally overlaid by one or two kilometres of sedimentary material) is about 10 km. thick; the intermediate layers between the granite and the ultrabasic rock (which transmits the normal waves) are together about 20 km. thick.

The existence of the crustal layers not only accounts for the phenomena of near earthquakes, but also goes a long way towards explaining certain observed characteristics of the surface waves which are not in agreement with the theory of waves on a homogeneous solid. For instance, the simple theory does not account for the long train of waves which characterises the L phase; the velocity of Rayleigh waves in a homogeneous earth is independent of the wave length and so the Rayleigh phase should consist of a single disturbance instead of a long train. In a layered crust, however, we find a qualitative explanation in the effect of dispersion (*i.e.* the dependence of the velocity of a wave on its period), which converts an impulse into a series of approximately harmonic waves; the amplitude of Rayleigh waves dies down with depth in a distance proportional to the wave length, so the short waves are confined to the upper layer, in which the velocity is least, whilst the longer waves extend into the lower layer where the velocity is greater. A quantitative analysis of the Rayleigh waves is not at present available because the variation of their velocity with wave length has not yet been worked out. A further difference between the simple theory and the observed facts is that the motion in the theoretical Rayleigh wave is wholly in the plane containing the vertical and the direction of propagation, whereas the actual motion in the long-wave phase of an earthquake has a strong component at right angles to the direction of propagation. This difficulty was removed in 1911 by Love, who showed that another type of surface wave, in which the displacement is horizontal and perpendicular to the direction of propagation, can be transmitted in a layer of finite thickness resting on a deep solid layer ; it is necessary that the velocity of distortional waves should be greater in the lower layer. Love waves, as they are called, are equivalent to horizontally polarised S waves reflected up and down within the upper layer in much the same way as light waves in a slab of glass, the upper face of which is a perfect reflector and the lower face bounded by material of higher refractive index. Since the displacement in these waves is wholly horizontal they do not appear on seismograms of the vertical component. The study of Love waves has enabled Stoneley and others to obtain estimates of the thickness of the continental upper layers ; their results are in reasonable agreement with those given by near earthquakes. Seismological evidence as to the structure of the crust under the oceans is not so abundant, but the fact that surface waves travel appreciably faster along oceanic paths than along continental paths indicates that the granitic layer is thin or absent under the oceans ; this fits in with geological evidence which shows that granitic rocks are comparatively rare on oceanic islands.

WAVES IN THE INTERIOR OF THE EARTH

The waves of near earthquakes do not penetrate very deeply into the earth and the information which they yield is confined to the upper layers. The waves of distant earthquakes, on the other hand, reach great depths, and they enable us to obtain some knowledge of the constitution of the interior of the earth. In the first place, the slopes of the time-distance curves of P and S indicate that the velocities of these waves, which are about 8 and 4·5 km. per sec. respectively just below the crust, increase with depth to about 13 and 7 km. per sec. at 2,900 km. below the surface. Since the velocity depends directly on the elasticity and inversely as the density of the material through which the wave is propagated, it appears that the elasticity increases more rapidly with depth than the density. The rate of change of velocity is not uniform all the way down to 2,900 km. however; the latest time-distance tables of Jeffreys and Bullen indicate that there is a rapid increase of velocity at a depth of about 400 km. for which there is at present no satisfactory explanation. Further information about the rate of change of velocity with depth has been obtained by studying the variation of the amplitudes of recorded pulses with distance; by this method Gutenberg inferred that a discontinuity occurs at a depth of about 1,200 km.

The waves which penetrate to a depth of 2,900 km. travel to a distance of about 10,000 km. When this distance is exceeded the ordinary P and S waves become very feeble and difficult to identify, but at about 16,000 km. a new compressional wave appears and remains recognisable to the antipodes of the epicentre. The delay of this wave, compared with the ordinary P, shows that the velocity, instead of continuing to increase with depth, must suffer a decrease at some considerable depth This led Oldham to suggest that at this depth, which is now known to be about 2,900 km., there is a discontinuity which forms the boundary of a central core of the earth. The core acts like a lens and waves which are transmitted through it undergo refraction at the boundary. The effect is to produce a shadow zone in which neither the waves which just miss the core nor those which are refracted by the core appear; thus on seismograms recorded at distances between 10,000 and 16,000 km. neither the ordinary direct compressional wave nor the compressional wave which has travelled through the core can be recognised. Another point of very great importance is that the core is apparently unable to transmit distortional waves; the S waves which strike the core are transformed on refraction into compressional waves which can change back to the S type when the boundary is crossed the second time. Since for the transmission of distortional waves a material possessing some rigidity is necessary, it is inferred that the central core of the earth is non-rigid and is therefore composed of material in the liquid state. On the seismogram of a distant earthquake a number of phases can usually be recognised in addition to the P and S waves or the waves transmitted directly through the core. Some of these phases are due to reflexion of waves at or near the earth's surface. Reflexion can also take place at the core boundary, and the fact that such reflected waves produce sharp pulses on the records shows that the transition between the solid shell and the liquid core must be a rapid one.

DEEP EARTHQUAKES

Although there is still some difficulty in deciding what figure we may take as the average depth of focus of normal earthquakes it is almost certain that the majority of shocks originate at depths of less than 50 km. below the surface That some earthquakes occur at much greater depths than this, however, was suggested by the late Professor Turner, who noticed that for some shocks the times of travel of the compressional waves to stations nearly antipodal to the epicentre were consistently abnormal ; if the focus is deeper than normal the waves arrive at distant stations earlier than the normal time. For some years, however, Turner's hypothesis of deep focus was not generally accepted because he also maintained the possibility of abnormally shallow focus to account for a few shocks in which the compressional wave arrived late at antipodal stations, and this implied that the normal focal depth is as great as 250 km. Independent evidence of the occurrence of deep focus earthquakes was obtained by Wadati, who noticed that some Japanese shocks showed an abnormal distribution of intensity over the region in which they could be felt and that the time intervals between the S and the P phases were abnormally large near the epicentre. The question was placed beyond doubt by the present writer, who identified on the records of supposed deep-focus earthquakes certain phases which could only be explained as reflexions of P and S waves near the epicentre ; such reflexions may occur in normal earthquakes, but it is only when the focus is deep that these additional phases can be separated from the direct pulses Another characteristic of deep-focus earthquakes, recognised by Stoneley and others, is that the surface waves are very weakly developed ; this is in agreement with theory which shows that the amplitudes of these waves should fall off rapidly with depth. The more recent work on this class of shock is mainly concerned with the construction of time-distance curves applicable to various focal depths. The preliminary curves prepared by the writer were obtained by applying appropriate corrections for the effect of abnormal depth to the Zoeppritz-Turner tables for normal earthquakes, and they therefore suffered from the errors which were known to exist in these tables ; improved curves have been obtained from detailed studies of the records of individual shocks. The occurrence of earthquakes at such depths as 500 km., equivalent to nearly a tenth of the earth's radius, is of great importance, for it implies the existence of materials of great strength at these depths. It is not definitely known what is the significance of the observations which led Turner to attribute abnormally high foci to certain earthquakes, but it is probable that the apparent anomalies were due to certain phases being wrongly identified on the records.

EARTHQUAKES PERIODICITIES

Although the primary causes of earthquakes lie within the earth, it is quite possible that the actual time of occurrence of a shock is determined by external factors. As a result of the gradual development of stresses in the earth's crust dislocation has to occur sooner or later, and the extra impulse which is sufficient to start the break may conceivably be brought about by secondary causes outside the solid earth. It is not surprising therefore that many investigators have endeavoured to trace connections between the occurrence of earthquakes and other natural phenomena. There have been numerous attempts to find periodicities and regular variations in the frequency of earthquakes, but so far the only two that appear to be well established are the solar diurnal and annual variations found by Davison. It is found that on the whole earthquakes are more frequent during the night than during the day, and more frequent in winter than in summer. A possible explanation, suggested by Davison and by Omori, is that these variations are due to the annual and diurnal changes in atmospheric pressure. The eleven-year period has been examined and it appears that in all parts of the world earthquakes are more frequent in the years of many sunspots. Davison also investigated a nineteen-year period, and in this case the maximum frequencies of the northern hemisphere correspond with the minimum frequencies of the southern hemisphere ; the period is the same as the nutation period of the earth, and it appears probable that the strains associated with the movements of the earth's axis are factors in determining when earthquakes shall occur.

A large earthquake is usually followed by a series of after-shocks, the daily numbers of which at first decline very rapidly. In a few days, however, the frequency curve flattens out but, as was demonstrated by Omori, it shows fluctuations which occur at intervals of a few days. Davison has examined these periodicities recently and finds that they are related to the phases of the moon. The occurrence of after-shocks was also investigated by Turner, who noticed that they appeared to recur at intervals of multiples of 21 minutes ; the reality of this period, however, cannot be said to be definitely established.

MICROSEISMS

Although seismology is usually considered to be concerned only with earthquakes in the ordinary sense of the word, it also includes other shakings of the earth which are not due to fractures or sudden disturbances. At nearly every seismological station the records frequently show continuous oscillatory movements known as microseisms. The oscillations are much greater in winter than in summer and they appear to be associated in some way with atmospheric storms ; in fact, in some parts of the world attempts have been made to use them as a means of forecasting the approach of stormy weather. The exact nature of the connection is still in doubt. According to one theory microseisms are due to sea waves breaking on exposed coasts, but from a recent survey of microseismic disturbance during one month all over the world A. W. Lee has come to the conclusion that there is no good evidence that waves on one coast or another are directly responsible for the production of microseisms. Neither is there any satisfactory evidence that the phenomenon is due to sea waves in shallow water There still remains the possibility that sea waves in deep water are effective, but this idea involves certain theoretical difficulties which must be cleared up before a satisfactory explanation is obtained. The effect of geological structure on microseismic disturbance has also been investigated theoretically by Lee, and it appears that the larger microseisms are to be expected where the sedimentary rocks are of greater thickness ; the results of the world survey show that this conclusion is consistent with the geological evidence.

CONCLUSION

It will be realised from this brief outline that modern seismology affords an excellent illustration of the progress which can be made by the application of sound physical principles to a subject which hitherto had no claim to be recognised as a science. From the foundations so firmly laid by Milne seismology has developed in accordance with the requirements of modern investigation and has become a quantitative science well worthy of its place as a branch of geophysics. It has already thrown much new light on the properties and structure of our globe, and it provides the most hopeful means of probing the secrets of the earth's interior.

ELEMENTS, OLD AND NEW[1]

By JAMES KENDALL, M.A., D.Sc., F.R.S.

Professor of Chemistry in the University of Edinburgh

"Chemistry as a science is essentially a thing of the present, or rather of the present and the future"—so runs the opening sentence in James Campbell Brown's *History of Chemistry* It is, indeed, one of the most fascinating features of chemistry that its outlook is forward, that its boundaries are continually extending, that each year brings new advances in knowledge Nevertheless, there is an art as well as a science of chemistry, and no one who proposes to participate in the progress of the subject can afford to neglect its cultural and historical side The more fundamental the problem under study, the more necessary it becomes for the experimenter to orient himself with the work of past masters before drawing up a plan of attack. In the consideration of the most fundamental of all chemical problems, the constitution of matter, it is not surprising therefore that I should find myself compelled to base my discussion upon the earliest of scientific philosophers, the ancient Greeks.

The continuity of matter, and its ultimate origin, were two questions which provoked their most intense speculation. All matter, according to Democritus, was granular in its structure; it was made up of minute particles or "atoms," and when we had (in fancy or otherwise) subdivided any particular sample of matter until we had reached individual atoms, then it was impossible to persevere any longer, since the atoms were indestructible, indivisible, unchangeable and eternal. The opposite opinion of Anaxagoras, however, that all matter was continuous and infinitely divisible, gained universal prestige through its adoption by the school of Aristotle, and until the beginning of the nineteenth century atoms remained completely in eclipse. As regards the ultimate source of matter, some favoured fire, others air, others water, others earth, but a compromise was finally reached whereby it was postulated that there were four "elements"—fire, air, water and earth—and that all matter was built up out of these four elements, assembled together in different proportions.

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[1] An abstract of the twenty-fifth Bedson Lecture, delivered at Newcastle-upon-Tyne, March 2, 1934, and of a lecture before the Chemical Colloquium of East London College, March 8, 1934.

This opinion, although assailed by the alchemists in the Middle Ages, who based their faith on the three principles—mercury, sulphur and salt—survived until the middle of the seventeenth century A typical argument in its support is quoted by Boyle in his *Sceptical Chymist*:

"If you will but consider a piece of green wood burning in a chimney, you will readily discover in the disbanded parts of it the four elements. . . . The fire discovers itself in the flame by its own light; the smoke by ascending to the top of the chimney, and then readily vanishing into air, like a river losing itself in the sea, sufficiently manifests to what element it belongs and gladly returns. The water in its own form boiling and hissing at the ends of the burning wood betrays itself to more than one of our senses; and the ashes by their weight, their fineness, and their dryness, put it past doubt that they belong to the element of earth."

Robert Boyle—"the father of chemistry and the brother of the Earl of Cork"—proceeded to refute both the hermetic philosophers, who set their trust in the four elements of Aristotle, and the vulgar spagyrists, who believed in the three principles, and put forward the following as his own definition of the term element:

"I mean by elements certain primitive and simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved."

Chemistry was still not sufficiently advanced at the time of Boyle for this definition to be given immediate practical application—the phlogiston theory, to which I shall return later, constituting the chief stumbling-block—but a century later Lavoisier placed it on a definite experimental basis, and all substances were regarded as worthy of the name of elements which could not be decomposed into, or built up by combination from, other substances in the laboratory. It is true that a few substances caused a great deal of controversy before their status could finally be agreed upon—quicklime, for example, was considered an element until Davy decomposed it by means of an electric current in 1808, and chlorine was long considered a compound of some other element with oxygen—but as chemists grew more experienced in their methods of attack the debatable cases became fewer and fewer, and the list of recognised elements steadily grew until the present maximum of 92 was attained The revival of the atomic theory by Dalton enabled us to assign a definite atomic weight to each element, and their classification in order of increasing atomic weight gradually resulted in the triumphant rounding-off of the periodic system of Mendelejeff, which arranged the 92 elements in a limited series of "chemical families." In spite of this separation into groups, however, the atoms of different elements were regarded as fundamentally distinct until the end of the nineteenth century. If the chemist of that time ever visualised the atom he visualised it as a tiny hard round ball; it was something static; it was dead matter; transmutation was a foolish dream of the alchemists.

There had been, it must be noted, an interesting suggestion advanced in 1815 by William Prout, a young Edinburgh medical graduate, that all atomic weights could be expressed by whole numbers on the basis hydrogen, the lightest, as unity, and that the atoms of all elements might therefore be built up from more or less complex aggregates of atoms of hydrogen. At first Prout's idea gained considerable support, but as more exact analytical methods were developed, divergences from integral values for the atomic weights of the heavier elements became so obvious that in 1860 the Belgian chemist Stas wrote the doom of Prout's hypothesis in the words: "I have arrived at the absolute conviction, the complete certainty, so far as it is possible for a human being to attain to certainty in such matters, that the law of Prout is nothing but an illusion, a mere speculation definitely contradicted by experience."

With the advent of radioactivity, however, all our notions about the atom had to be recast, and Prout's hypothesis in a revised form was disinterred The complex series of radioactive disintegrations from uranium through radium to lead demonstrated that atoms of one element were, after all, really transmutable to atoms of another, and a common fabric for the atoms of all elements was ultimately established in the dual form of protons (units of positive electricity) and electrons (units of negative electricity). Our first systematic "picture" of the atom, formulated by Rutherford in 1911, showed it as consisting of a minute central nucleus, positively charged, surrounded by planetary electrons. The nucleus, containing all the protons, is responsible for practically the entire *mass* of the atom; the external electrons, being set at relatively large distances from the nucleus, are responsible for practically all its *volume*. Only in radioactive transformations does the nucleus change; ordinary chemical reactions affect merely the external electrons.

The next step was made by Moseley, whose promising career was cut short by a Turkish bullet in the trenches at Gallipoli. Moseley lined up the elements like a company of soldiers on parade and made them call their " atomic numbers "—1, 2, 3, 4, etc., up to 92. These numbers represented, for each type of atom, the number of external electrons or, since the atom as a whole is neutral, the number of free protons in the nucleus. Hydrogen, the first element in the line, has one of each ; helium, the second, has two ; chlorine, the seventeenth, has seventeen ; uranium, the last, has ninety-two. All atoms except hydrogen also contain, it must be noted, " proton-electron pairs " (more recently termed *neutrons*) in their nucleus, in sufficient quantity to make up the difference between the atomic number and the atomic mass.

How can we, on this new basis, account for an element such as chlorine, with an atomic weight of 34·456 ? While the elements with atomic weights that are essentially integral are far too frequent for the agreement to be due merely to chance, there do exist many elements like chlorine with atomic weights far removed from a whole number. The explanation, due to Aston, is very simple, but utterly at variance with the old ideas. There are *two* kinds of chlorine atom—one a perfect 35, the other a perfect 37—and naturally-occurring chlorine is a mixture of the two in the ratio required to give an average value of 35·456. The only difference between the two *isotopes*, as they are called, is that the heavier contains two extra proton-electron pairs in its nucleus. Externally, both isotopes are exactly the same, and therefore they are chemically identical in all of their properties, since, as has already been mentioned, ordinary chemical reactions affect only the external electrons. A great many other elements besides chlorine have also been shown to be isotopic, but this is a point which I shall consider later in greater detail. It is time now for me, having made a lightning survey of modern advances in our conception of the elements, to return to the four elements of Aristotle.

It is very significant to note that modern chemistry may be divided into four main periods, in each of which one of these four elements—fire, air, earth and water—has been predominant. The " fire period " corresponds to the phlogiston age of chemistry, stretching between Boyle and Lavoisier. Boyle knew that the rust of a metal was heavier than the original metal from which it had been obtained by the action of heat, but he thought that the increase in weight was due to the fact that particles of heat had permeated the metal, and he even wrote a book to prove that heat had weight ! The advocates of the phlogiston theory thought that when a metal like iron was changed to rust by heating, something was driven out of it, and this invisible something was christened "phlogiston." When they were challenged to explain why the rust was heavier than the metal, instead of lighter, they advanced the beautiful hypothesis that phlogiston had a negative weight. Until Joseph Black put chemical experimentation on a strictly quantitative basis, the phlogiston theory could not be refuted, but when Lavoisier showed that it was oxygen from the air that united with a metal to form rust, and clinched every step in his experiments with numerical proof, the fire period of chemistry was ended.

It was succeeded by a brilliant air period—the age of "pneumatic chemistry"—during which all the common gases were recognised and studied, and even the rare gases of the atmosphere only just missed discovery by Cavendish. The small bubble of uncombined gas which Cavendish found to persist after repeated sparking of atmospheric nitrogen with oxygen remained undisturbed for more than a century, until Rayleigh and Ramsay added a whole new family of elements—neon, argon, krypton, xenon and niton—to our list.

The greater part of the nineteenth century constitutes the earth period of chemistry, during which analysis of rocks and earths from all quarters of the world resulted in the discovery of a multitude of metallic and non-metallic elements. Many more have been claimed, indeed, than were actually confirmed by subsequent investigation. Thus the rare earth family of fifteen elements beginning with lanthanum, which was long regarded by chemists as inordinately prolific although its status in the periodic system has now been satisfactorily established, has received at various times no fewer than seventy-six candidates in all for admission into its ranks. Most of these have not succeeded in passing the final tests. As an instance I may cite the element *carolinum*, proposed by an American chemist in 1904. When the same investigator, almost immediately afterwards, announced a second new element and wished to christen it *berzelium*, the counter-suggestion was made that one be called North Carolinum and the other South Carolinum! Unfortunately, however, both turned out to be illusory.

I trust that I may be pardoned, at this point, for a brief digression. Some years ago, I was asked to deliver a lecture at the University of Minnesota on "The Rare Earths." Arriving at Minneapolis, I purchased a local newspaper, and saw to my horror a headline therein: Kendall to speak on Rare Herbs. The reporter went on to say that I was a noted authority on rare herbs, that I had written a book on rare herbs, and that I had discovered several new rare herbs—all of which, of course, is untrue. The title of my talk, it seemed, had been given to the Press from the University over the telephone, and had been slightly confused in transmission When I entered the lecture theatre, I found an enthusiastic audience which appeared to me, in my embarrassment, to be mainly composed of botanists, pharmacists and boot-leggers, all waiting to get the latest dope on rare herbs. I was undecided how to start, until I remembered that one of the rare earth metals is named erbium. No Englishman is ever supposed to be capable of sounding an aspirate in the United States, so I concentrated my remarks on erbium for three-quarters of the lecture and then, when I ran out of material, switched to ytterbium. When all was over, I believe, some of my listeners were still uncertain as to whether I had been speaking about rare earths or rare herbs.

The earth period of chemistry is now over, but many problems connected with the earths are still of interest to chemists from a more modern viewpoint. I wish to describe, in short, one such problem which has recently been studied in my laboratories. Potassium, a common constituent of many rocks, is known to be slightly radioactive, and its radioactivity has been traced to the isotope of potassium with mass 41, whose half-life period has been estimated at between one hundred thousand million years (Holmes and Lawson) and one million million years (Mühlhoff). Now the product of this radioactive disintegration of potassium with mass 41 is calcium with mass 41, whereas the atomic weight of normal calcium is only 40·076 The possibility exists, therefore, that calcium extracted from a very old potassium-rich mineral may possess an average atomic weight different from that of ordinary calcium. Two suitable minerals—pegmatites from Rhiconich, Sutherlandshire, and Portsoy, Banffshire, both rich in potassium and containing only a small percentage of calcium, and with ages estimated by geologists at approximately one thousand million years and four hundred million years respectively—were accordingly examined, and the calcium obtained from them was found, indeed, to have an atomic weight slightly above the normal, namely, 40·089 – 40·092. Calculating back from this deviation, we arrive at a half-life period for the potassium isotope with mass 41 of one hundred thousand million years, in accordance with the estimate of Holmes and Lawson.

The fourth period of chemistry—the water period—may be regarded as beginning fifty years ago, when the work of Van't Hoff, Ostwald and Arrhenius made dilute aqueous solutions the central topic of physical chemistry. As time went on, the solutions investigated became more and more dilute, almost justifying the jibe that physical chemistry was the study of slightly contaminated water, but the climax has recently been reached in the discovery that pure water itself is not a single compound, since it contains a small fraction of " heavy water " with quite distinct properties. The steps leading up to this discovery, perhaps even more surprising than that of the rare gases of the atmosphere, may now be discussed in brief outline.

The revival of the hypothesis of Prout, mentioned in an earlier section, would seem at first sight to necessitate that, since all atoms are built up of aggregates of protons and electrons and the hydrogen atom contains one of each, all atomic masses should be exact multiples of that of hydrogen. Actually this is not the case ; it is true that atomic masses are all almost exactly integral on the familiar basis oxygen = 16, but on this basis hydrogen is not 1 but 1·00778, and if we recast our table with hydrogen as unity all the heavier atoms deviate very significantly from integral values. The explanation adduced for this is that the close packing of protons and electrons in the minute nucleus of heavier atoms involves such an enormous change in their energy as to affect their mass appreciably, and this packing effect is very nearly constant throughout the whole periodic system. The more exact later work of Aston does, indeed, indicate a slight variation in the form of a regular curve as we go up the series of elements, but still the only atomic mass that is fundamentally out of line is that of hydrogen. It is perhaps rather unfair to say this when in reality the hydrogen atom is the only one which behaves normally, but justice has to give way to convenience in this case as in many others.

Until very recently, the agreement between the chemical atomic weight for hydrogen (1·00778) and the mass of the hydrogen atom derived from Aston's mass-spectrograph measurements was considered perfect. The discovery that oxygen contained small amounts of isotopes with masses 17 and 18, however, disturbed this exact concordance. The discrepancy, it may be noted, was only about one-fiftieth of one per cent., but this was sufficient to entice chemists to postulate the existence of a heavy isotope of hydrogen in quantity sufficient to restore the balance, and spectroscopic evidence of an isotope with mass 2 was obtained by Urey, Brickwedde and Murphy in 1932.

This heavy isotope constitutes only about one part in six thousand of ordinary hydrogen, and in view of the identity of chemical properties exhibited by the isotopes of other elements—a point discussed in detail previously—it would appear unlikely that it could be readily isolated in a pure state, or that its compounds would be materially different from those of ordinary hydrogen. But the similarity between the isotopes of hydrogen is by no means comparable with the similarity between the isotopes of heavier elements, say the two isotopes of chlorine. The new variety of hydrogen atom comprises a nucleus of one free proton and one proton-electron pair, with one external electron. Not only is its nucleus twice as massive as that of ordinary hydrogen, whereas the difference between the two chlorine nuclei is only a small fraction, but this nucleus is also —particularly in the polar compounds of hydrogen where the single protecting electron migrates away—practically naked, whereas the nuclei of chlorine are wrapped around with successive and similar shells.

While, therefore, many years' work on the separation of the isotopes of chlorine by Harkins merely resulted in a very meagre fractionation, success in the separation of the two types of hydrogen has been rapid and complete. The most striking results have been obtained by Lewis, at the University of California, who by a series of fractional electrolyses has obtained a residue of pure " heavy water "—water in which all the hydrogen is in the form of the heavy isotope with mass 2 In view of the many articles that have recently appeared discussing this compound, I shall restrict myself here to mentioning its salient differences from ordinary water. Its density is more than ten per cent. higher, it freezes at +3·8° C (approximately 39° F.), it boils at 101 6° C. (approximately 215° F.), and it has a maximum density at 11·6° C (approximately 53° F.).

A few remarks about its biological behaviour, however, must be appended. Tobacco seeds will not germinate in pure heavy water, and the life processes of plants in diluted heavy water appear to be considerably retarded. Pure heavy water has also been shown to be toxic to various species of animal life, such as tadpoles and flatworms. What its effect upon the human organism may be is at present only a matter of interesting speculation ; it is quite possible that it may turn out to be of immense importance in medicine. Whether the many forms of " heavy alcohol " which are theoretically possible will prove to be inferior or superior to the familiar variety in potability and potency is another problem for the chemist of the near future to investigate.

In conclusion, the question may pertinently be raised : What will be the next period of chemistry ? Without attempting too ambitious a forecast, I should like to call attention to the fact that fire, air, earth and water do not, after all, exhaust the list of the classical elements. There still remains the " quintessence "—the imponderable "ether." The physicists have been much more faithful to this throughout the centuries than the chemists have been to the other four; only in very recent years has scepticism become general, and some of the older school still proclaim their faith in the ether. Perhaps we may see, in the current fusion which chemistry and physics are undergoing, the initial stages of an ether period of chemistry, when the effect of imponderable radiations on chemical substances will be the main topic of study, and a "universe of light" will be thrown upon many fields which remain at the moment quite obscured.

THE TURBULENT MOTION OF FLUIDS

By G. F. P. TRUBRIDGE, M.Sc.

A KNOWLEDGE of the nature of the turbulent motion of liquids and gases is important in the solution of problems in aerodynamics and engineering. In many technical applications of hydrodynamical theory the effects of turbulence play an essential part. In this article the nature of turbulence from the physical and mathematical points of view will be discussed A brief account will be given of the experimental work and of the recent theories which have been put forward in order to explain the phenomena.

It might well be asked what is the exact meaning of the word "turbulence," but so far no one has given a concise definition (although it has been known for some time that there were two distinct types of flow for a fluid). Osborne Reynolds was the first investigator to make a distinct study of the transition from laminar to turbulent motion, but he did not use the word "turbulence" He experimented on the flow of water in glass tubes A stream of water was allowed to flow slowly through a horizontal glass tube of about 2 inches diameter Since the transparency of the fluid rendered it impossible to see the internal motion, a small quantity of aniline dye was introduced at the entrance to the tube by means of a fine tube. Reynolds found that after a short time a single coloured stream extended right along the tube. As the velocity of flow was increased the coloured stream gradually became distorted and above a certain velocity, U_1, eddies began to form. As the velocity increased the eddies moved towards the mouthpiece of the tube and finally the whole motion was winding or sinuous. When the action was reversed the eddies died out gradually. The critical velocity U_2 in this case was found to be more definite than the velocity U_1, which was found to depend on the presence of slight disturbances which may be present before the water enters the tube.

Experiments with other fluids led to the introduction of the so-called Reynolds number R. This dimensionless number is used extensively to indicate the nature of the flow of fluids. If U is the mean velocity of flow in cm. per sec., μ the viscosity in grams per. cm. per sec., ϱ the density in grams per cc., and d the diameter of the tube in centimetres, then $R = \varrho Ud/\mu$.

When $R < R_2$ the motion is generally called laminar while when $R > R_1$, the motion is called turbulent. The indefinite region between these limits is also sometimes called turbulent. The term "completely developed turbulence" is used when the average properties of the flow are fairly regular. For very large values of R when the motion is extremely irregular, the state of flow is said to be tumultuous.

Turbulent motion consists then of the irregular and random motion of vortices or eddies. The velocity and pressure at a given point in the fluid have no fixed values, but fluctuate about definite mean values. Particles situated very near one another acquire quite different velocities. A turbulent flow usually experiences a high resistance which is approximately proportional to the second power of the velocity of the mean motion. This increase of resistance is usually undesired, but there is a compensation in that the presence of turbulence renders possible a greater rise of pressure.

Since Reynolds carried out his fundamental experiments many other investigations have been made in the flow of fluids. The variations of flow and of resistance as the Reynolds number changes may be studied by varying the diameter and the length of the tube, the velocity of flow and the working fluid. Very extensive experiments have been made by Stanton and Pannell at the National Physical Laboratory, working with air, water, and thick oils in commercially smooth drawn brass tubes and with velocities of flow ranging from 30 to 6,000 cm. per sec. The empirical formula for the surface friction D per unit area of the tube, which has been found by Lees for these results is,

$$D = \tfrac{1}{4}\varrho U^2[0{\cdot}00357 + 0{\cdot}3052\,(R)^{-0{\cdot}35}].$$

This formula holds from the critical velocity (for which $R = 2{,}320$) through the whole range of the experiments and is the same whatever the type of fluid.

For circular pipes R_2 is about 2,300, which value is not affected by the roughness of the pipe. For rectangular channels $R_2 = 1{,}440$. Davies and White found that there is a third critical velocity below which eddies are not transmitted through the pipe although they may be present in the fluid as it enters the pipe. The Reynolds number for this type of motion is 140 for square pipes and higher for circular pipes. According to Schiller and Ekman the value of R_1 can be raised to 22,000 for smooth tubes and to 13,200 for rough tubes.

Certain other methods of following the motion of a fluid besides using coloured streams have been introduced. Hele Shaw used small globules of air in a liquid to make the stream lines and turbulence visible. The fluctuations of velocity have been measured by a Robinson hot wire anenometer by Burgers and others. The Pitot tube with its orifice facing the flow and connected with a manometer has been used extensively. It must be remarked that the variations of the velocity at one place are so rapid that most measuring instruments furnish only the mean values of the quantity concerned. The instruments act as integrating devices.

Fage and Townsend have examined the turbulent flow in pipes with an ultramicroscope. Minute particles present in tap water were intensely illuminated and viewed against a dark background. All observations were made without introducing any instruments in the fluid. As the speed of flow was increased through the critical range of Reynolds numbers, the changes of flow occurred with great suddenness There was no sign of a growth in the degree of turbulence, but only a change in the periods of laminar and turbulent motion.

It will not be possible to give here detailed accounts of the experimental results and theories for the various problems which have been studied. The flow between two parallel planes, when stationary or when one or both planes are moving, the motion in tubes or cylindrical or other cross-sections, and between two concentric rotating cylinders have been studied extensively. The excellent Report on Hydrodynamics published in 1932 by the National Research Council of America gives a great number of results and contains a comprehensive bibliography.

In this paper we shall confine our attention to the problem of the flow between two smooth parallel planes. For this type of flow Kozeny gave the formula :

$$D = \tfrac{1}{2}\varrho U^2[0{\cdot}00066 + 0{\cdot}04883(R)^{-\frac{1}{4}}]$$

where the length used in the definition of R is half the distance between the planes.

Osborne Reynolds has developed a theory in which the real motion is decomposed into the so-called mean flow and the continually fluctuating relative motion. The assumption is made that the equations of motion suggested by Navier and Stokes for the flow of a viscous fluid are valid when the motion is turbulent. The recent researches of Taylor for the motion between two rotating concentric cylinders, in which these equations are used, give the best confirmation of their validity.

Let us then consider the flow of an incompressible viscous fluid between two parallel walls at a distance h apart. The X-axis of co-ordinates is taken in the direction of the main flow, the Y-axis perpendicular to the walls and the Z-axis perpendicular to both of the other axes. If u, v, w, denote the component velocities of the particle which is at the point x, y, z at time t, and p denotes the fluid pressure, the equations of motion are of the type:

$$\frac{\partial u}{\partial t} + \frac{\partial(u^2)}{\partial x} + \frac{\partial(uv)}{\partial y} + \frac{\partial(uw)}{\partial z} = -\frac{1}{\rho}\frac{\partial p}{\partial x} + \frac{\mu}{\varrho}\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right)$$

together with the equation of continuity:

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0.$$

Reynolds regards u, v, w as fluctuating functions. Time means are introduced by means of the equation:

$$\bar{\phi}\int dt = \int \phi(x, y, z, t + t')dt'$$

where each integral is taken between the limits $\pm$ T, T being a fixed time independent of x, y, z, t. In other cases space means are used. Reynolds now writes $u = \bar{u} + u'$, $v = \bar{v} + v'$, $w = \bar{w} + w'$, where $\bar{u}$, $\bar{v}$, $\bar{w}$ are the component velocities of the mean motion and u', v', w' are the component velocities of the relative motion. Mean values are always denoted by a bar.

When the fluctuations are sufficiently numerous and distributed at random, certain rules for the mean values, which simplify the calculations, have been suggested by Reynolds and used by many writers. By making use of these rules after taking the average values in the equations of motion, it can be easily shown that the equations for turbulent flow are of the same type as the equations for laminar flow, except for the additional stress components,

$$\tau_{xx} = \overline{\varrho u'u'},\ \tau_{yx} = \tau_{xy} = \overline{\varrho u'v'}$$

due to the turbulence. If the transformed equations are multiplied by u, v, w and added, the energy equation for the total flow is obtained. When this expression is integrated over the channel criterion for the increase or decrease of the mean energy of the turbulence may be obtained.

From these investigations a critical value R = 258 for flow between parallel walls may be deduced. Lorentz has used a similar method, in which he introduced a special distribution of vorticity in the form of elliptic vortices into the fluid, to discuss the stability of flow. Orr using the method and certain ideas from wave motion found the values $R = 180$ for circular tubes and $R = 117$ or 177 for flow between parallel planes according to the function assumed for the velocity. In these investigations it was necessary to restrict the motion to two dimensions to make the mathematics tractable.

From these theories we obtain some idea as to how the mean motion is influenced by the relative motion, as in the equation governing the former occur the mean values of the squares and the products of the velocity components of the relative motion. The principal object of a theory of turbulence is to calculate these mean values. A complete theory should offer some conception as to the manner in which the turbulence arises and give information about the structure of the motion. The mechanism of the connection between the motion in the central part of the channel and the boundary in the immediate vicinity of the walls and the cause of the greatly increased resistance should also be elucidated. A formula for the resistance should follow theoretically to replace the empirical laws which have been put forward, and agree with experiment over wide ranges of values for R.

During the last few years Prandtl and Karman have succeeded in developing theoretically formulæ for the velocity and resistance in the case of turbulent motion between parallel planes which are in excellent agreement with the experimental results of Nikuradse and Dönch, in contrast to the hitherto purely empirical formulæ. These new expressions remain valid for very large Reynolds numbers. In this theory an attempt is made to calculate the additional stresses due to the turbulence in terms of the velocity of the mean motion. The suggestion is made that the tensor for the eddy stresses has a simple connection with the velocity vector $\bar{u}$. The essential step is the introduction of a length l, which is called the Mischungsweg or mixing distance, and which plays a similar part to the free path in the kinetic theory of gases. This length was at first regarded as the diameter of a mass of fluid moving as a whole, but afterwards it was found more useful to identify it with the length of the path described by such a mass of fluid before it loses its individuality by mixing with the neighbouring masses of liquid. Consider a moving mass of fluid having a velocity equal to the mean velocity $\bar{u}$ at a place where it is at a certain time. If after a short interval it is displaced a distance l in the y-direction transverse to the direction of flow, the change in the velocity of this mass is $l\partial\bar{u}/\partial y$, by Taylor's theorem, provided l is small. The average fluctuation u' can be thus put equal to $l\partial\bar{u}/\partial y$. Assuming that the lateral motion v' arises because two fluid masses with different velocities u' collide or separate, v' is proportional to u'. The eddy-shearing stress acting between two adjacent layers of liquid may then be expressed in the form:

$$\tau = \varrho\overline{u'v'} = \varrho l^2(\partial\bar{u}/\partial y)^2$$

where the factor of proportion and the correlation factor occurring in the formulation of the mean product have been absorbed in the quantity l. This formula for the eddy stresses has been generalised for three dimensions by Prandtl and Tollmien. The quantity l is a function of position and is zero at the walls because there the lateral motion is restricted. Throughout this theory it is necessary to use the idea of eddy viscosity. This coefficient of molar or mechanical viscosity depends on the speed and also on the boundaries, whereas the molecular coefficient μ is independent of these quantities.

In a similar way Karman has developed a formula for the eddy stress for two dimensional motion. He uses the idea of mechanical similarity. The fluctuations of velocity in the y-direction are assumed to be small and of such a nature that they arise from a steady motion superposed on the main stream. The velocity of the mean motion is expressed in the form:

$$\bar{u} = \bar{u}_1 y + \tfrac{1}{2}\bar{u}_2 y^2 + \ldots$$

and the stream function for the whole motion in the form:

$$\psi(x, y) = \tfrac{1}{2}\bar{u}_1 y^2 + \tfrac{1}{6}\bar{u}_2 y^3 + \ldots + \psi_0(x, y)$$

where $\psi_0(x, y)$ is the stream function for the perturbation. Let us write now $x = l\xi$, $y = l\eta$, $\psi_0 = Af(\xi, \eta)$, where l and A are lengths dependent on $\bar{u}_1$, $\bar{u}_2 \ldots$ and f is independent of these quantities. If the two dimensional form of the equations of motion is taken and the stream function and vorticity are introduced by means of the relations:

$$u = \frac{\partial\psi}{\partial y},\ v = -\frac{\partial\psi}{\partial x},\ \zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}$$

we get the equations:

$$\frac{\partial\psi}{\partial x}\frac{\partial\zeta}{\partial y} - \frac{\partial\psi}{\partial y}\frac{\partial\zeta}{\partial x} = \frac{\mu}{\varrho}\nabla^2\zeta,\ \zeta = \nabla^2\psi$$

where ∇^2 is the operator $\partial^2/\partial x^2 + \partial^2/\partial y^2$. By substituting the above expansions in these equations and retaining only the principal terms, it is found that the resulting equation can be satisfied by taking, $\bar{u}_1 l^2 \cong A$, $\bar{u}_2 l^3 \cong A$, so that

$$l \cong \frac{\bar{u}_1}{\bar{u}_2} = \frac{\partial\bar{u}}{\partial y}\bigg/\frac{\partial^2\bar{u}}{\partial y^2},\ A \cong \frac{\bar{u}_1^3}{\bar{u}_2^2} \cong l^2\bar{u}_1.$$

To obtain these results μ must be put equal to zero. The eddy stress is then given by,

$$\tau = -\varrho\overline{u'v'} = \varrho\frac{\partial\psi_0}{\partial x}\frac{\partial\psi_0}{\partial y} = \varrho\frac{A^2}{l^2}\overline{\frac{\partial f}{\partial\xi}\frac{\partial f}{\partial\eta}}$$

so that $\tau \simeq \varrho l^2\bar{u}_1^2,\ l = k\bar{u}_1/\bar{u}_2.$

For the motion between two parallel planes $y = \pm h$, we obtain, with the substitution $l = k\bar{u}_1/\bar{u}_2$ the expression,

$$\tau = \tau_0 y/h = k^2\varrho\bar{u}_1^4/\bar{u}_2^2$$

for the shearing stress at a distance y from the centre of the channel. Since $\bar{u}_2/\bar{u}_1^2 = k(h\varrho/\tau_0 y)^{\frac{1}{2}}$, a simple integration gives the formula :

$$\frac{\partial\bar{u}}{\partial y} = \frac{1}{2k}\left(\frac{\tau_0}{\varrho}\right)^{\frac{1}{2}}\frac{1}{h-(hy)^{\frac{1}{2}}}$$

where we make $\bar{u}_1 = \infty$ at $y = h$. Integrating again we get with $\bar{u}_{\max} = \bar{u}_0$:

$$\bar{u} = \bar{u}_0 + \frac{1}{k}\left(\frac{\tau_0}{\varrho}\right)^{\frac{1}{2}}\left[\log\left\{1-\left(\frac{y}{h}\right)^{\frac{1}{2}}\right\} + \left(\frac{y}{h}\right)^{\frac{1}{2}}\right]$$

Using the values of τ_0 found by Nikuradse and Dönch in their measurements of the velocity distribution, it is found that a good agreement with experiment is obtained by setting $k = 0{\cdot}36$. For the mixing length l we obtain the result :

$$l = ku_1/u_2 = 2k\{(hy)^{\frac{1}{2}} - y\}.$$

If we put $y = h - y_1$, in the region of a plane we get,

$$l = ky_1\left(1 - \frac{y_1}{4h} - \frac{y_1^2}{8h^2} - \ldots\right)$$

By using Taylor's idea that the distribution of the vortices in the turbulence at the wall can depend only on τ_0, and certain ideas about the boundary layer, a formula for the resistance which agrees well with experiment can be deduced. This formula contains two disposable constants, and is valid for values of R from 2,000 to 1,600,000.

Instead of the assumption that $l = k\dfrac{\partial\bar{u}}{\partial y}\Big/\dfrac{\partial^2\bar{u}}{\partial y^2}$ made by Karman to give some ideas about the magnitude of this quantity l, Prandtl assumed the formula : $l = ky$, where k is a numerical constant. This theory was very inspiring and produced many interesting theoretical solutions to problems which had long remained unsolved.

From the nature of turbulent flow it appears likely that an application of the methods of statistical mechanics might yield interesting results. The first paper on this subject was publishe in 1924 by Von Karman, who made use of the idea of probabilit met with in the kinetic theory of gases. Let us consider th special case in which the stream function for the fluctuating relativ velocity is given by $\psi = A\cos(\alpha x + \beta y + \gamma)$, so that

$$u' = A\beta\sin(\alpha x + \beta y + \gamma),\quad v' = -A\alpha\sin(\alpha x + \beta y + \gamma).$$

We then have,

$$\overline{u'v'} = -\tfrac{1}{2}A^2\alpha\beta,\quad \overline{\left(\frac{\partial u'}{\partial y} - \frac{\partial v'}{\partial x}\right)^2} = \tfrac{1}{2}A^2(\alpha^2 + \beta^2)^2,$$

$\tfrac{1}{2}\overline{(u'^2 + v'^2)} = \tfrac{1}{2}c^2 = \tfrac{1}{4}A^2(\alpha^2 + \beta^2)$, where c is the mean velocity o fluctuation When we substitute for A, we obtain :

$$\overline{u'v'} = -\tfrac{1}{2}\frac{c^2\alpha\beta}{\alpha^2 + \beta^2},\quad \overline{\left(\frac{\partial u'}{\partial y} - \frac{\partial v'}{\partial x}\right)^2} = \tfrac{1}{2}c^2(\alpha^2 + \beta^2).$$

By fixing the value of c it is possible to determine the distribution of the quantities α, β by means of the theory of probability. By using the equation of energy a distribution function is deduced, from which the mean value of the local frequency in the x-direction and the "wave length" $\bar{\lambda}$ of the distribution are given by the expressions :

$$\bar{\alpha}^2 = 0{\cdot}155\frac{U_0\varrho}{\mu h},\quad \bar{\lambda} = \frac{2\pi}{\bar{\alpha}} = 2{\cdot}5\frac{h}{\sqrt{R}} = 2{\cdot}5\sqrt{\left(\frac{\mu h}{\varrho U_0}\right)},$$

where U_0 is the velocity obtained by dividing the volume of fluid which in unit of time flows through a perpendicular section of the channel, by the area of that section.

This statistical theory is not adequate enough for the determination of the quantity c. According to Von Karman, "for this purpose we must consider what actually happens near the wall, and establish in particular the conditions under which the so-called boundary layer equations permit periodic solutions." Since this is difficult, Karman has to resort to a rough approximation. The results are then found to be in fairly good agreement with the facts. Two objections have been made to this theory,—(1) that the elements of the phase space are assumed to have the same weight, which cannot in any way be substantiated, and (2) that if the Navier-Stokes differential equations are taken to be valid for the turbulent flow, then the function ψ must satisfy a differential equation of the fourth order, which cannot be the case.

In 1929 Burgers applied the methods of statistical mechanics to the turbulence problem in an entirely different way from that of Karman. The completely turbulent state of flow in two dimensions between two parallel walls is again considered. The restriction to two dimensions may introduce special features, as it appears quite likely that in the phenomena of ordinary turbulence vortices which have their axes parallel to the direction of the mean flow play an important part. Two possible ways of investigating the problem in three dimensions are being explored at present.

Burgers starts with the Navier-Stokes hydrodynamical equations and following Reynolds and Lorentz breaks up the motion into the continually fluctuating relative motion and the mean flow. Instead of the usual condition of constant total energy, Burgers uses the dissipation condition, which states that the relative motion obtains every second just as much energy from the mean motion as it loses in the same time in consequence of the viscous nature of the fluid.

In applying the methods of statistical mechanics to the problem particular " objects " must be chosen to describe the various states of flow. By introducing a quadratic space lattice, the state of flow can be fully specified by the values of the stream function for the relative motion at the lattice points. The velocity components are then represented by certain difference equations.

An N-dimensional phase space is now introduced with the values of the stream function at the lattice as co-ordinates. The various states of the relative motion at a series of instants with equal intervals of time between them are represented simultaneously by a number of points M in the phase space. When M is sufficiently large it is apparent that mean values with respect to the time can be calculated by using the points in the phase space.

This space is divided up into a number of small N-dimensional cells and the numbers of points in these cells are denoted by n_1, n_2, n_3, . . . The statistical probability W of the ensemble of fields of flow considered is then obtained by analogy with the kinetic theory, and is given by the usual formula $W = M!/n_1!n_2!n_3! \ldots.$ The hypothesis is then used that an appropriate set of points in the phase space enabling mean values to be calculated, which also satisfies the dissipation condition and the boundary conditions, makes W a maximum. By using this idea and the equation for the mean motion, an expression can be found for the distribution function. After a lengthy and difficult calculation in which very rough approximations have to be made, Burgers succeeds in finding a lower limit 0·0007 for the resistance coefficient, which is of the same order of magnitude as the values found experimentally. An approximate formula is also deduced for the distribution of the velocity of the mean motion over the breadth of the channel. This gives a velocity in the axis 1·25 times the mean velocity of flow, in contrast to the known value 1·1.

Since the theory is modelled on that of a conservative system, Burgers tried to prove the validity of Liouville's theorem for his phase space, but he only succeeded in proving the theorem for the case of an ideal fluid.

Tollmien has discussed this theory and he deduced, in conjunction with other facts, that the velocity fluctuations with wave lengths of order of magnitude of the lattice spacing preponderate to an unallowable degree. This tends to show that the introduction of the point lattice into the continuous fluid cannot be justified, and Tollmien recently proposed other conditions of control.

In 1933 Burgers published four papers in which a point of view suggested in his last paper of 1929 is elaborated. The stream function ψ is developed according to the normal solutions of a differential equation of the fourth order, which is derived from the distribution function. The co-ordinates which serve as a framework for the construction of cells in the phase space, are the amplitudes with which the various normal solutions occur in the development

Burgers now considers only quantities defined for instantaneously observed modes of motion. The space between the walls is observed at M instants, separated by equal intervals of time, M being a large number. It is assumed that all the possible modes of motion under consideration are classified and of equal weight. The sequence of M pictures obtained in the above way then defines a microscopically described state of the system. The various modes of motion are numbered consecutively 1, 2, 3, . . . and the number of times n_1, n_2, n_3 . . . each one of them occurs in the sequence of M pictures can be determined. By the usual argument the number of differently arranged sequences corresponding to the given set of numbers n_1, n_2, n_3, . . . is found to be $W = M!/n_1!n_2!n_3! \ldots$

The conditions to be satisfied are the shearing force and the dissipation conditions. By introducing the rules for the mean values, the relations from which the distribution function is derived are obtained. This function is :

$$\overleftarrow{n}_m = \exp\left(c + \beta\int dy\left[R^{-1}\frac{\partial\bar{u}}{\partial y}\frac{d\lambda}{dy} - R^{-1}\left(\frac{\partial\bar{u}}{\partial y}\right)^2 - \overline{u'v'}\frac{d\lambda}{dy}\right.\right.$$

$$\left.\left. R^{-1}\overline{\left(\frac{\partial u'}{\partial y} - \frac{\partial v'}{\partial x}\right)^2}\right]_m\right)$$

where c and β are constants, and λ is an arbitrary function. The square bracket denotes that the quantity must be calculated for the mode of motion numbered *m*. Since this exponent can be divided into two distinct parts, one depending on the mean motion and the other on the relative motion, it is possible to discuss these motions separately.

The theory is developed by investigating which stream function ψ gives a minimum value to the fraction .

$$-\int dy\, \overline{u'v'}\, \frac{d\lambda}{dy} \Big/ R^{-1} \int dy \overline{\left(\frac{\partial u'}{\partial y} - \frac{\partial v'}{\partial x}\right)^2}$$

For this purpose Burgers writes : $\psi = \chi_1(y) \cos \alpha x + \chi_2(y) \sin \alpha x$, which function occurs in the theoretical researches on stability. This leads to a consideration of the solutions of the fourth order equation :

$$\chi^{IV} - 2\alpha^2\chi'' + \alpha^4\chi - i\,\alpha R A(\lambda'\chi' + \tfrac{1}{2}\lambda''\chi) = 0,$$

where $\chi = \chi_1 + i\chi_2$ and dashes denote differentiation with respect to y.

Expressions are obtained for $-\overline{u'v'}$ and $\overline{\left(\frac{\partial u'}{\partial y} - \frac{\partial v'}{\partial x}\right)^2}$ by introducing the complete stream function for the mode of motion numbered *m*, in terms of χ. Further development of the theory depends on the assumption of a plausible form for the function λ, so that the differential equation may be solved. This leads to a hypergeometric equation.

By applying the same general statistical principles to the distribution of the mean motion the formula . $\overline{\bar{u}} = \frac{1}{2}d\lambda/dy - 6y$, is eventually obtained for the mean motion. Using the ideas of the boundary layer and complicated mathematical analysis Burgers obtained the value $C > 0{\cdot}0074$ for the resistance coefficient. These values are somewhat higher than the experimental values.

Although the results obtained by Burgers' theory are not yet satisfactory, there is no doubt that an important step forward has been made. The form in which the statistical method has been used can only be justified by its apparent simplicity, and is modelled on the statistical treatment of a conservative system In its present form the method leads to a divergence in the dissipation, but it is hoped to overcome this difficulty by further researches into the statistics of dissipative systems.

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SOME BIOLOGICAL AND ECONOMIC ASPECTS OF THE GALL MIDGES

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Part I. – Introductory and Biological

Introduction

The gall midges (Cecidomyidæ) comprise a family of delicate flies, second to none in their variety of form and abundance. Small in size, varying from under 1 mm. to about 6 mm in length, they may be most readily recognised by their antennæ and the simple venation of their wings. Most of them in their larval stage cause malformations or galls on plants and hence the name that has been applied to them

Historically they rank as old stagers, Pliny having described a gall formed by one of them on beech leaves. Further references to their galls can be found in the literature of the sixteenth, seventeenth and eighteenth centuries, but with few exceptions no mention is made of the insects producing these galls. Réamur (1736–40) was the first to describe some larvæ and Linnæus (1758) was the first to describe one of the perfect insects. Other old writers, including De Geer, Schranck, Geoffroy and Fabricius, described other kinds. In 1803 Meigen recognised them as a distinct group and created the genus *Cecidomyia* (*κηκιδος* gall, *μυια* fly) to include all such forms as were then known. Notwithstanding the initial monographs of Bremi (1847), H Loew (1850), and Winnertz (1853), and a succession of many writers such as Laboulbène, F. Loew, Meinert, B. Wagner, Karsch, Mik, Riley and Lintner, the knowledge of gall midges was very fragmentary until the last forty years. Attention had been focussed on the galls, and their producers had been largely either forgotten or ignored.

Then arose an international triumvirate—Rubsaamen, Kieffer and Felt—to whom the insect was more important than the gall and whose names will for ever be associated with the knowledge of gall midges. Tavares should also be linked with the big three of modern times. These four workers, with the possible exception of Kieffer (see his *Monographie des Cecidomyides d'Europe et d'Algérie*, 1900), turned their thoughts chiefly if not wholly towards the systematics of the Cecidomyidæ. Their work, represented by series of monographic papers, is of the utmost importance. They have laid the basis of a lasting classification. Besides these workers, several students of the group have paid special attention to such forms as are of outstanding economic importance, for example the gall midges of cereals were studied by Marchal; and all the time there have been numerous investigators who have been describing new malformations due to these insects. The standard works on galls are those of Houard, who has written a series of volumes dealing with the galls of Europe, Asia and Oceania as well as Central and South America, and that by Felt on insect galls of America.

Geologically, the oldest species known is one of the Secondary period found in English Purbeck. Several species which existed in the Tertiary period have been found in Baltic amber (Eocene) and in America representatives have been discovered in Oligocene beds and in Pleistocene swamp deposits. In the Quaternary period, a species has been found in copal of Africa.

Geographically, the distribution of these insects is world-wide, a representative being recorded even from New Caledonia. But while the European and North American fauna are comparatively well known, those of Australia, New Zealand, India, Africa and many other places have been very incompletely studied.

Numerically, the family is large. The number of species now described is about 3,500 belonging to about 490 genera. The proportion of genera containing only one species is considerable and probably more knowledge of the family will enable better groupings to be made in the future. It is perfectly safe to assume however that there are many species awaiting discovery. Only about 30 species are recorded as occurring in New Zealand, about 100 each in Australia and India and about 150 from Africa as compared with 900 from the United States of America and about 500 from Great Britain.

It is the purpose of this article to illustrate the diversity of habit exhibited by the gall midges and their economic importance and to follow this by an account of the various methods which have been found effective in their control. The diversity in their structure can be appreciated by reading the works of Felt (*Key to Gall Midges, N.Y. State Museum Bulletin 257*, 1925) and Kieffer (*Genera Insectorum*, fascicule 152, 1913).

MORPHOLOGICAL V
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resultant larvæ, and also by causing
rather distasteful plants, it has been
adults with a low number of antennal
hand by allowing the midges to ovipos
considerably larger midges possessing a
of antennal segments have been obtained.
individuals under bad conditions immediately
duction of small midges with a low number of
Such variation in antennal numbers obtains only
which do not have the generalised numbers of seg
food affects the size in all species. This may be a
the plasticity of certain species and subsequently ma
on the evolution of species.

UNISEXUAL FAMILIES.—Normally reproduction in
midges is sexual and although parthenogenesis has been
to occur this has never been substantiated. In a few in
however, unisexual families are the rule. In addition to
duction by the adults pædogenesis, which is the multiplication
individuals by larvæ, takes place. But this phenomenon will
discussed later.

The result of sexual reproduction as suggested by the chromosome theory is that both sexes are produced in approximately equal numbers. Nevertheless it has been shown by Hindle, when dealing with the common louse of man *Pediculus humanus*, and by Metz, whose studies on the fungus gnats of the genus *Sciara* deserve far greater attention than they have been given, that some insects although reproducing sexually give rise to four types of broods. These types are (1) entirely male, (2) entirely female, (3) male

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[1] *Zoo. Soc. London* for *1932*, 1932, 323–34.

ating, an
minating. Me
ny of a single female-
all-male families, 25 per
le the remaining 50 per
exes are represented. Of
males in the majority and
male does not directly affect
with which pairing takes place.
ndicate that there was selective
emale. But Metz has since shown
subtle and complicated state of
echanism. The possibility of non-
ggs does not enter into the question.
so much interest, especially as in some
r types of families have been observed.
vidson have obtained all-male families,
ixed families in *Frankliniella insularis*,
ase the all-male families were derived from
resumably the males in the mixed families
ed eggs as the all-female families were derived
ales.

on of unisexual families, as they are called, has
to be the rule in about six species of the genus
nd one species of the genus *Thomasiniana* among
ges.[1] Here again individual females of an all-female
give rise to any one of the four types of family. The
g feature of such sexual reproduction is that it usually
brother and sister mating. The result of this should be
l to the race as unwanted injurious recessive characters
t appear so frequently.

In other species, while unisexual families are not the rule, there
a significant departure from the 1 : 1 ratio irrespective of per-
entages. In still other species the males appear to be in the minority as a normal occurrence giving a sex ratio of about 25 : 75.

FLUCTUATION IN INSECT NUMBERS.[2]—For a variety of reasons the gall midges are particularly suitable for population studies. In the first place it is quite possible to obtain large numbers of a particular species with a minimum amount of trouble. For example, from 120 heads of Meadow Foxtail grass from Dorset over 21,000 larvæ of *Contarinia merceri* have been obtained. Another reason is that while some species are univoltine, others have several broods a year. However, it is only of recent years that such studies have been made and already several interesting results have been obtained.

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[1] *Jl. Genetics*, **24**, 1931, 225–34.

[2] *Jl. Animal Ecology*, **1**, 1932, 12–31, and 191–2; *ibid.*, **2**, 1933, 98–108.

The rapidity with which a pest can increase and decrease has been demonstrated ; in the space of a year or one generation the numbers of *Contarinia tritici* larvæ in 500 ears of wheat rose from 2,200 to about 19,300, and in another year a decrease of the same magnitude has been observed.

There are indications that weather conditions are of greater influence than natural enemies and parasites. In one year when the wheat ears burst their sheaths a fortnight earlier than usual, the midges emerged about three weeks before their normal date. As the midges lay their eggs on the newly emerged wheat-ears and are short lived, one would have expected that in such a season there would be a great reduction in the infestation of the crop. This is exactly what happened In the case of *Sitodiplosis mosellana*, one of the wheat-blossom midges, the number of larvæ found in 500 ears of wheat was just over 300 compared with about 3,000 the previous season. No such violent reduction in numbers has yet been found to be directly due to the action of parasites.

In another case, owing probably to the differential effect of weather on the midges and their parasites which caused the parasites to emerge before the host midges, an outbreak of the midge has been noted. From an initial population of nearly 1,600 midges, obtained from 100 heads of Meadow Foxtail grass, the population rose to nearly 5,000 in the same sized sample. The numbers of parasites likewise fell enormously. A similar increase has been seen in another species of gall midge, *Rhabdophaga heterobia*. This midge is multivoltine, and usually about 1,500 midges have been reared from 500 galls as well as about 1,500 Hymenopterous parasites. In one particular year, however, about 2,800 midges were bred and only about 400 parasites. Thus while the total insect population remained about constant the parasitism fell from about 50 per cent. to about 13 per cent. In another year owing to drought affecting the larval food supply the total population fell from the 3,000 mark to about 1,000, but in this case the relative numbers of midges and parasites were not altered.

Another peculiarly interesting fact has arisen regarding the parasitism of a multivoltine species, *Dasyneura pyri*. Samples of full-grown larvæ were obtained periodically from a very restricted area in Devon. For two years not a single parasite was recorded ; in the subsequent four years parasites appeared but not in every generation of the midges. Three times were they present in the overwintering generation and three times were they present in the second generation of the particular year, but not once were they recorded from the first generation of a year. Is this an instance where the parasite has not yet become perfectly adjusted to its host's life-cycle?

PARASITES AND ENEMIES.—Knowledge of the Hymenopterous parasites and other enemies of the Cecidomyidæ is still fragmentary, but Marchal has studied several parasites in detail including the Proctotrupid *Inostemma piricola* and the Platygaster *Trichacis remulus*. Birds, such as tits which attack the galls of *Rhabdophaga heterobia*, and bugs belonging to the family *Anthocoridæ*, for example *Anthocoris nemorum* which sucks the larvæ, pupæ and emerging adults of *Dasyneura pyri*, are to be numbered among the enemies. A vast field of research still lies open in this direction.

PÆDOGENESIS.—The production of larvæ by mother-larvæ, as they are called, has been known for a long time to occur in the subfamily *Heteropezinæ* of the Cecidomyidæ, but now this phenomenon has been shown to occur in addition in another subfamily, the Lestreminæ.

Harris[1] and Gabritschevsky,[2] two recent workers on pædogenetic forms, have shown that the larvæ of *Miastor* and *Oligarces* have three forms. There are (*a*) typical white pædogenetic larvæ without a sternal spatula or breastbone (anchor-process); (*b*) yellow pædogenetic larvæ without a sternal spatula, the significance of these forms is obscure; and (*c*) pupa-forming larvæ which possess a sternal spatula and which develop into adults. The adults mate normally and produce the pædogenetic larvæ. The occurrence of these three larval forms has been shown to depend on environmental conditions. Only one sex is derived from one mother-larva.

LARVAL FOOD HABITS.—The larvæ of gall midges exhibit an amazing diversity in their food habits and a close adaptation to their particular food, both of which typify the high degree of specialisation reached in the family. They can be primarily classified into two groups: (*a*) the phytophagous forms and (*b*) the zoophagous forms.

PHYTOPHAGOUS LARVÆ.—The great majority of gall midges come into this group which can be subdivided into those forms which cause galls, those forms which live in galls but do not cause them, and those forms which live on plants but neither cause galls nor live in them.

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[1] *Psyche*, **30**, 1923, 95–101; loc. cit., **31**, 1924, 148–54; *Biol. Bull.*, **48**, 1925, 139–44; *C. R. Soc. Biol. Paris*, **88**, 1923, 256–8; *Proc. Nat. Acad. Sci.*, **9**, 1923.

[2] *Bull. Soc. Ent. France*, 1928, 75–9; *Bull. Biol. Fr. Belgique*, **62**, 1928, 478–524; *Archiv. Entwick. Mech. Org.*, **121**, 1930, 450–65.

Although almost every part of the plant, including the root, seems liable to gall formation, most of the galls are formed on the soft parts such as the buds, leaves and fruit. Indeed Felt has estimated that 70 per cent. of the American gall midges form such galls, while only 30 per cent. form galls on the harder parts such as the stems. In addition, while most plants maintain one or more kinds of gall midge, there are striking groupings of allied species of midge and allied plant species. For example, species of the genus *Rhabdophaga* are, with few exceptions, restricted to the genus *Salix*. This is further evidence of the great plasticity of the gall midges in that certain genera have adapted themselves to various host plants.

There are relatively few forms which live in galls made by other insects but do not cause them. Most of these are to be found in the galls of other Cecidomyidæ; but several live in galls produced by Cynipids, others live in galls produced by Muscid flies, and at least one inhabits the galls made by a beetle. The exact mode of feeding of these larvæ is not clear, but they are certainly not predaceous.

The second largest group of the gall midges as a whole feed on plant tissue but do not inhabit galls. Many live in the florets of grasses and cereals, feeding on the developing seed. Other forms feed on resin, under the bark of trees, in decaying vegetation and on newly felled timber. It is of interest to note here that in recent years there has been an isolated case of gall midge larvæ being so numerous on the bodywork of a new motor-car (*i.e.* coming out of the wood) that the firm thought it desirable to burn the coachwork and presumably compensate the owner. Another group feeds on rusts and other fungi. There is also a coprophilous group which feeds on the excreta of various fly and beetle larvæ under bark and in old birds' nests.

ZOOPHAGOUS LARVÆ.—These gall midges can be conveniently divided into those which are internal parasites and those which are predaceous. In the first group the best known example is *Endaphis perfidus* whose larvæ live inside an aphid of Sycamore leaves. There is also another midge whose eggs are laid on the wings of the apple sucker *Psyllia mali* and whose larvæ are internal parasites of the *Psyllia*. There is need here for some detailed morphological studies as to the structural adaptations involved. The second group comprises a number of forms which are predaceous on other gall midge larvæ, those which feed on mites and red spider, and those which feed on greenfly (Aphididæ), scale-insects and mealy-bugs (Coccidæ), white-fly (Aleyrodidæ) and thrips (Thripidæ). Recent compilations have shown that there have been described about 50 species whose larvæ are known to feed on aphids, about 40 species on mites, about 40 species on scale insects and two on whitefly, while at least two are known to eat thrips.

Choice and Restriction of Host Plant.—It is well known that gall midges as a rule are very limited in their range of host plants and are intimately associated with them While some authorities have claimed that certain midge species live on a number of plant species, there are many indications in the opposite direction. These claims have usually been based on similar gall formation, or the larvæ found in the galls or in a few cases only on the adult midges. Any one who has had any experience of the gall midges as a group can at once point out the danger attendant on any one of these methods of identification, if used alone. The gall is the reaction of the plant to the presence of the insects and can vary considerably as will be shown later. No one would identify a midge species on larvæ unless previous experience had indicated that it could not possibly be mistaken for any allied species. And lastly midge species are exceedingly difficult to separate even in the adult stage. The more individuals one examines, the wider the variation seen. This applies to any one dealing with hundreds of either sex, not a paltry five or six.

The wheat-blossom midges have been found by experiment to be able to oviposit and live successfully on other plants besides wheat, *e.g. Contarinia tritici* on Couch grass, Slender Foxtail grass and Rye. Other host plants are also recorded for this species. Yet other species of the genus *Contarinia* seem restricted to one species of grass. Thus it appears possible that there are certain plastic species able to live on several host plants while other species are evolving and becoming restricted to particular plant species.

Investigations on willow midges[1] have shown that one midge, *Rhabdophaga heterobia*, will attack all varieties of *Salix triandra* but will not form galls on *S. viminalis*, *S. purpurea*, *S. purpurea* x *viminalis*, *S. viminalis* x *triandra*, *S. alba* var. *vitellina*, *S. alba*, *S. cærulea*, *S. fragilis*, *S. nigricans*, *S. americana* nor on *S. repens*. Sometimes eggs are laid on some of these species but no galls result. Another species, *R. terminalis*, lives on two willow species

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[1] *Ann. App. Biol.*, **18**, 1931, 75–82; loc. cit., **19**, 1932, 243–52; *The Times*, February 6, 1933; and unpublished data.

S. cærulea and *S. alba* var. *vitellina*, but chooses the former when available. It will not live on *S. triandra*, *S. viminalis*, *S. purpurea* nor *S. viminalis* x *triandra*. The possibility of midge strains must not however be ignored, especially in view of Painter's work mentioned below.

A few studies have recently been made with reference to the varietal resistance of plants to particular midge species. For example, Painter, Salmon and Parker [1] have studied the resistance of wheat to the Hessian fly. Among interesting data five characteristics which affect the resistant qualities are discussed. These are (1) a decided difference in the number of flies which develop on the several varieties, (2) a kind of tolerance permitting the fly to develop without material damage to the plant, (3) the ability of some varieties of wheat to develop fly better and faster than other varieties, (4) the ability of wheat to develop tillers after infestation, and (5) the stiffness of straw in relation to Hessian fly damage. Painter has also found distinct biological strains of Hessian fly, one in the hard-wheat belt of central and western Kansas and one in the soft-wheat belt area of eastern Kansas.

GALL AND INSECT SPECIES.—Students of galls often identify gall midges by the kind of gall produced. There are many dangers in such a procedure. The state of growth of the plant at the time of oviposition by the insect and immediately subsequent to this has a great influence on the gall produced. For example, *Rhabdophaga terminalis* lays its eggs on the terminal buds of *Salix cærulea* and the terminal leaves remain in a curled and crinkled state instead of unfolding naturally. The gall is at first reddish but later turns black. Sometimes, however, when the development of the eggs is retarded by the inclement weather more than the growth of the plant, the shoot goes on growing away from the leaves on which the eggs were originally laid. In such cases these latter leaves open and the larvæ form blister-like galls along the midvein of the leaf.[2] In much the same way, the galls of *R. heterobia* vary from a type of gall which is merely a swollen lateral bud to the type in which it is a perfect rosette of small leaves.

A single species of gall midge frequently produces several distinct types of galls. The pea midge,[3] *Contarinia pisi*, causes malformed flowers, clusters of leaves and flowers with the flower-stalks absent or nearly so, and the larvæ also live inside the pods.

Rhopalomyia hirtipes,[4] an American species, produces an apical gall on stunted shoots of *Solidago* but it also causes a subterranean gall on the buds from the root stocks.

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[1] *Kansas Agric. Expt. Station, Tech. Bull. 27*, 1931.

[2] *Ann. Appl. Biol.*, **19**, 1932, 246–7. [3] *Jl. Min. Agr.*, **34**, 1927, 159–61.

[4] *N.Y. State Mus. Bull.*, **257**, 1925, 24.

Furthermore several species of gall midges can be reared from the same gall and from the same kind of gall on one or more plants. In short then, it is hazardous in many cases to identify gall midge species using the galls or the galls and larvæ only until much more biological work has been done on the midges themselves and the interrelationships of plant reactions and midge attack.

Such then are the bionomics of the gall midges.

Part II.—Economic Importance and Control

ECONOMIC IMPORTANCE

One would expect that a large family of flies, in which "adaptive radiation" is so pronounced, would be of considerable interest to the economic entomologist. Such is the case. The well-known Hessian fly is perhaps the best example because of the immense loss it has caused in the past to wheat growers in the United States of America. The pear midge, *Contarinia pyrivora*, whose larvæ destroy young pears, is better known in this country. The variety of crops which suffer from gall midge attacks is almost infinite: among field crops may be mentioned swedes and cauliflowers; wheat and sorghum among cereals; pear, cranberry and tomato among fruit; violets and newly budded roses among horticultural plants; grapes, chrysanthemum and *Hydrangea* among greenhouse plants; and box among ornamental shrubs. Among willow growers both for the basket and the cricket-bat industries, midges are again responsible for losses. Hop growers also suffer from the "Strig maggot." Seed production, especially that of red clover and grasses such as meadow foxtail, cocksfoot and timothy, is seriously affected.

Besides the many injurious species of gall midges there are some of beneficial importance although they have only received scant attention up to the present. The prickly pear midges do so much damage that Felt has stated that they would be regarded as beneficial in an area where these plants are undesirable. Felt has also stated that *Dasyneura gibsoni*, whose larvæ live in thistle-heads, is regarded as a useful check on the spread of thistle seed. In addition, many of the predaceous gall midges feed on injurious insects and mites and without doubt form a useful link in the faunal "chains" they frequent. As an example *Therodiplosis persicæ*, whose larvæ feed on red spider in glass-houses, can be mentioned.

CONTROL MEASURES

The chief methods of controlling insect pests are (1) cultural, (2) biological, (3) physical such as the use of heat, (4) chemical, (5) trapping such as grease banding, and (6) legislation. The first and last of these may be considered as preventive, while the fourth can only at best be palliative. Biological control may be either preventive or palliative. While there is a very real difference between the prevention and the cure of insect outbreaks, it is sometimes difficult to draw hard and fast lines of distinction between the methods of control. A measure may be partly preventive and partly remedial.

CULTURAL METHODS.—Such methods of control are by far the most important in thwarting the ravages of gall midges and it will be seen that with few exceptions any control measures known to be useful against these insects are cultural in one form or another. Nearly all such methods aim at destroying the normal sequence of events in the life history of the pest and its food or host plant.

The use of clean seed is fast becoming more and more a *sine qua non*. It is almost certain that one of the meadow foxtail midges, *Dasyneura alopecuri*, originally a European pest, was introduced into New Zealand, where it is now a pest, through infected seed. Other midges that could be distributed with the seed are the pea midge (*Contarinia pisi*), the red clover midge (*Dasyneura leguminicola*) and the pod midge (*D. brassicæ*).

The date of sowing is very important in the case of attacks by at least one midge. A crop should be sown early or late according to the life history in the particular locality of the midge to be avoided. In America the sowing of wheat is delayed until the "safety" or "fly-free" date, which is when the main flight of the Hessian fly is over. This method of control is based upon the fact that midges are short lived and that if a crop be sown after the main flight of a brood the crop should escape most of the injury to which it is liable under normal conditions. The "safety" date originally had to be determined in the first place separately for each locality by direct observation. Such observations have been eliminated to some extent by the judicious use of the bioclimatic law which was developed by Hopkins. This states that all other conditions being equal, the variation in the date of a periodic event in the seasonal activities of a plant or animal in America, north of Mexico, is at an average rate of four days for each degree of latitude, five degrees of longitude and 400 feet of altitude—earlier southward, westward and descending in the spring and early summer, and later in the reverse directions in late summer and autumn. It has been shown that wheat sown before the "fly-free" date was heavily infested, but that when wheat sowing was delayed until after this date very little infestation occurred providing that there was no volunteer or self-sown wheat.

The time of harvesting has been shown to have a marked effect on infestation by certain midges. In order to grow a crop of red clover seed it is the practice to cut the first growth green and so starve out the larvæ of the red clover seed midges which are in the green heads of the clover. The subsequent growth of clover is free from midge attack as the direct result of this early cutting and so a good seed crop is obtained. Parallel to this cutting of a crop in order to kill larvæ already present, there is another method. This is to delay the crop flowering until the main flight of the particular midges involved is past and so prevent the larvæ ever getting on to the crop. This latter method has been shown to work in the case of the meadow foxtail midges. These midges normally prevent a good seed crop. By grazing or cutting the grass in order to prevent it from flowering until after the main flight of the midges, the loss of seed has been lowered in an experiment from 80 per cent. to 11 per cent.

Manurial treatment of a crop is sometimes beneficial in promoting rapid growth which results in a potential insect attack being frustrated. As an example of this it can be stated that whereas the normally slow growing cow-parsnip is very prone to attack by the midge *Macrolabis corrugans*, the cultivated parsnip is only rarely susceptible. In experiments although the midge oviposited time after time on the unopened leaves of the cultivated parsnip, only when the plants were developing slowly did the larvæ establish themselves. In every other instance the leaves uncurled rapidly and exposed the eggs of the midges to direct sunlight in which they perished. If the larvæ had had time to hatch the leaves would never have uncurled and so the larvæ would have survived.

The judicious use of manures and soil cultivation has also been shown to reduce the numbers of gall midge larvæ in the soil. For example there is the work of Klee[1] which has shown that top dressing in April preceded by ploughing in the autumn materially reduced attacks by the two wheat-blossom midges, *Contarinia tritici* and *Sitodiplosis mosellana*. Deep ploughing alone was distinctly beneficial but not so good as when followed by a top dressing.

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[1] *Die Ernährung der Pflanze*, **18**, **1932**, **323–4.**

A well-known practice to avoid pear midge attack is intensive cultivation between the rows of pear trees during the summer as a control against midge attack the following year. This cultivating and hoeing has on occasion [1] been highly successful. It is designed to destroy the larvæ in the soil after they have left the young pears.

The general cleanliness of crop is very important in the control of several midges, *e g.* the Hessian fly, the swede midge and the wheat-blossom midges. Weeds must be discouraged as they provide alternative host plants for these midges. Self-sown wheat and crop remnants also help the midges in maintaining their numbers.

Rotation of crops can *a priori* be expected to keep down midge numbers and make things more difficult for them, especially in view of the fact that gall midges are not strong fliers.

The use of early or late varieties of plants as well as resistant varieties has been shown to be useful in the case of the Hessian fly, the wheat-blossom midges and willow midges. It is here that the entomologist must work hand in hand with plant breeders.

Trap crops have been recommended as a means of controlling the swede midge and the Hessian fly. Handpicking is always helpful when the crop is on a small scale, for instance in small orchards against pear midge attack.

There is an effective old method of preventing attack by *Thomasiniana oculiperda*, the red bud borer on bush roses. This midge is in the habit of laying its eggs in the slit between the inserted bud and the stock of newly budded plants such as roses and apples, thereby preventing the bud from taking. Thousands of roses have been lost by this happening. The method is to earth up the bushes so that the soil is above the level of the buds, in this way the adult midges are unable to lay their eggs. The soil is levelled again in the autumn after the buds have taken. This method has proved a distinct success in surface buddings.

The custom of spreading grease, such as vaseline,[2] over the tie as a precaution against the red bud borer when budding roses and apples may be considered as cultural in so far that it could and should be a normal practice in the procedure of budding. After the bud has been inserted, it must be tied in position and then a thorough coating of the grease should be applied. This prevents the midges from ovipositing. The actual reason for this avoidance by the midges is probably due to the stickiness, although it may be partly chemical. Such a method cannot be considered as trapping because the midges are not caught on the grease.

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[1] *Ann. Rept. II Supplement, East Malling Res. Stat.*, 1931, 196–7.

[2] *Jl. Min. Agric.*, **37**, 1930, 59–63.

Biological Control.—This method of pest control is as yet untried against gall midge attacks. But on the other hand there is a project under discussion whereby an attempt will be made to use a midge, *Therediplosis persicæ*, against red spider in glass-houses.

Chemical Control.—Straightforward applications of insecticides and fumigants have so far proved of little avail against insects of this family, with two exceptions. In America the boxwood leaf miner (*Monarthropalpus buxi*) is controlled by spraying with nicotine which penetrates the leaf tissue, and the chrysanthemum midge (*Diarthronomyia hypogæa*) is also controlled by fumigation and the use of sprays. But in the latter case the additional use of clean cuttings from the stock plants is most desirable.

Legislation.—The use of legislation is essential in order to prevent the entry into new areas of proven pests. For example the boxwood leaf miner was introduced from Europe into the United States of America. Another instance was the recent outbreak of the chrysanthemum midge in a few English glass-houses. This was obviously a case of introduction from an infected area. Luckily it was prevented from establishing itself as a pest by the immediate spraying and burning of the infected plants whereby it seems to have been entirely wiped out.

To sum up, the most efficient and desirable ways of controlling and preventing gall midges doing great damage are those which can be grouped under the heading of Cultural Methods.

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS.

By E. Maitland Wright, M.A., D.Phil.,
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Waring's Problem—Two recent publications by Professor L. E. Dickson of Chicago make this a topical subject; of these, one is a set of tables of representations of all integers up to 300,000 as sums of fifth powers, and is reviewed elsewhere in this number, the other is a paper in the *Bulletin of the American Mathematical Society* (**39**, 1933, 701–27).

It was conjectured by Waring (*Meditationes Algebraicæ*, 1770) that every positive integer is the sum of at most 9 positive integral cubes, also of at most 19 fourth powers, and so on. In the same year Lagrange had proved that every integer is the sum of four integral squares The next theorem of this type was proved by Liouville, who showed that every positive integer is the sum of not more than 53 fourth powers. This limit was reduced successively to 41, 39, 38 and finally to 37 by Lucas, Fleck, Landau and Wieferich respectively.

Hilbert (1909) proved the important theorem that every positive integer n is the sum of at most $g(k)$ positive integral k-th powers, where $g(k)$ depends on k but not on n. Hilbert's proof has since been considerably simplified.

Wieferich proved that every integer is the sum of 9 cubes and Landau that every sufficiently large integer is the sum of 8 cubes.

The next great step forward in this field was made by Hardy and Littlewood, who developed an analytic method to deal with the problem. They proved that every sufficiently large integer is the sum of at most $G(k)$ positive k-th powers, where $k > 4$ and

$$G(k) \geq (\tfrac{1}{2}k - 1)2^{k-1} + k + 5 + \zeta_k,$$

the number ζ_k being the greatest integer less than or equal to

$$\frac{(k-2)\log 2 - \log k + \log (k-2)}{\log k - \log (k-1)}.$$

Gelbke applied a method due to Vinogradoff to simplify the proof of this theorem.

In his paper referred to above Dickson gives an account of a method developed by himself and his pupils to determine an upper bound for $g(k)$. The method may be illustrated by an account of the work for the case of fifth powers. Using the Hardy-Littlewood-Gelbke method, James has proved that every $n > L$ is a sum of not more than 54 fifth powers where $\log L = 34251$. It remains to show that every integer less than or equal to L is a sum of not more than 54 fifth powers. The number L is, of course, enormously large and it is quite impossible to calculate representations for all integers up to L. However it was found by direct calculation that all numbers from 98,605 to 191,263 are sums of 16 fifth powers; all from 191,264 to 470,348 are sums of 15; all from 470,349 to 786,159 are sums of 14 and all on to 839,000 are sums of 13. The tables from which these results were deduced are those published by the British Association for L. E. Dickson (see above). "A further table was made from $A = 839{,}000$ to $929{,}000$ and used to prove that all numbers from A to $B = 1{,}466{,}800$ are sums of 13 fifth powers. Another table from B to 1,600,000 showed that 12 powers suffice except for seven numbers which seem to require 13. It was used to prove that 13 powers suffice from B to $C = 3{,}470{,}000$. A final table from C to $D = 3{,}600{,}000$ showed that 11 powers suffice except for 13 numbers which seem to require 12. It was used to prove that 12 powers suffice from C to $D + 18^5 = 5{,}489{,}568$." Dickson then applies his "Formulæ of Ascent" to these results and shows without further direct calculation that 54 powers suffice from A to L. Hence $g(5) < 54$.

This outline indicates the tremendous amount of labour involved and makes it appear probable that the real value of $g(5)$ is much lower than 54. It is known that $g(5)$ is not less than 37. The results now published are as follows: $g(4) < 35$, $g(5) \leq 54$, $g(6) \leq 160$, $g(7) \leq 320$, $g(8) \leq 575$, $g(9) \leq 1177$, $g(10) \leq 2421$, $g(12) \leq 10711$. These are all improvements on previous results.

Waring's Problem may be modified as follows. We may consider the representations of n in the form

$$n = \pm m_1{}^k \pm m_2{}^k \pm m_3{}^k \pm \ldots \pm m_s{}^k,$$

where $n, m_1, m_2, \ldots, m_s$ are integers, and try to determine $v(k)$, the least value of s for which such a representation is possible for all values of n. Obviously $v(k) \leq g(k)$. The problem is due to Oltramare, although he considered only the case of cubes. He proved that $v(3) \leq 5$ as follows. If n is any integer, the number $n^3 - n$ is divisible by 6, for

$$n^3 - n = n(n-1)(n+1).$$

Since $n - 1$, n, $n + 1$ are consecutive numbers at least one is divisible by 2 and at least one by 3 ; hence their product is divisible by 6. Then

$$n - n^3 = -6v = 2v^3 - (v + 1)^3 - (v - 1)^3,$$

and so

$$n = n^3 + 2v^3 - (v + 1)^3 - (v - 1)^3.$$

This shows that $v(3) \leqq 5$. On the other hand, it is easy to see that $v(3) > 4$; for every cube has the remainder 0 or 1 when divided by 9 and so numbers of the form $9x \pm 4$ require at least 4 positive or negative cubes to represent them. We see then that $v(3) = 4$ or $v(3) = 5$; which of these is true is as yet unknown.

Vesely (*Casopis pro pestovani Matematiky a Fysiky*, **62** (1933), 123–7) has recently pointed out that $v(2) = 3$. If n is odd, we have

$$n = 2m + 1 = (m + 1)^2 - m^2,$$

while if n is even

$$n = 2m = (m + 1)^2 - m^2 - 1^2.$$

On the other hand the number 6, for example, cannot be expressed by less than 3 squares.

It is not difficult to extend Oltramare's method and one can prove that $8 \leqq v(4) < 12$, $5 < v(5) \leqq 10$, and so on. The most striking result in this direction is that $v(20) < 185$. This may be compared with the known result that $g(20) > 1{,}000{,}000$.

In a recent paper (*Mathematische Annalen*, **109** (1933), 196–9), R. D. James has shown that every sufficiently large integer is a sum of not more than eight pyramidal numbers, that is numbers of the form $(x^3 - x)/6$, where x is a positive integer. Empirical results indicate that a better result is true and that eight should be replaced by a smaller number.

THE FUNDAMENTAL THEOREM OF ARITHMETIC.—In a short note (*Quarterly Jour. of Math.*, **4** (1933), 319), Professor Lindemann has given a new and very simple proof of the fundamental theorem of arithmetic that every integer can be factorised into primes in one way and one way only, apart from the order of the factors.

If the fundamental theorem is not true, let n be the least integer with two essentially different decompositions into prime factors, namely

$$n = p_1 p_2 p_3 \ldots = q_1 q_2 q_3 \ldots$$

Then we shall show that a number $n' < n$ exists for which two different decompositions are possible. This contradicts the hypothesis that n is the least such number and so the theorem is proved.

We may clearly suppose that $p_1 < p_2 < \dots$, $q_1 \leq q_2 < \dots$, and also that no p_i is equal to any q_j (for otherwise these could be removed and we should have found our n'). Hence $p_1 \leq \sqrt{n}$, $q_1 \leq \sqrt{n}$ and, since $p_1 \neq q_1$ one of these must be actually less than $\sqrt{n}$. Hence $p_1 q_1 < n$ and

$$p_1 p_2 p_3 \dots - p_1 q_1 = q_1 q_2 \dots - p_1 q_1$$

is a positive number. Hence

$$p_1(p_2 p_3 \dots - q_1) = q_1(q_2 q_3 \dots - p_1).$$

There are two possibilities:

(*a*) q_1 divides $p_2 p_3 \dots$ Then $n' = p_2 p_3$, smaller by the factor p_1 than n, has two decompositions, one containing q_1 and one not.

(*b*) q_1 does not divide $p_2 p_3 \dots$ Then it does not divide $p_2 p_3 \dots - q_1$. Hence

$$n' = p_1(p_2 p_3 \dots - q_1) = p_1 r_1 r_2 \dots,$$

where no r is q_1, while, since

$$n' = q_1(q_2 q_3 q_4 \dots - p_1),$$

it also has a decomposition in which one prime is q_1. Thus n', which is smaller by $p_1 q_1$ than n, has two different decompositions.

ASTRONOMY. By R. W. Wrigley, M.A., Royal Observatory, Edinburgh.

The Planets.—In his George Darwin Lecture (*Monthly Notices, R A S.*, **93,** No 9) V. M. Slipher took as his subject "Spectrographic Studies of the Planets," and gave a general account of the work done in this connection during the last thirty years at the Lowell Observatory, an institution founded primarily for the intensive study of the planet Mars. Recent advances in spectrography have been mainly due to the enormous improvement in the recording power of photographic plates. At the beginning of the century photography of the red end of the spectrum was practically impossible; with the commercial plates then available λ 6000 was the maximum limit in wavelength, and even with the aid of sensitising dyes it could not be advanced more than another 500 ångstroms towards the red. In striking contrast, it is now possible to record the spectrum to beyond λ 10,500 in the deep infra-red, and in no astronomical field is this gain in photographic efficiency as important as in the study of the planets. The atmospheres of these bodies show a strong selective absorption which increases rapidly in the long wavelengths, and as this tendency persists as far as λ 10,500 it is probable that still more valuable knowledge is awaiting any further penetration into the heat spectrum.

The method followed at the Lowell Observatory is to photograph alongside each planetary spectrogram one of the moon taken as nearly as possible at the same elevation in the sky. The path of the light through the earth's atmosphere is then identical in the two cases, and any observable differences between the two spectra can safely be ascribed to the peculiar action of the planet in question.

Dr. Slipher divides the planets into two main classes. The first group, containing Mercury, Venus, the Earth, Mars and, somewhat strangely, Pluto show but little addition to the lines produced by our own atmosphere , whereas the second, comprising the four giant planets Jupiter, Saturn, Uranus and Neptune show strong selective absorptions peculiar to themselves, which seem to increase in intensity with the planet's distance from the sun.

Mercury is always a difficult object to observe owing to its proximity to the sun, but its spectrograms, extending down to λ 9000, show no indication of the existence of any dark bands. Confirmation is thus given to the general opinion that the low surface gravity of Mercury has long since allowed the complete dissipation of any atmosphere which may once have been present. These results are supported by observations at Mount Wilson, where the spectrum of Mercury in the region λ 7500 to λ 8900 has been studied by Adams and Dunham when the planet was near its greatest easterly elongation (*Annual Report of the Director*, 1933). Comparisons with the spectrum of the sun and of Venus show no differences from the solar spectrum in the forms or intensities of the lines, nor any evidence of the presence of bands. Terrestrial lines of oxygen and water vapour are symmetrical.

The question of the rotation period of Mercury is dealt with by E. M. Antoniadi in the *Journal of the Royal Astronomical Society of Canada*, December 1933. After consideration of all the available observations extending over more than 100 years and including those of Schröter and Schiaparelli, he concludes from his own studies with the 33-inch telescope at Meudon that Mercury presents a constant face to the sun, with rotation period equal to that of its revolution round its primary. Antoniadi remarks on cloud formations which he has observed on the planet's surface, and attributes these to violent dust storms raised by the intense heat to which it is exposed when near perihelion. In the total absence of an atmosphere it is remarkable that Mercury should show more pronounced cloud effects than Mars.

Venus used to be regarded as the planet most closely resembling the earth, at any rate in size and position, and her telescopic appearance and high albedo seemed to indicate her possession of a considerable atmosphere, which, by analogy, might reasonably have been expected to contain quantities of oxygen and water vapour. The spectrograms obtained at the Lowell Observatory show no traces of either of these substances, neither are any bands present in the infra-red. At Mount Wilson (*Annual Report of the Director*, 1932) the infra-red spectrum of the planet has been photographed with a plane-grating spectrograph of 9 feet focal length at the coudé focus of the 100-inch reflector. From these it is concluded that if oxygen or water vapour exist in appreciable quantities in the atmosphere of Venus, it must be at a level below that to which the spectroscopic observations penetrate. Further study of the spectra has however given very interesting results, described by T. Dunham in *Publ. Astron. Soc. of the Pacific*, August 1933. Three new bands well resolved into individual lines have been detected. The spacing of lines in these bands leads to a moment of inertia in close agreement with that determined for carbon dioxide. This identification has been verified by photographing in the absorption spectrum of 40 metres of carbon dioxide at a pressure of ten atmospheres a faint line which agrees closely with the position of the head of the strongest band at λ 8688. The atmosphere of Venus therefore remains a mystery. Carbon dioxide seems undoubtedly present, but oxygen and water vapour must be either absent or hidden beneath a veil as yet impenetrable to our instruments. A series of spectra have been taken at Flagstaff to determine the planet's rotation period. The absence of rotational effects shows that the period certainly cannot be shorter than ten days or a fortnight, and the short period comparable to that of the earth which has found favour with some astronomers in the past seems definitely precluded.

The observation of Mars has always had a prominent place in the programme of the Lowell Observatory, and its spectrum has been repeatedly photographed both there and from the top of the San Francisco Mountains, an elevation of 11,400 feet. It was found that the condition of the air was of more importance than the extra elevation, and that the driest winter nights at the observatory (7,250 feet), gave very satisfactory results, the chief telluric vapour band being almost invisible. The spectrograms of Mars compared with those of the moon provide, in Dr. Slipher's opinion, competent proof of the existence of water vapour in the Martian atmosphere. No bands, however, have been detected in the infra-red. As the surface gravity on Mars is only three-eighths of that on the earth its atmosphere might naturally be expected to be less dense, but to extend to a considerable height above its surface. Evidence for this has been provided at Lowell, where clouds have been observed at a height of about 15 miles, whereas in our own atmosphere they are rarely found above 5 miles.

A search for oxygen in the atmosphere of Mars has lately been prosecuted by Adams and Dunham at Mount Wilson (*Astrophysical Journal*, April 1934). The B band, with its strong widely spaced lines, is well adapted for this purpose. The observations were continued through a period when the radial velocity of the planet with respect to the earth varied from − 13·8 km. per sec. to + 12·6 km. per sec. These velocities are sufficiently high to ensure that the planetary oxygen lines, if present, would be fully resolved from the corresponding terrestrial ones, but, rather surprisingly, no evidence of the presence of such lines could be detected either visually or with the microphotometer. There was also no sign of any asymmetry in the wings of the telluric lines whether the planet was approaching or receding. The conclusion seems to be that if free oxygen exists in the atmosphere of Mars, the quantity above unit area of the surface cannot exceed one-tenth of one per cent. of that above unit area at sea-level on the earth.

Measurements of the radiation from Mars have been made with the thermocouple by Pettit and Nicholson at each opposition since 1924, when the planet was at perihelion, to 1933 when it was close to aphelion. These measures clearly establish the influence of the planet's orbital eccentricity on its climate. The temperature of the sub-solar point varies widely, being only 0° C. when the planet is at aphelion and no less than 27° C. when it is at perihelion, and the mean maximum temperature on Mars throughout the Martian year is approximately 14° C. lower than it would appear to be from measurements made solely at perihelion. Coblentz and Lampland (*Lowell Observatory Bulletin*, No. 85) have found that the intensity of the planetary radiation from the northern hemisphere of Mars is less than that from the southern. This is possibly due to the preponderance of dark areas in the southern hemisphere, while an additional cause may be found in a difference in the atmospheric conditions on the surface of the planet. It is difficult to determine the actual Martian temperatures with accuracy, but there is no doubt as to the wide diurnal and seasonal variations which are experienced. The subsolar point seems to be at least 40° C. higher than the sunrise limb and 10° C. higher than the sunset limb, and there is but little doubt that the temperature, even on the equator, descends well below freezing-point during each night.

The spectra of the four large planets, Jupiter, Saturn, Uranus and Neptune all show strong selective absorptions, persisting far into the infra-red. There seems to be a general increase from Jupiter to Neptune, except that Saturn, possibly owing to its very low density, shows bands but little stronger than those of Jupiter. Uranus and Neptune are too faint to give any record beyond λ 8600, but it is probable that additional absorption bands, increasing in number and intensity, extend far further. The selective absorption is responsible for the distinctive colour of these two planets.

Recent spectrograms of Jupiter and Saturn taken by T. Dunham (*Mount Wilson Observatory, Annual Report*, 1933) show a number of bands between λ 6450 and λ 9100 which have been identified as due to ammonia and methane. In the case of ammonia more than 60 lines have been found in the spectrum of Jupiter, while 18 lines of methane have been identified in the spectra of both Jupiter and Saturn in the unblended planetary band near λ 8640. The ammonia bands are stronger in the spectrum of Jupiter, the methane bands are stronger in that of Saturn. By comparison with laboratory results it is indicated that above the reflecting level in the atmosphere of Jupiter there is the equivalent of a path about 5 to 10 metres long through ammonia at atmospheric pressure. From this a minimum temperature of about 170° Absolute may be assigned to the reflecting level in the planet's atmosphere, for at a lower temperature the vapour pressure of ammonia would be insufficient to support the observed amount of gas when the surface gravity is as high as that of Jupiter.

Slipher calls attention to the very low temperatures which the radiometric observations made at Lowell and Mount Wilson indicate for Jupiter and Saturn. The former at any rate shows such vigorous activity in his surface markings that a much higher temperature seems called for. Slipher suggests that the selective and general absorption in the atmospheres of the giant planets is sufficiently strong to reduce greatly their heat radiations in the region to which our own air is transparent. It is therefore possible that these distant planets may be making the most effective use of their small share of the solar energy. Certainly their spectra indicate that their atmospheres retain the energy of the longer heat waves, and therefore act as blankets, regulating to a considerable extent the temperature of each planet's surface.

Pluto is too faint a body for its spectrum to be studied with present equipments, and no disc is visible even in the 100-inch telescope. During February and March 1933 Baade at Mount Wilson made comparisons of its magnitude with those of stars in

Selected Area 51. The resulting photographic magnitude was found to vary over a range between 15·2 and 15·4, but it cannot yet be definitely stated that Pluto varies in light to this extent. At Flagstaff the colour of the planet has been studied, and it has been found that it is brighter visually than photographically. Uranus and Neptune are distinctly blue in colour, but Pluto is a yellowish planet, resembling Mars and Mercury and possibly standing between the two. As it is impossible to determine its diurnal rotation with the spectrograph the only available method seems to be the photometric study of the reflectivity of its disc. If, as is suspected, different faces of the disc show different reflectivity sufficient to give an appreciable light variation, then the rotation period can be determined, and this matter is now being studied at the Lowell Observatory.

The question as to who may rightfully claim the credit for the discovery of Pluto is not yet settled to everyone's satisfaction. In *Publications of the Astron Soc of the Pacific*, February 1934, William H. Pickering puts forward his own claim to the distinction. His prediction of the longitude of the new planet, published in the *Annals of the Harvard Observatory*, **82,** 49, appeared in 1919 and eventually proved to be only 69′ in error Four photographs were taken at Mount Wilson in December of that year, but although the planet was duly recorded it was not identified owing to its position 4° from the ecliptic in a region very rich in stars. After the discovery of Pluto by Tombaugh at Lowell Observatory the Mount Wilson plates were re-examined, and the planet was then found only about one degree in longitude from the place predicted by Pickering. The latter made use of the perturbations of Neptune in his calculations whereas Lowell relied on those of Uranus. The former were four times as large, and therefore much more easily distinguishable from accidental errors of observation. The mass of Pluto is very much less than the estimate of Lowell, and the perturbations of Uranus were so small that Pickering maintains that his rival's success in predicting the longitude of the new planet to within 7° might almost be said to be accidental. Certainly a comparison of Pickering's own predicted elements with the true orbit of Pluto makes an impressive showing, and it is unfortunate that the planet was not identified on the 1919 Mount Wilson plates. The case bears a distinct resemblance to the discovery of Neptune, and the conflicting claims to priority of Adams and Leverrier.

PHYSICS.

By L. F. BATES, Ph.D., D.Sc., F.Inst.P., University College, London.

THE STRUCTURE OF LIQUIDS.—In a recent public lecture reference was made to some experiments, of fundamental importance on the structure of liquids, in which determinations of the ratio of the velocity of sound to the velocity of light in the liquid had been made. As these experiments have to some extent been overlooked, it seems desirable to discuss them here. Ever since the publication of van der Waals' theory of the continuity of the gaseous and liquid states, fairly definite views on the physical nature of liquids have been held. However, in view of the ordered arrangement of atoms which has been so definitely proved to exist in the case of solids, the possibility that some kind of ordered arrangement in a liquid might also exist has had to be considered.

Now, many experiments have recently been made on the scattering of light by fluids. They fall into two classes. In the one we have the important experiments on the Raman effect, wherein appreciable differences between the wavelengths of the incident and scattered radiations are observed, which can be correlated with the rotational frequencies of the molecules. In the other, we have the experiments on classical or Rayleigh scattering, where the wavelength of the scattered light is the same as that of the incident light except for the very slight changes produced by Döppler effects. When such effects are present, the observed changes in wavelength will obviously depend upon the angle between the directions of the incident and scattered rays. Thus, in the case of a gas illuminated by monochromatic light and viewed along the direction of illumination, only a simple symmetrical widening of the line will be observed in the scattered radiation. For example, Cabannes and Daure (*Comp. Rend*, **186**, 1533, 1928) in an examination of the radiation classically scattered from butane vapour, carried out with a Fabry and Pérot interferometer, found a widening of ·04Å and a displacement of 0·01Å towards the red; the latter observation is not confirmed by other workers.

A mere broadening of the line, however, cannot be expected in the case of light classically scattered within a solid, for although we may picture each atom as vibrating about a mean position as suggested by Einstein in his theory of the specific heats of solids, we also know that the vibration of each individual atom is influenced by neighbouring atoms. As Debye showed in his extension of Einstein's work on specific heats, the atoms move as if the crystal were traversed by an enormous number of sound waves with a large range of frequencies. The effect of this motion on the scattering of light waves was examined theoretically some time ago by Brillouin (*Ann. de Phys.*, **17**, 88, 1922), who showed that it resulted in periodic changes in density, and, therefore, of the refractive index, in the solid.

Brillouin's analysis leads to the result that the solid is divided up by the sound waves into a series of layers of moving atoms from which the incident light may be regularly scattered, with definite angle and phase relations, much in the same way that X-rays are scattered from the layers of atoms in crystals. Moreover, the light reflected from these layers must exhibit a Döppler effect such that a monochromatic source gives rise to scattered radiation consisting only of two sharp components, separated by $d\lambda = 2\cdot\lambda\cdot\frac{v}{V'}.\sin(\theta/2)$ where v and V' are the velocity of sound and light in the solid, respectively, and θ is the angle of scattering. Using a 30-step echelon and making observations with $\theta = 90°$, Gross (*Zeit für Phys.*, **63**, 685, 1930; *Nature*, **126**, 603, 1930) proved that in the case of quartz this relation was satisfied. Light of the same wavelength as the incident line was always present, even if weak, in the scattered radiation, *i e.* a triplet and not a doublet was observed. In some cases, Gross also observed lines of higher order.

We have thus seen that the scattered radiation from a gas is merely a broadened line, while from a solid it consists of sharply defined components; of what should the light scattered from a liquid consist? The investigation of this scattered light is in any case very difficult, because the incident light must be strictly monochromatic and very intense. However, Meyer and Ramm (*Phys. Zeit.*, **33**, 270, 1932) carried out experiments with carbon tetrachloride, benzene, toluene and water, at first examining the light with a 16-step echelon with 10-mm. steps and $\theta = 180°$. In later experiments they used a 33-step echelon with 15-mm. steps, and a resolving power of 680,000 for the mercury 4,358 line from a quartz mercury vapour lamp. They more recently obtained their best results with the zinc 4,680 line emitted from a quartz lamp filled with zinc vapour.

In all cases they observed a triplet consisting of a line of unchanged wavelength and a sharp companion line on either side of it. The separations between these companion lines were 0·124, 0·120 and 0·080Å for benzene, toluene and carbon tetrachloride respectively, which may be compared with the calculated values 0·112, 0·108 and 0·084Å respectively. A strong continuous radiation was superimposed upon these lines, but no lines of higher order, such as were reported by Gross, were found. Consequently, it is clear that the structure of a liquid bears a close resemblance to the structure of a solid, although, perhaps, special assumptions are necessary to account for the presence of light of unchanged wavelength (cf. Debye, *Phys. Zeit.*, **33**, 849, 1932). In view of the ease with which direct reflection may occur, this point may possibly not warrant further consideration.

Now, as such effects are observed in an undisturbed liquid, may they not be produced in much greater intensity by exposing the liquid to supersonic waves? This was done by Debye and Sears (*Proc. Nat. Acad. Sci.*, **18**, 409, 1932), and independently by Biquard and Lucas (*Comp. Rend.*, **194**, 2132, 1932). The stationary waves set up in the liquid by an oscillating crystal, formed a series of layers of compression and rarefaction which behaved as a grating with respect to the incident light waves. Hence, on collecting the scattered light with a lens and projecting it upon a screen, a direct image of the slit source, bounded by spectra of considerable intensity on either side, was obtained. Consequently, only the angles of incidence and reflection of a particular wavelength in any one of the spectra need be determined in order to find the distance of separation of the grating layers, *i.e.* the wavelength of sound in the liquid.

Biquard and Lucas (*Comp. Rend.*, **195**, 121, 1933) have extended their observations to solids. They threw a beam of light upon a sheet of crystalline quartz, 15 mm. thick, which was maintained in resonance with an odd harmonic of its fundamental frequency of $9{\cdot}8 \times 10^6$ vibrations per second, produced by piezo-electric means. Six or seven spectra on each side of the central fringe were easily observed, and, thus, by observing the diffraction spectra from the quartz and from a liquid set in vibration by the quartz, by purely optical methods the ratio of the velocity of sound in quartz to the velocity of sound in the liquid could be obtained.

An interesting advance was made by Bär and Meyer (*Phys. Zeit.*, **34**, 393, 1933), who replaced the slit source used by the previous workers by a source consisting of a regular "square" pattern of fine holes punched in a cardboard screen. Light from a mercury vapour lamp was passed through a condenser lens to form a slightly convergent beam which fell upon the perforated screen. An image of the latter was formed by a projection lens upon a screen or a piece of photographic paper. Somewhere between the projection lens and the screen the liquid exposed to supersonic waves was placed, whereupon the image of each hole was drawn out into a small strip diffraction pattern, such strips lying on a straight line coinciding with the direction of propagation of the sound. This method has many advantages, and has been used to show the reflection of the sound waves at a plane surface immersed in the liquid, and to study the effects produced by different groups of sound waves present in the liquid at the same moment. Heidemann and Asbach (*Zeit. für Phys.*, **87**, 442, 1934) have recently used this method to obtain pictures of the reflection of supersonic waves at concave and convex cylindrical surfaces immersed in a liquid.

Three methods may be used to render visible these stationary supersonic waves in a liquid. Thus, Boyle and Lehmann (*Rep. Canad. Res. Coun.*, 1933) made use of the air bubbles which collected at the nodes when sufficiently intense sources of sound were used. Again, Boyle, Lehmann and Reid (*Trans. Roy. Soc. Canad.*, **19**, 167, 1933) used small particles of cokernut charcoal, which sank slowly through the liquid in which supersonic waves were propagated parallel to the surface. The particles were then driven forward by the pressure of the sound to collect in the nodal planes.

Obviously, these methods cannot be used for very short wavelengths, and Bachem, Heidemann and Asbach (*Zeit. für Phys.*, **87**, 734, 1934) have therefore made use of the changes in density which may be observed in the liquid along the direction of propagation of the sound. In this case the nodes are regions of constant density and the antinodes regions of maximum variation of density. Consequently, the transparency to light varies from place to place along the wave, and regions of maximum transmission are found at distances half a wavelength apart, and may be rendered visible, as a series of light bands separated by dark ones, by a " Schlieren " method. In this way pictures of stationary waves reflected from plane and curved surfaces were obtained. The pictures are remarkably clear and by measurement of the distance between two bright or two dark bands the half wavelength of the sound can be found with considerable accuracy. It is clear that we shall hear more of these interesting experiments.

A New Type of Radioactivity.—It has been known for some time that when certain light elements—Be, B and Al—are bombarded with α-rays from polonium they emit positive electrons, and the mechanism suggested for this emission was discussed in these columns last October. In a further examination of this emission, Curie and Joliot (*Comp. Rend.*, **198**, 254, 1934) have found that the emission persists for some time after the activating polonium has been removed. In the case of boron, it may persist for 15 minutes or so.

Curie and Joliot placed a sheet of aluminium about 1 mm. from a strong polonium source and radiated it for about 10 minutes. It was then placed above a Geiger-Müller counter closed by a thin window of aluminium. It was found that the sheet emitted a radiation which decreased exponentially with time with a half-value period of 3 min. 15 sec. Boron and magnesium gave periods of 14 min. and 2 min 30 sec., respectively. The initial intensity seemed to reach a limiting value of about the same magnitude in the three cases. A number of other light elements did not appear to show these effects, possibly because their periods of decay were too short to be observed. Experiments with an expansion chamber showed that the radiation from aluminium and boron consisted of positive electrons, of energies of $2{\cdot}2 \times 10^6$ and $0{\cdot}7 \times 10^6$ electron-volts respectively. The number of these electrons was reduced by reducing the energy of the bombarding α-rays, no electrons being liberated when the α-ray energy fell below 10^6 electron volts.

Curie and Joliot suggested that the following equation represented the mechanism of emission in the case of aluminium, where n^1 is a neutron,

$$Al^{27} + He^4 = P^{30} + n^1.$$

The isotope of phosphorus, P^{30}, was assumed to be radioactive, breaking up according to the equation,

$$P^{30} = Si^{30} + e^+.$$

Similar equations were suggested for the cases of boron and magnesium, and it was also suggested that similar examples of induced radioactivity would undoubtedly be produced by bombardment of light elements with other particles.

Such experiments have been made by Lauritsen, Crane and Harper (*Science*, **79**, 234, 1934, and *Phys. Rev.*, **45**, 430, 1934), who bombarded a target of B_2O_3 with deutons of $0{\cdot}9 \times 10^6$ electron-volts energy. The equation suggested to represent the mechanism was

$$B^{10} + H^2 = C^{11} + n^1 = B^{11} + n^1 + e^+.$$

Crane and Lauritsen (*Phys. Rev.*, **45**, 497, 1934) have also used $0{\cdot}9 \times 10^6$ electron-volt protons for the same purpose, and obtained induced activities of about 10 to 20 per cent. of the intensity of those produced by deuton bombardment. The equation suggested to represent the mechanism in the case of boron under proton bombardment was

$$B^{11} + H^1 = C^{11} + n^1 = B^{11} + n^1 + e^+.$$

Actually the positron energy spectrum was continuous, some positrons being given off with energies in excess of 0·9 × 10^6 electron-volts.

When a B_2O_3 target which had been exposed to deuton bombardment was placed inside a cloud chamber, it was found that after a short time a large number of tracks appeared to originate, not from the target, but from the gas in the chamber. This was found due to the presence of a radioactive gas which had diffused out of the target. Some of this active gas was driven from the target by heating, and collected, mixed with air, of course. From an examination of the mixture it was decided that the active gas was CO or CO_2.

GENERAL AND ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London

New Oxides.—For many years fluorine and bromine occupied a special position among the elements in that, though they were very reactive, no oxides of them were known. In 1927 Lebeau and Damiens obtained a gaseous oxide F_2O, more stable than the corresponding chlorine compound, by the electrolysis of moist, molten acid potassium fluoride below 100° C., and later showed that it could be obtained more easily by passing fluorine into dilute sodium hydroxide solution. Ruff and Menzel (*Zeit anorg. Chem.*, 1933, **211**, 204) have now, by passing an electric discharge through an equimolecular mixture of fluorine and oxygen under 15 mm. pressure at the temperature of liquid air, obtained an oxide F_2O_2 as a yellow solid m.p. $-160°$ C volatilising as a brown gas which above $-100°$ C. dissociates to a colourless gas FO which does not reform F_2O_2 on cooling.

The oxides of bromine are less well characterised; Zintl and Rienäcker (Ber., 1930, **63**, 1098) obtained small quantities of a volatile oxide, perhaps Br_2O, by passing bromine vapour over mercuric oxide and by treating bromine vapour with ozone under reduced pressure at 0°. Lewis and Schumacher (*Zeit. anorg. Chem.*, 1929, **182**, 182) by a similar method obtained a white solid to which they assigned the constitution $(Br_3O_8)_n$.

Schenk (*Zeit. anorg. Chem.*, 1933, **211**, 150; *Zeit. Elektrochem.*, 1933, **39**, 594) has prepared the long-sought sulphur monoxide, SO, by passing an electric discharge through a mixture of sulphur dioxide and sulphur vapour at low pressures. The gas, when dry, decomposes to sulphur dioxide and sulphur slowly at room temperature but rapidly at 100° C. Whilst sensitive to water it does not react with oxygen at room temperature; it is condensed to an orange-red solid on cooling in liquid air, but this solid, rather surprising, on evaporating gives sulphur and sulphur dioxide.

ATOMIC WEIGHTS.—The Committee of the International Union of Chemistry on Atomic Weights have introduced nine changes into the Table for 1934. Selenium becomes 78·96, a fall of 0·24; Tellurium 127·61, a rise of 0·11; Cæsium 132·91, a rise of 0·10; Erbium 165·20, a fall of 2·44; Ytterbium 173·04, a fall of 0·46; Osmium 191·5, a rise of 0·7; the remainder have alterations only in the second decimal figure.

HYDROGEN ISOTOPES.—Since the discovery by Washburn and Urey and development by Lewis and Macdonald (*J. Chem. Phys.*, 1933, **1**, 341) of a simple method of isolating water containing the hydrogen isotope of mass two, a large number of investigators have taken up the study of the important consequences and the subject is rapidly acquiring an extensive literature. Unfortunately the urge to publish the results of preliminary experiments makes it likely that some of the statements made will not survive closer investigation and several contradictions already occur.

NOMENCLATURE.—Urey, Brickwedde and Murphy, who first obtained concentrates of the heavy isotope of hydrogen, suggest the names protium and deuterium for the light and heavy isotopes and proton and deuton or deuteron for the respective nuclei, retaining the term hydrogen for the naturally occurring mixture. Rutherford (*Nature*, 1933, 956) suggests the retention of hydrogen and proton for the molecule and nucleus of hydrogen of mass one and diplogen and diplon for those of hydrogen of mass two. The relative advantages and disadvantages of various names are discussed by Urey, Brickwedde and Murphy (*Nature*, 1934, 173) and by Sidgwick (*ibid.*, 256). The chief objection raised to deuterium is a possible confusion between neutron and deuton or deuteron in the spoken language. The necessity of making this distinction may encourage scientists to speak more clearly and does not seem a sufficient justification to reject the name of the parents; diplogen should not survive the devastating criticism of Armstrong (*Nature*, 1934, 173). As this substance is becoming of great importance to the chemist proper it is to be hoped that the nomenclature will not be decided by physicists. Whatever name is ultimately adopted it seems that the symbol D will meet with general acceptance.

CONCENTRATION OF DEUTERIUM OXIDE.—Fortunately the concentration of deuterium oxide in water, though tedious and expensive, presents few difficulties as the efficiency of the electrolytic separation is, to a considerable extent, independent of the conditions. The process depends on the electrolysis of water containing sodium hydroxide as electrolyte when hydrogen is evolved at a greater rate than deuterium.

The efficiency of the process, α, may be most conveniently defined by the relationship $d\ ln\ n_H = \alpha d\ ln\ n_D$ where n_H and n_D are the number of hydrogen and of deuterium atoms in the solution respectively (Topley and Eyring, *J. Amer. Chem. Soc.*, 1933, **55**, 5058). This is the reciprocal of the α originally used by Lewis and is equivalent to the ratio of the amount of hydrogen to the amount of deuterium evolved when an infinitely small amount of an equimolecular mixture of water and deuterium oxide is decomposed.

Collie (*Nature*, 1933, 568) found that the efficiency of the process was poor when using an electrolyte containing 10 per cent. of KOH unless the current density was kept below 0·1 amp. $cm.^{-2}$, but with 0·3% KOH a satisfactory separation was obtained with current densities as high as 0·5 amp. $cm.^{-2}$. Bell and Wolfenden (*Nature*, 1934, 25) found little difference in the efficiency at 10° C. and 100° C. using 2% NaOH and a nickel cathode and no very marked difference between a current density of 10 amps. $cm.^{-2}$ and 0·07 amp. $cm.^{-2}$ when using 1% NaOH. They also find practically no difference between the use of 1% NaOH and 8% NaOH, but do not specify what current density was used in these experiments. Likewise using nickel, platinum or copper cathodes with 1% NaOH produced little difference. Lewis and Macdonald obtained α = about 5, Collie 3·8, Bell and Wolfenden values from 3·7 to 5·5 depending on conditions. Topley and Eyring (*J. Amer. Chem. Soc.*, 1933, **55**, 5058; *Nature*, 1934, 292), while agreeing with Bell and Wolfenden that moderate changes of current density have little influence, point out that the nature of the cathode is an important factor, α varying from 7·9 to 2·8 in the order smooth Pt, Pb, Fe, Cu, Ag, Ni, W, Pt black, Ga liquid, Hg. Harteck (*Proc. Phys. Soc.*, 1934, **46**, 277) finds that the efficiency of the process is almost independent of the concentration of alkali over a wide range and that there is no definite increase in efficiency with increasing current density provided this is greater than 0·6 amp. $cm.^{-2}$. With carefully cleaned nickel electrodes he obtained α = 5·5 to 6·7. When the concentration of deuterium in the electrolyte approaches 10 per cent. the loss of deuterium in the evolved gases becomes considerable and it is desirable to recover it. The burning of the electrolytic gas presents a problem which Harteck has solved by bubbling it through paraffin oil to prevent back-fire and combining on the surface of a hot wire inside a wide condenser, the condensed water collecting under the oil.

Although the electrolytic method of concentration is the only one which has been extensively employed there is promise that certain chemical methods may be of use, at least in the final stages of the operation. A. and L. Farkas (*Nature*, 1934, 139) found a value for α when metals undergo solution in 0·1N sulphuric acid of Na 1·2, Ca 1·5, Al 2, Zn 4, but Hughes, Ingold and Wilson (*Nature*, 1934, 291) find that the value for Na is 2·9, and independent of the nature of the medium from strongly acid to strongly alkaline; this value is supported by the work of Davis and Johnston (*J. Amer. Chem. Soc.*, 1934, **56**, 492) who obtained a figure 2·5.

Hughes, Ingold and Wilson (*J. Chem. Soc.*, 1934, 493) have examined the action of Li, Na, K, Mg, Ca, Zn, Al, Ce, Si, Cr, Mn, Fe and Co, where possible in both acid and alkaline media, certain impure elements, a number of Zn couples, sodium amalgam, calcium hydride, calcium carbide and aluminium carbide. The values for α they find are Ca 1·3, Li 1·5, K 1·9, Mg 2·1, Si 2·3, Na 2·8, Al 4·0, Co 4·1, Fe 4·3, Cr 4·5, Mn 5·1, Zn 5·6 and in the case of a zinc-copper couple containing 50 per cent. of copper 8·0. In the light of their results they discuss the mechanism of the reducing action of dissolving metals and suggest that there are no less than three different mechanisms by which this is brought about, depending upon the particular metal and the alkalinity or acidity of the solution.

An important observation is due to Hall and Jones (*J. Amer. Chem. Soc.*, 1934, **56**, 749) who find that there is an appreciable fractionation of deuterium oxide on distillation, especially under reduced pressure, of aqueous solutions containing 2 per cent. or more (cf. Washburn and Smith, *J. Chem. Phys.*, 1933, **1**, 426). For example, on distilling a 2 per cent. solution of deuterium oxide at 10 mm. in an ordinary flask without a fractionating column, the first fifth of the distillate contained 32 per cent. less deuterium than the starting material and the last fifth about 25 per cent. more. This may explain certain discrepancies in the results obtained by those working with solutions of this strength.

REACTIONS.—Oliphant (*Nature*, 1933, 675) has observed that when deuterium is kept in contact with water for long periods an exchange of atoms occurred between the gas and water. Horiuti and Polanyi (*ibid.*, 819) find that this reaction is catalysed by platinum black and suggest that Oliphant's results are due to a hidden catalyst, possibly the glass walls of the vessel (cf. Bonhoeffer and Rummel, *Naturwiss.*, 1934, **22**, 45). Gould and Bleakney (*J. Amer. Chem. Soc.*, 1934, **56**, 247) find, however, that the concentration of deuterium-hydrogen mixtures in contact with water in clean glass vessels remains constant if precautions are taken to avoid preferential solubility, diffusion and the presence of metallic surfaces.

The interchange of hydrogen and deuterium between compounds of the former and deuterium oxide is of special interest and offers a wide field for investigation. A rapid equilibrium is established between the hydrogen of ammonia and the deuterium of the deuterium oxide in which it is dissolved (Lewis and Macdonald, *J. Amer. Chem. Soc.*, 1933, **55**, 3502). When acetone is treated with deuterium oxide in the presence of potassium carbonate part of its hydrogen is replaced by deuterium and it is suggested that the reaction occurs through enolisation of the ketone (Halford, Anderson and Bates, *J. Amer Chem Soc.*, 1934, **56**, 491). Hall, Bowden and Jones (*ibid.*, 750) find that there is no interchange of hydrogen and deuterium when the following substances are dissolved for a brief time in water containing 2 per cent. of deuterium oxide: potassium hypophosphite, potassium acetate and sodium benzoate, but that one-third of the hydrogen is immediately exchanged in the case of ethylene glycol. With potassium acetate some exchange occurs with the hydrogens of the methyl group on prolonged contact even when nothing is added to promote enolisation. This does not seem to be in agreement with the results of Bonhoeffer and Klar (*Naturwiss.*, 1934, **22**, 45), who find that no exchange takes place between deuterium and the hydrogen of the methyl group in sodium acetate Lewis and Schutz (*J. Amer. Chem. Soc*, 1934, **56**, 493) find that there is no exchange between the methyl hydrogens and deuterium in deuterium acetate[1] (deutacetic acid), CH_3CO_2D.

Pure deuterium acetate has been prepared by Lewis and Schutz (loc. cit.) by the action of pure DCl on silver acetate. It melts at 13·3° C., 3·3° lower than acetic acid. These authors suggest that the difference between the isotopic forms of such substances as water and ammonia can be chiefly ascribed to the increase in strength of the D bond over the H bond The electrolytic concentration of deuterium oxide in water and the action of metals on a mixture of deuterium oxide and water point in the same direc-

[1] For the compounds of heavy hydrogen the name deuterium and symbol D will be used in SCIENCE PROGRESS until some definite agreement on nomenclature is reached; *e.g.* CH_2DCOOH is monodeuteroacetic acid and CH_3COOD, deuterium acetate. The name used by the authors of the papers referred to will be given in brackets.—ED.

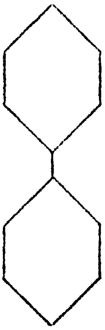
tion (cf. Hughes, Ingold and Wilson, *J. Chem. Soc.*, 1934, 493). Lewis and Schutz have obtained confirmation of their views by the study of the vapour pressures of deuterium acetate and deuterium cyanide (deutocyanic acid) compared with those of acetic and hydrocyanic acids. They have obtained (*J. Amer. Chem. Soc.*, 1934, **56**, 1002) an even more interesting result from preliminary measurements of the dissociation constant of deuterium acetate in deuterium oxide which show that the value is not more than one-third of that of acetic acid, indicating how much more tenaciously a deuton is held by a pair of electrons of another atom, in this case the carboxyl oxygen, than is a proton.

BIOLOGY OF DEUTERIUM OXIDE.—The biochemical behaviour of deuterium oxide naturally gives rise to speculation. It seems possible that there is a selective absorption of deuterium oxide in cases of plant growth, for the water obtained from the sap and from the combined hydrogen of the woody parts of a willow tree was found to be richer in deuterium than the local water (Washburn and Smith, *Science*, 1934, **79**, 188). Lewis (*ibid.*, 151) found that tobacco seeds germinate less readily in deuterium oxide and that flat-worms apparently died in a 90 per cent. solution but some recovered on placing in water. The effect of large doses on a warm-blooded animal (mouse) was to produce the symptoms of inebriation and the more deuterium oxide drunk the thirstier the animal became ! Apparently it would require some litres to produce a similar reaction in a human being.

Pacsu (*J. Amer. Chem. Soc.*, 1934, **56**, 245) finds that the rate of the alcoholic fermentation of glucose is greatly reduced in pure deuterium oxide, but Meyer (*Science*, 1934, **79**, 210) records that Aspergillus grows much more rapidly in Pfeffer's solution made with 0·47% deuterium oxide than with distilled water. Macht and Davis (*J. Amer. Chem. Soc.*, 1934, **56**, 246), as the result of ten pharmacological experiments with water containing one part of deuterium to two thousand of hydrogen, find that no striking physiological or pharmacological effects are produced with the exception of a slight inhibitory influence on the germination of some seeds.

STEREOCHEMISTRY OF DIPHENYL COMPOUNDS.—There are few branches of organic chemistry in which more interesting advances have been made in recent years than that of stereochemistry. In the field of optical isomerism practically all the progress has been made as the result of the escape from the shackles laid upon the subject by van t'Hoff when he introduced the term " asymmetric carbon atom," and the realisation that optical isomerism was due to molecular dissymmetry and that compounds of the type *Cabcd* were but a special case of this dissymmetry (Barker and Marsh, *J. Chem. Soc.*, 1913, **103**, 837). Few ideas have been more prolific of experimental results than that of the dissymmetry of diphenyl compounds substituted in the ortho positions to the juncture of the two benzene rings (VII).

In diphenyl (I) the two benzene rings are either co-planar or can become so owing to their free rotation about the bond joining them.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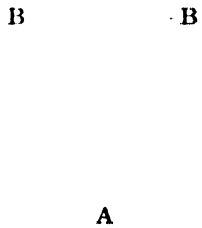

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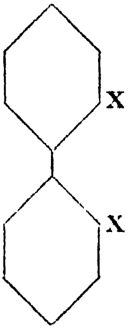

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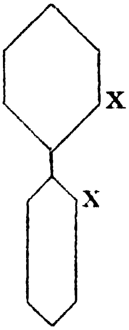

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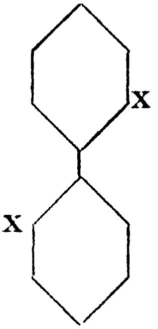
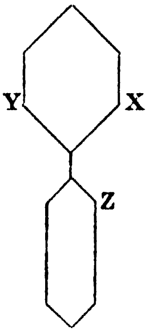


(V)

In this case the compound possesses several planes of symmetry, for example the plane of the paper, a plane at right angles to the plane of the paper XI A . . . A, and a third at right angles to the plane of the paper B . . . B ; the mirror image of the compound is therefore superimposable upon the original and optical isomerism is impossible. If, however, the diphenyl has two substituents, one in each ring, ortho to the point of juncture (III) the size of these substituents may be enough to prevent the rings becoming co-planar when these two groups are on one side The two rings will then be inclined to one another (IV) and the planes of symmetry disappear and optical isomerism is possible. If, however, the group X can pass the hydrogen in the other ortho position free rotation will enable the two rings to become co-planar again (V) and optical isomerism disappears. The conditions for optical isomerism in these compounds is therefore that the degree of rotation about the bond joining the two rings shall be less than 180°. This has been achieved experimentally in numerous cases by introducing two sufficiently large substituents in the ortho positions in one ring and one in the other (VI).

Providing Z for spacial reasons cannot pass X or Y and cannot approach sufficiently near either of them for the two rings to become co-planar the molecule is dissymmetric and the compound can exist in optically active forms. This condition exists, for example, in the cases of 2 : 2′ : 6 : 6′-dinitrodiphenic acid (VII) and 6-nitro-2-methyldiphenyl-2′-carboxylic acid (VIII), both of which have been resolved into optical isomerides



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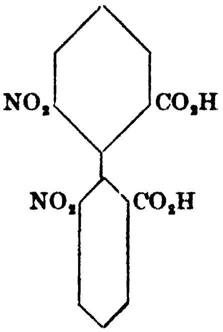

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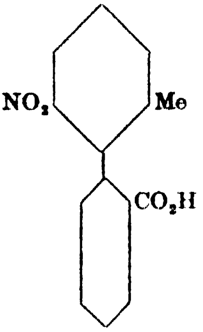

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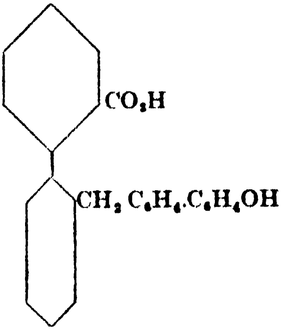
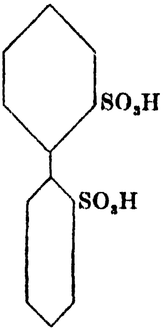


(IX)

Recently investigations have been made to ascertain which is the smallest atom or group which will give the necessary restriction of rotation Judging from the rate of racemisation of suitable optically active compounds the order Br > Cl > OMe > F has been arrived at as the relative effective interference with the rotation of a benzene nucleus with a carboxyl group in the ortho position (Yuan and Adams, *J. Amer. Chem. Soc.*, 1932, **54**, 2966; Stoughton and Adams, *ibid.*, 4426). Diphenic acid (IV), where $X = CO_2H$, cannot be resolved as the carboxyl group is sufficiently small to pass the hydrogen atom in the ortho position. It is, however, possible to find groups sufficiently large to fail to pass this hydrogen and 2-(hydroxydiphenylmethyl) diphenyl-2′-carboxylic acid (IX) (Corbellini and Pizzi, *Atti. R. Accad. Lincei*, 1932 (VI), **15**, 287), diphenyl-2 : 2′ disulphonic acid (X) (Lesslie and Turner, *J. Chem. Soc.*, 1932, 2021), *o*-(2-dimethylaminophenyl) phenyltrimethylammonium (XI) (Shaw and Turner, *ibid.*, 1933, 135), and 2 : 2′-di-iodo-diphenyl-4 : 4′-dicarboxylic acid (XII) (Searle and Adams, *J. Amer. Chem. Soc.*, 1933, **55**, 1649) have now been resolved into optical enantiomorphs.

The most interesting case, however, is that of the 2-(*m*-bromophenyl) phenyltrimethylarsonium ion (XIII) which Lesslie and Turner (*Chem. and Ind.*, 1933, **52**, 19; *J. Chem. Soc.*, 1933, 1588) have found to exhibit evidence of optical activity. Here the restricted rotation is due solely to the inability of the large trimethylarsonium group to pass the ortho hydrogen atoms on the other nucleus. This other nucleus must be meta-substituted since



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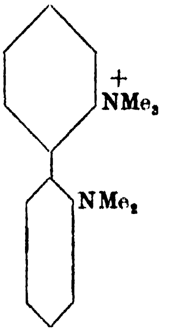

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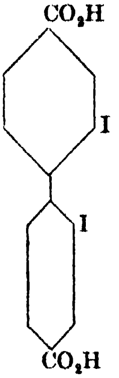

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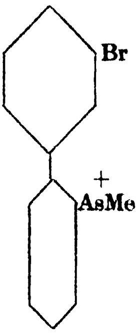
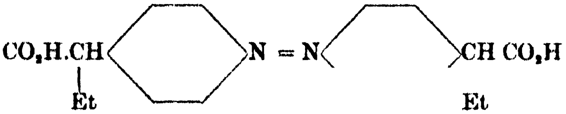


(XIII)

otherwise the compound would possess a plane of symmetry when the two rings were at right angles.

The Structure of Azoxy-compounds.—An ingenious proof of the unsymmetrical structure of azoxy-compounds has been furnished by Chu and Marvel (*J. Amer. Chem. Soc.*, 1933, **55**, 2841). They prepared meso- and dl-α-p-azophenyl butyric acids (XIV).



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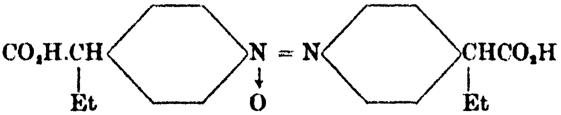
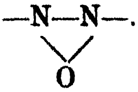


(XV)

They resolved the dl-compound thus establishing that their other compound was indeed the meso form and consequently had either a plane or a centre of symmetry. This meso-compound on oxidation with hydrogen peroxide gave an azoxy-compound (XV) which was resolvable. The introduction of the oxygen atom must therefore have destroyed the plane of symmetry as in (XV) but could not have done so if the azoxy-compound had possessed the ring structure  $\underset{\diagdown\,\mathrm{O}\,\diagup}{\mathrm{-N-N-}}$. The meso compound was not isomerised during the oxidation as the dl-compound (XIV) on similar oxidation gave a different azoxy-derivative which was also resolvable.

PHYSICAL CHEMISTRY. By O. H. WANSBROUGH-JONES, Ph.D., M.A., Department of Colloid Science, Cambridge.

CHEMICAL literature in the past few years has contained a large variety of papers on subjects dealing with the application of sound waves, sometimes as a stimulus, sometimes as a measuring device, to chemical systems. There is no logical connection between the different applications, and beyond the single connecting link that energy is applied in the form of sound waves there is no justification for attempting to summarise the various types of experiments that have been performed in a single report, but since some of the applications will prove of increasing and others of diminishing importance, the present is a convenient time to discuss most of them. Their rapid development has been due in great part to two major experimental advances, the work of Wood and Loomis (*Phil. Mag.*, (vii) **4**, 417, 1927) on the production and properties of ultra-sonic vibration, that is sound vibration of very high frequency with considerable energy content; and the development of acoustical interferometric measurements particularly by Pierce (*Proc. Am. Acad. Sci.*, **63**, 1, 1928) and Hubbard (*Phys. Rev.*, **41**, 833, 1932); and their scope includes studies on the dispersion and precipitation of colloid particles, the stimulation of chemical reaction, the examination of the hydration of electrolytic ions, reaction velocities, the thermodynamic constants of gases and probably of the most far-reaching importance, on the mechanism of energy redistribution during and after collision between molecules. It is proposed to treat more or less briefly all these various subjects.

The application of ultrasonic waves to colloidal systems was mentioned in Wood and Loomis' original work; and it was clear that here was a very convenient method of dispersing a material of small tensile strength in a medium. The converse phenomena precipitation, and the breakdown of gels, have also been studied. Thus in the first case, Szalay (*Z. Phys. Chem.*, **164**, 234, 1933) exposed solution of starch, gum arabic, or gelatine to ultrasonic vibrations of over 700,000 cycles frequency and detected both by iodometric estimation and by the changes in viscosity of the solution some depolyerisation of the large molecules. Simpler molecules also showed some slight effect, cane sugar being slightly broken down to monosaccharides, and paraformaldehyde and hexamethylenetetramine were also decomposed, but a large number of other substances which were tried showed no such effect. It may be mentioned here that there are certainly large local heat changes in the solution, and that any dissolved gas, such as oxygen, will very readily react under these conditions with the material of the solution. Bull and Söllner (*Koll. Z.*, **60**, 263, 1932) showed that heterodisperse systems of mercury of particle size of the order of 1μ might readily be prepared by the application of ultrasonic vibration, the systems being stable in the presence of the usual emulsifying agents, and when once prepared showing no properties distinguishing them from emulsions prepared by more conventional means.

Thixotropic systems are normally liquefied by short-wave sound. Freundlich, Rogowski, and Söllner (*Z. Phys. Chem.* A., **160**, 469, 1932 ; *Koll. Beihefte*, **37**, 223, 1933) have shown that ferric hydroxide and aluminium hydroxide thixotropes are liquefied by ultrasonic sound waves and that the liquefaction starts at the air-gel interface and then spreads through the material. Again the liquids show no special properties, setting to the gel form in the same time interval as if they had been liquefied by normal shaking; so that it is fair to assume that no material alteration occurred in the substance, particularly as subsequent prolonged exposure of the liquid to the radiation produced no effect. Silica gels under the same treatment were observed to remain solid, but temporarily lost the greater part of their elasticity. Results analogous to those quoted on the formation of sols were found with rubber and gelatine, whose peptisations were much accelerated, according to these authors by reason of the severe local heat-effects at the interface. Marinesco (*Comp. Rend.*, **194**, 1824, 1932) has also examined hydrophilic and thixotropic gels, and observed that besides the normal liquefaction, peculiar eddies are formed in the substance. The thixotropic transformation was practically instantaneous when the sound waves were applied. Slightly different results were obtained by Boutaric and Bouchard (*Bull. Soc. Chim.*, **51**, 543, 1932) by whom lyophobic sols were examined under this stimulus. Arsenious sulphide or gum mastic sols were prepared containing just enough electrolyte to prevent flocculation, and their turbidity observed with a spectrophotometer. This provides a delicate means of observing the formation of any slight turbidity caused by incipient flocculation and it was found that while very slight agitation was sufficient to precipitate the sol, ultra-sonic waves of 18 mm. wave-length caused flocculation to occur only when turbulence was produced, stationary wave systems being quite ineffective.

To summarise the work on colloidal systems, it does not appear to the writer that any great advance has been made in this field, and in fact such work as has been done seems really to indicate that no great advance is to be expected, though it is possible that further information on gel structure might be obtainable.

Such papers as have appeared on the stimulation of chemical reactions by sound waves have, with one exception, referred to the use of ultra-sonic frequencies. The single exception (Flosdorf and Chambers, *J. Am. Chem. Soc.*, **65**, 3051, 1933) dealt with work with audible sound, 1,000 to 15,000 frequency, naturally of high intensity, and according to these authors certain reactions were markedly accelerated by its means. Thus albumen was almost instantaneously coagulated at 30° C., two other complicated molecules, agar and starch, gave rise to some breakdown products of dextrin and reducing sugars ; while the rate of hydrolysis of simpler molecules such as sucrose, and ethyl acetate was greatly increased. Any free oxygen in solution rapidly oxidised any reducing compound such as H_2S, a reaction commonly met with by workers with ultra-sonic waves. No explanation of these facts is offered by the authors but they state that the effect may be due to local rises in temperature as the sound energy is converted to heat, though it is easily shown that even though all the energy put in was converted into sound, uniform distribution throughout the whole system would not make the temperature rise nearly enough to account for the observations. The formation of bubbles, cavitation, undoubtedly has much to do with some of the reactions seen to be accelerated by ultra-sonic waves, but for this set of studies it has been shown that the effects persist when increase of pressure to 4 to 10 atmospheres has suppressed the cavitation.

Schmitt, Johnson, and Olson (*J. Am. Chem. Soc.*, **51**, 320, 1929) examined the effect of ultra-sonic waves of frequency 750 kc. on various reagents contained in a test-tube dipping into the liquid dielectric above the oscillating quartz crystal. Several chemical changes were found to take place, but it seems likely that they owed their origin to the action of dissolved oxygen either directly or through the formation of hydrogen peroxide. Thus iodine was liberated from KI, and sulphur from H_2S though admittedly in small quantities. Reactions with indicators have caused some disagreement, Schmitt, Johnson and Olson having observed that brom-thymol blue solution was decolorised, giving a slightly acid reaction in which the dye molecules seemed not to have been destroyed, while Szu Chih Liu and Hsien Wu (*J. Am. Chem. Soc.*, **54**, 791, 1932) obtained much fading, with twelve different indicators, including brom-thymol blue, only when oxygen was present in the solution, and found that when the solutions were again made alkaline the colour was not restored, but that the oxidation had permanently destroyed the dyestuff molecules. Their control experiments seem to have been most careful and there is little difficulty in accepting their conclusions. Olson and Gardiner (*J Am. Chem. Soc.*, **54**, 3617, 1932) also appear to accept these conclusions, but consider that the earlier results they reported are not necessarily in disagreement with these, since samples of brom-thymol blue differ enormously, and they are inclined to ascribe the apparent differences to the inhomogeneity of the dyestuff. To summarise, the effects seem all to be due to dissolved gases, and any more work done in this field should be devoted to attempts to analyse the mechanism ; it seems that reaction takes place at the interfaces formed by cavitation and requires a certain critical bubble size though Szu Chih Liu and Hsien Wu suggested the oxygen was directly activated. Beuthe (*Z. Phys. Chem.* A., **163**, 161, 1933) found similar effects due to dissolved oxygen being freed. There have also been two papers including work on heterogeneous systems ; Marinesco and Trillat (*Comp. Rend.*, **196**, 858, 1933) showed that while ultra-sonic waves did not directly affect a photographic plate they did increase a latent image already existing, and facilitate development, and Moriguchi (*J. Chem. Soc. Japan*, **54**; 949, 1933) showed that hydrogen was freed more rapidly by zinc from hydrochloric or sulphuric acids, or carbon dioxide from calcium carbonate and hydrochloric acid on exposure to ultra-sonic vibrations.

The most ingenious suggestion for an application of sound waves is due to Debye (*J. Chem. Phys.*, **1**, 13, 1933). The problem of the determination of the degree of solvation of an ion is an old one, of the first importance in any theory of electrolytic dissociation, and a method revealing directly the masses of the ions would be of the greatest value ; but ordinarily the mass does not enter into any calculation that may be checked by experiment, for the forces due to the friction of the ions in the solvent are so much greater than the dynamical reactions of the masses that the latter must be negligible. The application of ultra-sonic waves to a solution should, however, cause no frictional forces between the ions and liquid, which will behave like a homogeneous medium unless the masses of the different ions are taken into account. Then, however, there will be a different movement for different masses, resulting perhaps in a separation of the positive and negative ions, causing a periodic potential difference in the electrolyte of the same frequency as the exciting vibration. An approximate calculation shows that potentials of the order of 10^{-6} volts might well be produced which should be detectable. Unfortunately, definite experimental results are not yet available, but it is greatly to be hoped that these very interesting experiments will prove successful. It may be mentioned that in readiness for them, Syôten Oka (*Proc. Phys. Math. Soc. Japan*, **15**, 413, 1933) has worked out a more exact theory for the expected effect, taking into account the times of relaxation of the ion atmospheres, and the electrophoretic forces which would be present.

Richards and Reid (*J. Chem. Phys.*, **1**, 14, 1933) have endeavoured to use sound velocity measurements to determine the rate of dissociation of nitrogen tetroxide. This is not a new idea, for attempts were made by Natanson as long ago as 1885 to use the method for this purpose, while Nernst and Keutel experimentally in 1910 and Einstein in a theoretical study (*Sitz Ber Akad*, 380, 1920) have examined the problem; but in the past the manifold technical difficulties and uncertainties of interpretation have made it difficult to be sure that such measurements as had been made had any real meaning. It seems now that real progress has been made, as will be briefly reported. The principle of the method lies in the measurements of sound dispersion, the separation of the total dispersion into that due to the heat capacity of the gas and that due to its dissociation now being possible since the original expression of Einstein (cf. also Bourgin, *Phys. Rev.*, **42**, 721, 1932) has been modified to include heat-capacity dispersion, which is now known to occur in certain non-dissociating gases such as carbon dioxide. This latter type of dispersion is taken to indicate a lag between translational and internal energy adjustment (Herzfeld and Rice, *Phys. Rev.* (2), **31**, 691, 928, Kneser), and it shows the same type of dependence on pressure, temperature, and frequency as that due to the reaction rate, so that the separation of the two effects is far from easy, but while the attempts to do so by Richards and Reid have been adversely criticised (Teeter, *J. Chem. Phys.*, **1**, 251, 1933) it seems that their case is a good one. Luck (*Phys. Rev.*, **40**, 440, 1932) extended Einstein's formula for ideal gases to mixtures of real gases, and showed that absorption would greatly hinder the use of the method for the measurements of reaction rates, while the departures of real gases from ideality would also cause disturbing effects, but Richards and Reid seem to have been very successful in isolating such effects.

That there is some uncertainty in interpreting the dispersion measurements due to inaccurate knowledge of the heat-capacity variations is fully admitted, but in a long analysis and after consideration of the numerical orders of magnitude involved, these authors are satisfied that their rate dispersion values are significant.

To obtain a value for the latter it is strictly necessary to employ two frequencies for each temperature and pressure, and to show in each case a dispersion greater than the maximum value possible for a pure heat-capacity change, and it is satisfactory that the results seem to justify the procedure adopted, since they are reasonable and self-consistent.

The experimental methods used led also to some difficulties in interpretation. The oscillator was driven at a known frequency, ranging from 9 kc. to 451 kc , the velocity of the sound could be measured to about ± 0, 1 m./sec. The maximum dispersion found was about 5 m./sec. after the various " theoretical " heat-capacity and " experimental " tube corrections (due to the gas being confined in the boundaries of a narrow tube) had been applied, and the authors conclude after a consideration of the approximations they introduced and the uncertainties of the experimental values, that the reaction velocity constants for nitrogen tetroxide would be accurate to about 10 per cent. To discuss the meaning of the results is more properly a matter for an article on reaction kinetics, but it is interesting to state that the velocity constant decreases slightly with diminishing pressures, and that like nitrogen pentoxide, the molecular area for activation by collision is apparently too large, for the tetroxide being about three times the normal kinetic theory value.

It has been mentioned above that some of the conclusions drawn were open to criticism. Teeter (loc. cit.) based his main criticisms on the facts that the dispersion produced by the time lag in the equilibrium between the translational-rotational and the vibrational degrees of freedom would cause an increase in sound velocity parallel with that caused by dissociation. This is, of course, admitted by Richards and Reid, but while they claim that their analysis of the results justifies their procedure in assigning a value to the rate dispersion s, Teeter considers from his study of the theory and supported by experimental observation of his own, that the sonic method cannot usefully be used to give the required information as to the rate at which equilibrium in a dissociating gas is attained. Richards and Reid (*J. Chem. Phys.*, **1**, 737, 1933) do not consider that his criticisms are valid, and certainly it seems that their arguments are the stronger, but the subject is a rather difficult and complicated one. They have, however, extended their studies to include the explanation of the absorption of sound, and they show that absorption measurements, as well as dispersion measurements, should be capable of distinguishing between absorption due to heat capacity or dissociation.

Theory showed that an absorption maximum should be discoverable, experiment unfortunately failed to disclose it, and while the authors of this paper state that in their opinion dispersion rather than absorption measurements offer the better chance of obtaining quantitative results, they consider that with care and more refined apparatus positive results on these lines might be obtained, and indeed if measurements of sufficient accuracy and certainty could be made, they would actually give information not available from dispersion methods.

Richards' next paper (*J. Chem. Phys.*, **1**, 863, 1933) is a theoretical study of the influence which different relaxation times of various molecular states will have on the variation of the velocity of sound with the frequency. Equations are derived to cover the acoustical behaviour of a non-dissociating gas having several independent energy states all of different relaxation times, and while these equations are cumbrous and complicated, approximations by means of which direct test with experiment may be made, are suggested. Their use is shown in the two latest of this series of papers (Richards and Reid, *J. Chem. Phys. Z*, **193**, 206, 1934) in which measurements on CO_2, CS_2, and C_2H_4 are reported. In carbon dioxide, at so low a frequency as 9 kc. it was found that part of the ideal heat capacity was not making its contribution in the adiabatic elasticity at this frequency, and it is suggested that even at lower frequencies this term is not excited. An examination of the band spectroscopical evidence supported the view that this mode of vibration was the symmetrical linear $O \longleftarrow C \longrightarrow O$ type. The measurements also led to the not surprising conclusion that the van der Waals' forces follow the acoustical cycle without lag, quantitatively through all the regions of pressure, temperature, and frequency that were studied. The rotational terms of the molecule are fully maintained throughout the whole region, and they are classically excited, and in order to excite the deformation vibration term, considerable energy of activation of the order of 6,000 cal./mol. is necessary. Carbon disulphide gave similar results so far as the van der Waals' forces are concerned, and for this substance the two modes of vibration $S \longleftarrow C \longrightarrow S$ and $S \uparrow C \downarrow S \uparrow$ leave the acoustical cycle at the same sound velocities, though in this case at much higher frequencies than for CO_2. The activation energy for the collision process exciting their vibration is about 5,400 cal./mol. The analysis of sulphur dioxide proved more troublesome, partly because the spectroscopic data was not quite adequate, but here again a lag in the adjustment of certain energy states is clear, though in this case the introduction of the activation energy is difficult. Finally, ethylene has been studied in its dispersive region in an attempt to gain more information as to the mechanism of the process of activation by collision, particularly in the lower vibrational states Again the necessary band spectral data are not available for a complete interpretation, but it seems probable that the vibrational energy attains equilibrium at every effective collision, and that the energy of activation for such collision is about 2,800 cal From the purely chemical standpoint, the measurements made on mixtures of ethylene with other gases are particularly interesting for the efficiency with which collisions between molecules of ethylene with other gases cause vibrational energy transitions in ethylene itself may be estimated, and it can be concluded that argon, nitrogen and helium molecules fail completely to bring about this adjustment, while hydrogen-ethylene collisions have an efficiency about $\frac{1}{16}$ of that between two molecules of the latter gas. This finding is quite in line with our knowledge of the efficiency of hydrogen in maintaining the velocity constant at low pressures in the unimolecular decomposition of organic molecules, in which also the hydrogen molecule is effective in converting translational to vibrational energy, but it does not offer any explanation of this fact. Tentative suggestions are made by Richards and Reid, and amongst them the suggestion that the small moment of inertia and consequent large rotational quantum may be concerned; but since they also suggest experimental tests by means of which the rival theories may be compared, and there is hope that the experiments will shortly be tried, it is reasonable to leave the discussion at this point in the hope that a few months' work will provide a more definite answer

Richards' series of papers form at once the most complete and the most coherent of the studies on the acoustic properties of gases, so that it was thought better to describe their contents in sequence. In doing so a good deal of parallel or earlier work on allied problems, particularly on the redistribution of energy in the molecule from translational to internal degrees of freedom, has been passed over, and though some of the points there have already been raised, it is desirable to return to them. Kneser (*Ann. Phys.*, **11**, 777, 1931; *ibid.*, **16**, 337, 19; *Phys. Zeit.*, **33**, 179, 1931) showed that in carbon dioxide, sound waves of high frequency had an apparently abnormal velocity (becoming constant 3·77 per cent. above the normal at ca. 5×10^5 cycles/sec.) due to the time lag before complete energy redistribution could occur, the vibrational quantum lifetime being $1{\cdot}0 \pm 0{\cdot}2 \times 10^{-5}$ sec. Kneser and Zühlke (*Z. Phys.*, **77**, 649, 1932) extended these observations and showed that in the frequency region of from 10^5 to 10^6 cycles the velocity of sound increases by about 4 per cent. and then remains constant, the effect being due to the time lag of the transverse oscillations of the molecule, which, at high frequencies, are not excited and do not contribute to the specific heat. Pielemeier (*Phys. Rev.*, **41,** 838, 1932) from studies both on the absorption and velocity of sound in CO_2, of frequencies from 3×10^5 to 2×10^6 cycles assigned a time lag of from 10^{-7} to 10^{-6} for the transfer of energy between internal and external degrees of freedom. For oxygen, Henry (*Nature,* **129,** 200, 1932) shows that a relaxation time of about 10^{-4} sec. is required. This effect of the time lag is now apparently well established, and may prove most important in helping to analyse the kinetics of homogeneous gas reaction. It may be mentioned here that the greatest difficulties inherent on theories of this subject are due to our very imperfect understanding of molecular collision phenomena, so that a new line of attack, such as this, is exceedingly useful.

Finally, to conclude this attempt to summarise the various applications of sound in chemical problems, its classical use in determining the thermodynamic properties may be mentioned Exact values of these constants are of importance both to the physicist and chemist, and indeed one of the very earliest applications was made when the ratio $\frac{Cp}{Cv}$ for rare gases was made by Ramsay, and they were shown to be monatomic. Keesom, Van Itterberck, and Van Lammeren (*Proc. Acad. Sic. Amsterdam*, **34,** 996, 1931) have measured sound velocities in oxygen at low temperatures, and have shown γ, *i.e.* $\frac{Cp}{Cv}$, to increase from 1·400 at 0° C. to 1·408 at the temperature of the liquid gas. The value of the second virial coefficient could also be found. Similar measurements by van Itterberck and Keesom (*Proc. Acad. Sic. Amsterdam,* **34,** 988, 1931) on hydrogen show a value for γ of 1·67 at about 19° C., and Keesom and Van Lammeren (*Proc. Acad. Sic. Amsterdam,* **35,** 727, 1932) have also made measurements on nitrogen over a wide range of low temperatures.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

GEOLOGY AND PETROGRAPHY OF IGNEOUS ROCKS—EXTRA-EUROPEAN.—While Dr. Niels Nielsen in his work entitled, "Contributions to the Physiography of Iceland, with particular Reference to the Highlands West of Vatnajökull" (*D. Kgl. Danske Vidensk. Selsk. Skr., Naturv. og Math. Afd.*, **9**, Raekke IV, 5, 1933, pp. 187–287, 32 pls., 9 maps), deals mainly with problems of physical geology and glaciology, he has also recorded many valuable volcanological and stratigraphical data. A full review of this memoir by Dr. M. A. Peacock is given in the *Geog. Rev.*, April, 1934, 345–6.

Dr. and Mrs. Hawkes describe the petrology and mode of intrusion of the Sandfell Laccolith in south-eastern Iceland (*Quart. Journ Geol. Soc.*, LXXXIX, 1933, 379–400). A phyolitic magma was intruded at a depth of 1,800 feet below the surface of a basaltic plateau which, at the same time, was in process of being built up. As intrusion continued a "dome of elevation" was formed, at first symmetrical, but later broken by a fault fracture on its north-eastern side. Submergence of the dome by lavas and tuffs followed; then came dike injection and a westward tilting of the whole structure.

The writer has described the petrography of some Kainozoic igneous rocks, and of rocks from the Cape Parry alkaline complex, East Greenland, collected by the Wordie Expedition of 1926 (*Geol. Mag.*, LXIX, 1932, 520–7). New analyses of basalt and monchiquite of undoubted Tertiary age, and of spherulitic riebeckite-trachyte from C. Parry, are recorded. Some of the rock types seem to indicate the existence of central volcanoes similar to those of Skye, Ardnamurchan, Mull, and Arran. This conjecture has been strikingly confirmed by Mr. L. R. Wager's discoveries during the recent British Air Route Expedition to Greenland (see Chapman, *Northern Lights*, 1932, Appendix on Geological Results by L. R. Wager).

H. G. Backlund and D. Malmqvist have described the geology and petrology of the basic members of the (Kainozoic) "basalt-formation" of north-eastern Greenland, with the aid of new analyses of dolerite (a sill), plagioclase-basalt and olivine-trachybasalt (lavas) (*Medd. Grönland*, **87**, Nr. 5, 1932, 61 pp.). The authors indicate the existence of ring structures in this region.

In his paper entitled "Zur Kenntnis des Peridotits von Kaersut (Grönland) und seines Ganggefolge," F. K. Drescher (*Min. u. Petr. Mitt.*, **43**, 1932, 207–70) describes a sill of peridotite associated with "dikes" of dolerite, veins of kaersutite-pegmatite, and aplitic vein-fillings, which collectively present a suite remarkably similar to the peridotite, picrite, teschenite, and analcite-syenite suite of western Scotland.

The central part of the Rest Island Granite of Minnesota and Ontario is described by J. H. Cram (*Journ. Geol.*, XL, 1932, 270–8) as a highly quartzose biotite-soda-granite, whilst the borders are composed of shonkinite. Ordinary crystallisation-differentiation is invoked to explain the arrangement of the component parts of the intrusion.

By collecting the available analyses of igneous rocks and adjacent sediments in Minnesota Professor F. F. Grout (*Journ. Geol.*, XLI, 1933, 196–218) shows that they fail to support the early view that the magmas of Minnesota were formed almost in place by hydrothermal fusion of older rocks, although some degree of assimilation and a considerable amount of injection, have taken place in some instances, leading to an apparent transition between igneous rocks and country rocks. Grout further discusses the evidence for actual fusion of crustal rocks at magmatic temperatures.

J. T. Stark's paper on " Igneous Rocks in the Baraboo District, Wisconsin " (*Journ. Geol.*, XL, 1932, 119–39), is a study of certain Pre-Cambrian rhyolites which, in the past, had been variously misinterpreted as " breccia " and " granite."

The Tertiary igneous activity of the Stockton and Fairfield Quadrangles, Utah, as described by J. Gilluly (*U.S. Geol. Surv., Prof. Paper*, **173**, 1932, 171 pp), began with the ejection of thick masses of latite breccias and tuffs, followed by more than 3,000 feet of lavas consisting of latite, rhyolite-obsidian, and, rather surprisingly, of nepheline-basalt and olivine-basalt. Monzonitic intrusives represent the plutonic equivalents of the lavas. An excellent example of a ring-dike surrounding a breccia-filled vent is described.

Dr. A. A. Fitch discusses the Sierra Nevada as a co-magmatic region on the basis of the Niggli magmatic type system (*Amer. Journ. Sci.*, XXIV, 1932, 481–95). It is shown that, since the Early Mesozoic, this region has been characterised by a suite of igneous rocks for which al, fm, and $c + alk$ are sensibly invariant, while c and alk have a reciprocal relation, the rocks becoming more alkalic with time. The Sierra Nevada co-magmatic region was individualised at the beginning of the present orogenic cycle, in the Trias, and has preserved its identity until recent time.

It is impossible to summarise here Professor Howel Williams's fine memoir on the " Geology of the Lassen Volcanic National Park, Cal." (*Univ. Cal. Publ., Bull. Dept. Geol. Sci.*, **21**, No. 8, 1932, 195–385). It provides a very comprehensive statement of the volcanic phenomena, geological succession, and petrographical characters of a region that has become classical for the study of igneous geology. Ten new chemical analyses of the Lassen lavas are published.

According to Professor H. Williams, Mt. Shasta, another famous volcano of the Cascade Range, is a typical strato-volcano composed mainly of andesitic and basaltic lavas with minor flows of dacite (*Journ. Geol.*, XL, 1932, 417–29). After the mountain had been built up to nearly its present height plug-domes and cinder-cones were formed along a N. to S. fissure. *Nuées ardentes*, a crater-plug, and extensive lava flows took place from Shastina, a later parasitic cone The last eruption of Shasta, which probably occurred only a few centuries ago, left a thin cover of pumice over the whole mountain.

The volcanic history of the interesting obsidian volcano of Glass Mountain, North California, has been investigated by C. A. Anderson (*Amer. Journ. Sci* , XXVI, 1933, 485–506). One of the illustrations is an aerial view of Glass Mountain which shows to perfection, not only the configuration of the lava flows and domes, but also the tortuous flow lines on their surfaces

The volcanic history and the petrography of the lavas of the Modoc Lava-bed Quadrangle, Cal., are exhaustively dealt with by H. A. Powers (*Amer. Min.*, **17**, 1932, 253–94) aided by many new chemical analyses. The lavas range in composition from olivine-rich basalts to rhyolites. Their mineralogy and chemistry conclusively relate them to the lavas of the Cascade Range.

The thick Tuscan formation of North California consists of volcanic breccias and tuffs which are believed to be of Pliocene age, and to be connected with Cascade vulcanicity. Dealing with the geology of the formation C. A. Anderson also gives a valuable discussion of the origin of volcanic breccias (*Univ. Cal. Publ., Bull. Dept. Geol. Sci.*, XXIII, No. 7, 1933, 215–76). The nature and extent of the Tuscan breccias suggest the hypothesis that they were distributed by mud-flows consequent upon the melting of ice and snow during early Cascade eruptions.

O. D. von Engeln describes two little-known explosion-craters (the Ubehebe Craters) on the western side of Death Valley, California (*Journ. Geol.*, XL, 1932, 726–34). Some concentrically-spalled, cracked and split pebbles in the explosion-breccias are explained as due to sudden great heating in volcanic blasts.

E. Callaghan discusses the succession and petrographical characters of the volcanic rocks which build up the Cascade Range in Oregon, with the aid of an instructive series of chemical analyses, some of which are new (*Trans. Amer. Geophys. Union, 14th Ann. Meeting*, 1933, 243–9).

Gabbro, hornblende-quartz-diorite, biotite-quartz-diorite, trondhjemite, and albite-granite, are the abundant plutonic rocks of eastern Oregon. The biotite-quartz-diorite is less sheared than the other types and may belong to a later igneous cycle. This hypothesis has been tested by study of the heavy mineral residues, by J. C. Reed and J. Gilluly (*Amer. Min.*, **17**, 1932, 201–20), in which it was found that the rock in question contained a distinctive mineral assemblage, and that the other types, although with less distinctive minerals, probably belong to a different, but consanguineous, series.

M. G. Hoffman has studied the structural features of the Columbia River basaltic lavas of the Moses Coulee in central Washington (*Journ. Geol.*, XLI, 1933, 184–95). This exhibits a 1,900-ft. section of lavas resting on Upper Miocene sediments. The lavas show various types of columnar structure, cross-jointing, fan-like forms, rosettes, ellipsoidal and brecciated forms. The conditions of formation of the latter are fully discussed. Thick and extensive lavas are ascribed to emission from numerous small fissures in the crust above a shallow intrusive mass.

Bald Butte Ridge, Washington, according to the same writer (*Journ. Geol.*, XL, 1932, 634–50), is a stock of the Idaho Batholith which protrudes through late Tertiary basalts. Its rocks form a petrographic series and have crystallised in the following order: hornblende-biotite-tonalite, biotite-tonalite, aplite dikes, quartz-gabbro dikes, pegmatite dikes, and quartz dikes.

A. F. Buddington and J. G. Fairchild describe the Eocene volcanic rocks of south-eastern Alaska (*Amer. Journ. Sci.*, XXIII, 1932, 490–6). These eruptives occur within a belt 200 miles long by 20 miles wide, and consist of olivine-basalts, labradorite-basalts, and andesites, together with a series of quartz-keratophyres, riebeckite-microgranites and rhyolites. Ten new analyses are recorded.

The igneous rocks of Fernando Noronha are described by W. Campbell Smith and C. Burri (*Schweiz. Min. u. Petr. Mitt.*, XIII, 1933, 405–34). The types described are nosean-phonolite, alkali-trachyte, nepheline-basalt, and nepheline-basanite, with monchiquite and gauteite dikes. Essexite occurs but has not been found *in situ*. Trachyandesite, limburgite, and tephritic trachybasalt, are also recorded. The series compares closely with that of the Bohemian Mittelgebirge. Five new chemical analyses are published.

The basalts of Patagonia, some of which are described by the present writer (*Journ. Geol.*, XL, 1932, 374–83), seem to belong to those slightly undersaturated types which are characteristic of "plateau-basalts" in the restricted sense of J. W. Gregory and H. Reck. This type contrasts with that of "flood-basalts" which, in general, are oversaturated with silica.

An exhaustive petrographical study of the lavas and associated hypabyssal and plutonic rocks of Teneriffe, La Palma and Grand Canary, has been made by Mme. E. Jérémine (*Bull. Soc. Min. Franc.*, LVI, 1933, 189–261), with the aid of 24 chemical analyses, the majority of which are new. The three islands are shown to be built of alkaline lavas ranging from ultrabasic to acid magmatic types, resting on a plutonic basement complex which is composed of nepheline-syenites in Teneriffe and Grand Canary, but of dolerite and gabbro in La Palma.

In the writer's " Petrographical Notes on Rocks from the Gulf of Guinea " (*Geol. Mag.*, LXXI, 1934, 16–23) oceanite and biotite-trachyte lavas from Annobon Island (with new analyses), tinguaites from Principe Island, and gneisses and amphibolite from the mainland of Spanish Guinea (Rio Muni), are described.

In two papers on East African volcanic rocks Mme. E. Jérémine (" Roches volcaniques de la bordure occidentale du lac Rudolphe," *Comptes Rendus, Paris*, **198**, 1934, 673–5 ; " Note sur quelques roches de la Colonie de Kényа," ibid., 838–40) describes, with new chemical analyses, pantelleritic pitchstone, phonolite, nepheline-syenite, basalt, basanitoid, and ankaramite from L. Rudolf ; oceanite and phonolite from Kenya.

F. D. Adams and F. F. Osborne describe two nepheline-sodalite-syenites from new localities in Northern Rhodesia (*Canad. Journ. Research*, **6**, 1932, 571–6). The low content of binary oxides and the presence of aluminous ægirine render these rocks of petrographical interest. They are chemically related to lavas at the northern end of L. Nyassa and probably belong to the same petrographic province. Further specimens of these interesting rocks are described by the same authors from another locality in Northern Rhodesia (*Amer. Journ. Sci.*, XXVII, 1934, 135–45), along with scapolite-amphibolites thought to have been derived by the contact action of nepheline-syenite magma on gabbro.

In his paper, " Zusammensetzung und Genesis der Alkaligesteine Südafrikas," S. J Shand (*Min. u. Petr. Mitt.*, **44**, 1933, 211–16) states that he finds Daly's " limestone-syntectic " theory most valuable in explaining the origin of the alkaline rocks of South Africa. The occurrences at Pilansberg, Sekukuniland, and Palabora are regarded as representing different stages of the assimilation process.

P. Niggli and B. Lombaard exhaustively discuss " Das Bushveld als petrographische Provinz " (*Schweiz. Min. Petr. Mitt.*, XIII, 1933, 110–86) on the basis of an accumulation of over 100 analyses including 21 new analyses made for the purposes of this memoir.

They come to the conclusion that the granitic magma of the Bushveld lopolith has probably been generated from the basic magma by a continuous process of crystallisation-differentiation, and that assimilation has played at most a subordinate part.

S K. Chatterjee deals with the petrography of the igneous rocks of the West Gir Forest, Kathiawar, India (*Journ. Geol.*, XL, 1932, 154–63). Both basic and acid types are developed and are associated in flows. The lavas and minor intrusions, however, are mostly basic and are contemporaneous with the Deccan Traps, although they differ from them in several features which are peculiar to Kathiawar. Seven new analyses are given. The suite seems to have resemblances to that of the Tertiary volcanic centres of the West of Scotland.

In his "Study of the Granitic Intrusions with their Associated Rocks in Ranchi and Singbhum Districts, Bihar and Orissa," L. A. N. Iyer (*Rec. Geol. Surv India*, LXV, Pt. 4, 1932, 490–533) records interesting granitisation phenomena, hybrid rocks and synantectic minerals in granites and hornblende-schists, with no fewer than 18 new analyses. The latest stage of igneous activity in the region is represented by dolerite dikes of which 5 analyses are given

In two recent papers C. Burri and H. Huber ("Geologie und Petrographie des jungvulkanischen Gebietes am Lower Chindwin, Upper Burma," *Schweiz. Min. Petr. Mitt.*, XII, 1932, 286–344; "Grundzüge der Geologie im Burma mit besonderer Berücksichtigung des jungen Vulkanismus," *Zeitschr. f. Vulk.*, XV, 1933, 153–79) provide a most useful summary account of the geology, tectonics, volcanology, and petrography of Burma, with special reference to the latest (Tertiary) period of volcanicity. New analyses of olivine-basalt, pyroxenolite, and pyroxene-hornblendite from the Lower Chindwin district are recorded.

The lavas of Mauritius, as described by S. J. Shand (*Quart. Journ. Geol. Soc.*, LXXXIX, 1933, 1–13), provide yet another example of the association of basalts with trachytes and phonolites in oceanic islands. An hypothesis of the derivation of trachyte from olivine-basalt by "gas-streaming" is put forward. If, however, the "pacificite" type of basalt is the parent magma then phonolite is to be expected as a late differentiate. Pacificite is a doubtful rock type as anemousite feldspar, on the occurrence of which the type is based, is now suspect (see C. E. Tilley in discussion of Shand's paper, p. 12, *cit. supra*, and Beliankin's work on anemousite, for which see summary, SCIENCE PROGRESS, April 1934, p.

Kerguelen, that almost exact counterpart of Iceland in the Indian Ocean, has been the subject of a comprehensive monograph by E. Aubert de la Rue (*Rev. géogr. phys. et géol. dynam., Paris*, V, fasc. 1 and 2, 1932, pp. 1–231, 25 pls., 2 maps) in which the geography, physical geology, stratigraphy, and petrography of the archipelago are fully treated. The lavas of which Kerguelen is composed are mostly basalts of a mildly alkaline cast, associated with domes of trachyte and phonolite Plutonic types such as nepheline-syenite, gabbro and monzonite, have been found in the western part of the main island. Nine new chemical analyses are recorded.

In a paper entitled "The Geology of Marysville, Victoria," E. S. Hills (*Geol Mag.*, LXIX, 1932, 145–66) describes Upper Devonian granodiorites intruded into a series of lavas which comprise nevadites, toscanites, meta-andesites and meta-basalts. Interesting contaminated and hybrid types are described. The work is accompanied by 12 new chemical analyses.

The Upper Devonian igneous rocks of the Warburton Area of Victoria are dealt with in two papers by A B. Edwards ("The Geology and Petrology of the Warburton Area, Victoria," *Proc. Roy. Soc. Vict.*, XLIV, Pt. 2, 1932, 163–81 ; "On the Dacite-Granodiorite Contact Relations in the Warburton Area," ibid., 182–99). They comprise an acid suite of lavas including soda-rhyolites and several types of dacite. Granodiorite is intruded into dacite lavas, and has produced an interesting series of contact-metamorphic rock types. Five new chemical analyses are given.

Again, in the Black Spur (Healesville) district of Victoria, Mr. Edwards has demonstrated the existence of a series of acid lavas of Devonian age, associated with a mass of granodiorite containing xenoliths of dacite and porphyrite (*Proc. Roy. Soc. Vict.*, XLIV, Pt. 1, 1932, 49–76). A magmatic reaction-relation between hypersthene and biotite is demonstrated in the dacites. Eleven analyses are given of which four appear to be new.

F. J. Turner has written an account of the petrography of tinguaites and camptonites from the vicinity of Haast Pass, New Zealand (*Trans. N.Z. Inst.*, **61**, 1932, 215–29). These rocks are shown to be later than the Lower Cretaceous orogeny of the region.

BOTANY. By PROFESSOR E. J. SALISBURY, D.Sc., F.R.S., University College, London.

ECOLOGY.—Interesting details regarding the biology of the biennial Melilotus alba are furnished in a paper by J. N. Martin (*Amer. Jour. Bot.*, **21**, 140–159, 1934). Germination in the wild state occurs at the end of March and the cotyledons become raised one or two inches above the surface, but owing to subsequent contraction in the radicle and hypocotyl the cotyledonary node is brought below the ground level. The plumule forms the first-year shoot whilst buds in the axils of the cotyledons, already present in the resting seed, form the " crown buds " which in the second year grow out into the flowering shoots. Data are furnished which show that shoot growth occurs mainly in the early part of the growing season whilst root growth takes place chiefly in September. There is a high degree of correlation between root weight and the diameter of the stem (+ 91). The seasonal distribution of shoot and root growth appears to be independent of soil differences or competition, but the latter appears to depress root growth more than shoot development. Removal of the shoot prior to, or at the beginning of, the period of root development not only had an inhibiting effect on root growth but greatly increased the winter mortality. Crown-bud development and hence flowering is markedly correlated with the growth of the root system.

The vegetation of the Great South Lake Region is the subject of an account by S. Flowers (*Bot. Gaz.*, XCV, No. 3, pp. 353–415). At Midlake, where the precipitation is only 5·39 inches, the annual evaporation is equivalent to about 69 inches. The water of the lake contains about 20 per cent. of salt which is slightly less than that of the Dead Sea. Blue-green Algæ are the chief plants present with *Aphanotheca utahensis* as the dominant species. *Microcystis* and *Oscillatoria* also occur in the most saline areas. Chlorophyceæ are represented by a species of *Chlamydomonas* and *Tetraspora lubrica*. The salt flats are occupied by *Salicornia*, *Suaeda* and *Allenrolfea*, of which the first forms a belt where the average salinity is from 2 to 3 per cent. with a maximum of 6·5 per cent. ; the Suaeda zone averages 1 to 2 per cent. (maximum 4 per cent.) and the *Allenrolfea* zone 1 to 1·5 per cent. (maximum 3 per cent.) Species of *Atriplex*, *Sarcobatus* and *Kochia* grow on the alkaline plain where the salinity is between 0·6 and 1 per cent. Myxophyceae occur in the hot mineral springs where the water temperature attains 61° C.

From a study of the conditions of growth and reproduction at the forest margin in Alaska and from pollen analyses from the underlying peats R. F. Griggs and P. W. Bowman conclude that the existing forest limit is of recent origin and is still advancing. Reasons are given for attributing this to the secular changes since the last glaciation, equilibrium not having yet been attained since the retreat of the ice sheet (*Ecology*, XV, 80–100, 1934).

The mixed types of climax forest which characterise the Cranberry Lake region of the Adirondack mountains are found by V. H. Young to include a number of diverse communities with which are associated marked differences in the soil characters. The soil heterogeneity is regarded as determining the dominant species in the various habitats studied. This conclusion, if valid, emphasises the importance of recognising that in the same climatic area a considerable diversity of soil conditions may become evolved (*Ecology*, XV, 154–96, 1934).

The soil temperatures of sand-dunes in relation to the vegetation is the subject of a paper by Salisbury (*Trans. Norfolk and Norwich Nat. Soc.*, XIII, 333–55, 1934). Appreciable differences are shown to result from differences in the type of vegetation cover; from the colour of the sand, due to the amount of organic material on the surface, and from topography. The conductivity of the sand is an important factor varying with content of organic material and the moisture present. Owing to the high surface temperatures attained by the sand, the air temperatures immediately above the sand surface may be 10° C. to 15° C. higher than that of the air generally even in spring. The importance of these high air-temperatures for the assimilation of the winter-annuals is emphasised since such may well enable them to assimilate actively when plants in other habitats are below the critical temperature for that process.

An interesting paper by F. J. Richards (*Ann. Bot.*, XLVIII, 225–59, 1934) gives a statistical analysis of data regarding the rate of accretion, erosion and horizontal extension of salt marshes in the Dovey Estuary. As is to be expected the accretion is slow at first increasing with the density of the vegetation, but subsequently decreasing with the rise in height. In the *Armerietum* it is calculated that a rise of one foot would occupy from about thirty to eighty years. For the Festucetum rises of from 1·57 to 3·38 cms. in about two years were recorded and in the Juncus zone from 1·2 to 5·1 cms. in the same period.

CRYPTOGAMIC.—A very lucid account of the nuclear changes in a discomycetous fungus *Ascophanus Aurora* is given by Gwynne-Vaughan and Williamson (*Ann. Bot.*, XLVIII, 262, 1934). On germination the spores give rise to a haploid mycelium containing nuclei with two chromosomes. Globular antheridia are formed containing from four to ten nuclei and distended club-shaped oogonia containing from eight to twenty nuclei. The male nuclei pass down the unicellular trichogyne and fuse in pairs with the female nuclei. The oogonium then forms from one to four ascogenous hyphæ containing nuclei with four chromosomes. Two nuclei fuse in the normal way to form the ascus nucleus which is thus tetraploid with eight nuclei. The first and third divisions are reduction divisions producing eight haploid spores.

A synopsis of the European species of *Pogonatum* and *Polytrichum* by J. H. Albrecht (*Jour. Bot.*, LXXII, 75–80 and 104–110) is illustrated by details of leaf structure drawn by Mr. Sherrin. No new species are described but new sub-species are recognised under *P. aloides*, *P. urnigerum*, *P. formosum*, *P. commune* and *P. piliferum*.

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—A paper that will be of interest to all morphologists is one by R. E. Snodgrass on the genital ducts and the ovipositor of insects (*Smithsonian Misc. Coll.*, **89**, No. 8, 1933, 148 pp.).

An important contribution to our knowledge of the physiology of the cuticle and of ecdysis has been made by V. B. Wigglesworth in a paper (*Q.J.M.S.*, **76**, 1933, 269–318) dealing with *Rhodnius prolixus*. Special reference is made to the function of the œnocytes and of the dermal glands. The cuticle is shown to consist of two primary layers: a very thin epicuticle and a thick endocuticle of protein and chitin. The epicuticle is composed of material for which the name "cuticulin" is proposed and which is perhaps a complex mixture of fatty or waxy substances. The endocuticle is made up of two parts separated by a distinct horizontal line; of these only the outer is laid down before moulting. In the adults the outer part of the endocuticle is impregnated with cuticulin over the greater part of the abdominal tergites and sternites, but in nymphs the endocuticle of the abdomen is impregnated with cuticulin only below the little plaques which bear bristles. As a consequence, the stretching of the abdomen to receive large meals of blood is accomplished by a different mechanism in nymphs and adults.

In the formation of the new cuticle the non-chitinous epicuticle is laid down first and below it is laid down the chitinous endocuticle. This appears to be secreted in fluid form around filamentous processes by the epidermal cells which become the pore canals. The impregnation of the outer part of the endocuticle with cuticulin takes place gradually after the endocuticle is formed. The dermal glands are associated with moulting and afterwards degenerate. They are formed afresh at each moult from embryonic cells in the epidermis and secrete the moulting fluid whose chief function is to digest the old cuticle. All but about 14 per cent. of the old cuticle of the abdomen is digested and absorbed, but the epicuticular structures are not affected. Experimental evidence is given showing that there is a continuous circulation of the moulting fluid, and that the products of digestion are absorbed through the general surface of the new cuticle which does not develop its waterproof properties until the time of moulting. The dermal glands appear to play no part in the formation of the new cuticle.

Insects may be killed at high temperatures either on account of the heat or by desiccation. Death by desiccation is liable to occur in insects which cannot conserve the water in their bodies, as the evaporative power of the air is greater at high than at low temperatures. K. Mellanby (*J. Expt. Biol.*, **11**, 1934, 48–53) has now shown that starvation is a third factor causing death at high temperatures Unfed lice and mosquitoes could not withstand as high temperatures as they could when fed. This is stated to be so because they had such small food reserves and at high temperatures their rate of metabolism was so increased that they died of starvation.

The annual address of the president of the Hawaiian Entomological Society for 1932, delivered by R. N. Chapman (*Proc. Haw. Ent. Soc. 1932*, **8**, 1933, 279–98), was entitled "The Causes of Fluctuations of Populations of Insects." It deals with the author's *Tribolium* studies. Two of Volterra's laws, that of the conservation of the averages and that of the periodic cycle, have already been substantiated. In addition it has been shown that any advantage or disadvantage which individual members of a population may possess will be lost in the next generation unless inherited.

The efficiency of estimations of insect populations by means of the method of collecting by sweep nets has often been called into question, but the insect net is apparently still largely used. H. E. Gray and A. E. Treloar (*Ecology*, **14**, 1933, 356–67) have made a statistical attempt, after sweeping in an alfalfa plot, to determine the accuracy of this technique. They find that the use of the sweeping method for purposes of comparing different types of environment under the same conditions, or the same type of habitat under different conditions, has little quantitative value unless based on extensive collections. They state that for a reduction of the error range to 10 per cent. the number of collections must vary from approximately 124 to 1,079, involving from 3,100 to 26,975 sweeps of the net.

Another study (*J. Counc. Sci. and Ind. Res.* (*Australia*), **6**, 1933, 145–59) on the seasonal fluctuations of insects has appeared recently. It deals with *Thrips imaginis* Bagnall and associated blossom thrips.

The writer, J. W. Evans, formed the opinion that the numbers of individuals comprising the first spring generation determine the possibilities of a subsequent infestation. He found that the numbers present in this generation are dependent especially on the meteorological conditions during the preceding four months. He further concluded that the control by parasites or predators at any one time or in any place appears to be of very little account beyond being of academic interest.

G. Salt has contributed two papers dealing with experimental studies in insect parasitism. These experiments are designed to provide data for the better understanding of the numerical interaction that goes on generation after generation between the parasite and its host. Up to the present such data are scarce and such workers as V. Volterra, W. R. Thompson and V. A. Bailey, who have dealt with the problem from a mathematical standpoint, have based their work on field observations. The last named has recently (*Proc. Roy. Soc.*, A, **143**, 1933, 75–88) considered the interaction between several species of host and parasites. The first of Salt's papers (*Proc. Roy. Soc* , B, **114**, 1934, 450–4) describes the technique to be employed; this aims at providing a controlled and uniform spatial exposure of the hosts for a definite period of time under controlled environmental conditions. The insects to be used are the parasitoid *Trichogramma evanescens* Westwood and the moths *Sitotroga cerealella* and *Ephestia kühniella*. In his second paper (loc. cit., 455–70) superparasitism is discussed. Evidence is brought forward to prove that *Trichogramma* can distinguish between unparasitised and parasitised hosts and avoids ovipositing in the latter. They can also retain their eggs rather than deposit them in parasitised hosts. Thus the hypothesis that the progeny of parasitoids are distributed at random without reference to the previous parasitisation of the host is untenable for this species.

Nellie M. Payne has been studying the differential effect of environmental factors upon *Microbracon hebetor* Say (a Braconid) and its host *Ephestia kühniella* Zeller (*Biol. Bull.*, **65**, 1933, 187–205). Two strains of the moth, one developing much faster than the other, were used. Several interesting facts concerning host-parasite balance are recorded. For instance, in general high temperatures (above 32° C.) and high relative humidity favour the host while low temperatures (15° C. and below) and low relative humidity favour the parasite. The relative speeds of development and the time of emergence of the parasite are shown to be important in the question as to whether the parasite can exterminate the host or not. For example, if the parasite emerges at the beginning of the pupal period of the host it can exterminate the slow strain of host only at 27° C., but it can overtake the fast strain at temperatures from 27° C. to 20° C. inclusive. On the other hand, if the parasite emerges during the larval stage of the host and can complete two generations during this stage, it can exterminate the host. Similarly if the adult parasite emerges at the beginning of the egg stage it can exterminate either strain of host except at 15° C. and below.

The recent outbreak of the Colorado beetle at Tilbury in the autumn of 1933, of which an account has been written by J. C. F. Fryer (*J. Min. Agric.*, **40**, 1934, 907–12), has brought to the front the whole question of legislative attempts to prevent the entry of undesirable insect pests. This method of insect control is in some countries rather reminiscent of the Cinderella of legend. In California however plant quarantines, like biological control, have been highly developed. A few years ago a committee of distinguished entomologists, plant pathologists and agricultural economists under the chairmanship of H. S. Smith was appointed to make a study of the efficacy and economic effects of plant quarantines in California. Their report (*Univ. California Bull.*, **553**, 1933, 276 pp.) has now been published. The committee seems to have considered every aspect of the question most thoroughly, both from the general and particular viewpoints On the whole, although several minor alterations are recommended, the report is distinctly favourable.

Two aspects of biological control work are worthy of note. H. Womersley (*J. Counc. Sci. and Ind. Res.* (*Australia*), **6**, 1933, 83–91) suggested after a preliminary investigation that the Bdellid mite, *Biscirus lapidarius* Kramer, might be a possible control of the clover springtail or lucerne flea in Western Australia. Incidentally this appears to be one of the first cases of an Acarine being used for biological control. ' Many years ago (1873) a mite was brought in to control the *Phylloxera* on grape vines in France. G. A. Currie (loc. cit., **7**, 1934, 9–20) in a survey of some of the agricultural areas of Western Australia has found evidence supporting Womersley's claim that in certain cases the mite does control the lucerne flea.

P. A. Buxton in 1927 suggested introducing mosquitoes of the genus *Megarhinus* (whose larvæ are predaceous) into Samoa and Fiji for the control of another mosquito *Aëdes scutellaris* (*variegatus*) which transmits human filiaris. R. W. Paine (*Bull. Ent. Res.*, **25**, 1934, 1–32) has now recorded successful introductions of *M. splendens* (in 1931) and *M. inornatus* (in 1933) into Fiji and gives an account of the spread of the former species since its introduction.

COLEOPTERA.—Dorothy J. Jackson (*Ann. Appl. Biol.*, **20**, 1933, 731–70) has made a study on the flight muscles of *Sitona* weevils.

In macropterous weevils of *S. hispidula* marked variation occurs in regard to the development of these muscles, some individuals being able to fly while others are permanently flightless. The histology of the fibrous muscles is entirely different in these two forms. Miss Jackson finds that in newly emerged adults of *S. lineata*, an excellent flier, the condition of the flight muscles and their chitinous supports closely resembles that of the flightless form of *S. hispidula*. However in *S. lineata* the normal condition is usually attained a short time after emergence. The flightless macropterous weevils of *S. hispidula* can therefore be regarded as individuals in which the normal post-metamorphic development of the flight muscles and their supports has been arrested. Evidence obtained by breeding *S. hispidula* suggests that unfavourable environmental conditions during ontogenetic development may be responsible for the production of the flightless weevils.

LEPIDOPTERA.—It has been suggested that arsenic resistant strains of codling moth larvæ were being developed. This arose from the fact that greater difficulty is being experienced in controlling the Colorado strain of larvæ by means of arsenical sprays than the Missouri and Virginia larvæ. L. Haseman and R. L. Meffert (*Univ. Missouri Agric. Expt Sta*, *Research Bull 202*, 1933, 11 pp) have been injecting measured doses of arsenicals directly into the digestive tract and into the hæmocoele of such larvæ. They have found that there is no marked difference in the three strains as regards susceptibility to arsenic.

HEMIPTERA.—W. E. China (*Ann. and Mag. Nat. Hist.*, **12** (10th series), 1933, 180–96) has constructed a "family tree" for the Hemiptera-Heteroptera based on recent studies and showing their relationships and origin. This is an important contribution on the phylogeny.

A study of colour and colour variation in *Aphis gossypii* Glover has been made by R. E. Wall (*Ann. Ent. Soc. Amer.*, **26**, 1933, 425–64). Among some interesting results, one notes that body size and length of appendages are directly correlated with colour in the individual, the darker forms being larger. Moreover, a considerable number of the light forms show a reduction in segmentation of the antennæ in addition to a diminution in general size.

HYMENOPTERA.—The flight range of the honey bee has received the attention of J. E. Eckert (*J. Agric. Res.*, **47**, 1933, 257–85). Various other writers have estimated the range to be from one to seven or more miles but this author has presented experimental evidence. When bees were separated from a given nectar-producing area by land which possessed no source of food, they flew a maximum distance of at least 8½ miles. But one of the most interesting results obtained was the fact that bees from apiaries situated within a nectar-producing area have a tendency to fly in only one or two major lanes of flight, neglecting similar forage plants in other directions. Bees would fly from 2·75 to 4 6 miles in one direction when located within a nectar-producing area and confine their efforts to working in that direction rather than in nearer fields of seemingly equal attractiveness.

DIPTERA.—R. P. Hobson (*Biochem. J.*, **37,** 1933, 1899–1909) in a paper on the growth of blowfly larvæ on blood and serum has dealt with the response of aseptic larvæ to vitamin B. They resemble higher animals in the vitamin B requirements, each of several substances in yeast being necessary in adequate amounts for growth. One of these growth factors is shown to be identical with vitamin B_1. The chief effect of vitamin B deficiency is shown to be retarded growth, vitamin B thus appearing to act as a catalyst of growth accelerating a reaction which proceeds at a slower rate in its absence. Further, experiments have shown that blowfly larvæ fail to develop on blood unless vitamin B is added or living bacteria are present; this helps to confirm the hypothesis that the function of the symbionts in blood-sucking insects is vitamin synthesis.

A systematic revision of the genus *Lucilia* R.-D has been made by Miss D. Aubertin (*J. Linn. Soc. London, Zool.*, **38,** 1933, 389–436).

OTHER ORDERS.—E. R. SPEYER (*Ann. Appl. Biol.*, **21,** 1934, 120–52) in a study of some common species of the genus *Thrips* has shown that there is considerable range of variation within the species of characters which have in the past been used for the separation of species, especially measurements of antennal joints and setæ on the body. New structural characters, notably the chætotaxy of the abdomen, of true specific value are given.

ARCHÆOLOGY. By E. N. FALLAIZE, B A.

EARLY MAN IN CHINA.—Further discoveries in the cave of Choukoutien, the home of Peking man, announced only a short while before the untimely death of Dr. Davidson Black, open up a vista of possibilities in the way of future discovery which emphasises the loss of his services not only to archæology and human palæontology in China, but to the science of prehistory in general. Dr. Black at the time of his death was engaged on the results of the previous season's work at Choukoutien; but a preliminary report had been presented to the Geological Society of China, of which an account appears in *Nature,* January 20, 1934. A pocket of grey sediments of apparently modern facies, situated above the Sinanthropus deposits towards the top of the hill, and known as the "Upper Cave" has yielded human skeletal remains and traces of an industry with abundant evidence of fire. The human remains consist of two almost complete, but somewhat crushed skulls, fragments of skulls, teeth, fragmentary lower jaws, bones of the upper extremity, vertebræ and leg and foot bones. The skulls are of modern type (*Homo sapiens*). The artefacts are three stone implements of chert and quartz, a deer canon bone worked at both ends, thirty or more fox canine teeth perforated for a necklace, an ornamental cylindrical piece made from the long bone of a bird, and a quantity of oolitic hematite, probably imported from a distance. The associated fauna included *Hyæna* (extinct but differing from that of the Sinanthropus beds), *Felis tigris*, *Cynailurus* (now restricted to India), *Equus hemionus*, and *Cervus elaphus*. Both hyæna and elaphus show resemblances to specimens from the quaternary deposits of Sjara-osso-Gol. On lithological, stratigraphical and faunistic evidence these finds are clearly to be distinguished from those of the Sinanthropus deposits. They are pleistocene in age ; and the industry and human remains were assigned by Dr. Black to a late pleistocene date and palæolithic culture corresponding approximately to that of the upper palæolithic of Siberia and Europe. The industry appears to be slightly more developed than those of Ordos (Shui-tung-ko and Sjara-osso-Gol), described by P. Teilhard de Chardin and previously the only known palæolithic culture in the Chinese area, with which no typically worked bones have thus far been found in certain association.

The importance of this extension of the distribution of *Homo sapiens* of Pleistocene Age to China needs no emphasis, though a more precise description will be awaited with interest. It does not, however, exhaust the season's discoveries in the immediate neighbourhood of the Sinanthropus cave. In the limestone to the south of Locality I, among a number of fossilised bones in a peculiar red deposit, were several teeth and limb bones of a large baboon, stated probably to be *Cynocephalus wimani* Schlosser. Whether these represent a pre-Choukoutien stage was not clear at the time of reporting, but it points to a successive occupation at Choukoutien by baboons, Sinanthropus and a modern type of *Homo*.

PRE-CRAG MAN IN EAST ANGLIA.—Claims recently formulated by Mr. J. Reid Moir on behalf of the high antiquity of man in East Anglia are hardly less startling than the evidence of early man which has come from China. Mr. Reid Moir has been engaged in the further investigation of the Suffolk bone beds below the decalcified Red Crag at Ipswich and at Bawdsley on the sea coast. Ray Lankester's investigation of the fauna of the bone beds led him to the conclusion that the remains were derivative and of diverse ages ranging from miocene to pliocene. Mr. Reid Moir is of the opinion that an analogous diversity in age and origin is to be attributed to the pre-palæolithic implements found in these beds. His bases of classification are type and patination. He distinguishes no less than four groups of implements, in which the interval in time and the distinction in form between Group 1 and Group 4 is possibly as great as that between early Chellean and Acheulean. Further he has found a rostro-carinate encrusted with a deposit which, he holds, makes it probable that this implement was at one time embedded in Diestian sandstone. As it is clearly later in date than the flaking on the implements of Group 1 it would appear, Mr Moir concludes, that the earliest flint implements in these beds belong to some unspecified period preceding the Diestian. On typological grounds these pre-Crag implements are to be regarded as more advanced than the Harrisonian eolith, and the product of an appreciably higher development in culture, arguing, Mr. Moir would hold, a considerably earlier dating for the simpler and more primitive type.

PALÆOLITHIC MAN IN THE NILE VALLEY.—Although the importance of the Nile Valley from the point of view of the geologist and the archæologist have long been appreciated, it was not until 1926, when W. J. Arkell and K. S. Sandford were sent out by the Oriental Institute of the University of Chicago, that any attempt was made to survey it systematically as a whole. In the first three seasons' work a preliminary survey was made of the river banks between Luxor and Kom Ombo and in the region of the First Cataract, and the Faiyum and Delta were covered. In 1929–30 the expedition returned to Upper Egypt, traversing again and extending the ground examined in the first season. The results of this last survey have been embodied in a report on the geology and archæology of the 350 miles of river from Semnah to Luxor. (*University of Chicago Oriental Institute Publications*, vol. XVII: *Prehistoric Survey of Egypt and Western Asia*, vol. II.) No pliocene deposits were found south of Aswan. Plio-pliocene river platforms and terraces were followed from Wadi Halfa at 300 feet, 200 feet and 150 feet, for the greater part of the distance to Aswan. No implements of pliocene dating were found. Artefacts do not appear until the 100-foot terrace is reached. Terraces and implements are associated as follows: 100-foot terrace, Chellean types; 50-

foot terrace, Acheulean types; 30-foot terrace, Le Moustier I; 10-foot terrace, Le Moustier II; Basal part of silt, Le Moustier III. A notable feature of the industries is the variety of material used, ranging from chert to sandstone and ironstone. The Sebilian industry, which follows the Mousterian, is fully described and figured. No Aurignacian types were found. Sebilian, both Lower and Middle, show very clearly their Mousterian derivation; but the authors regard it as better to employ the term Lower Sebilian for the earlier type, rather than abandon it for a further qualification of Mousterian, here following M. Vignard. The Upper Sebilian, known only from surface finds, presents in general a marked contrast to Middle Sebilian It is an industry of little but microliths, and the authors conclude that a new and outside influence entered the culture in Upper Sebilian times They tend to associate this influence with some form of the Capsian and regard its appearance as in all probability a late event. This part of the Nile, in fact, remained a stronghold of the Mousterian technique long after it had been forgotten elsewhere. It also seems probable, judging from the evidence of the Nile levels, that the Upper Sebilian culture is to be related to the oldest drawings, in which giraffes appear, in a series of rock engravings which was discovered at Wadi el-Arab. If this attribution be accepted, it may relate the Capsian element to the movement from the Mediterranean to the south of Africa, for which the Abbé Breuil argues on the evidence of the rock-drawings of eastern Spain, north Africa, the Nubian Desert and the art of the Bushman

CAPSIAN CULTURE IN NORTH AFRICA.—The discussion of the character of the Upper Sebilian microlithic culture of the Nile Valley and its possible inclusion of a Capsian element should be read in the light of an examination of the Capsian industry of north Africa by M. R Vaufrey in *L'Anthropologie*, vol. XLIII, Nos. 5–6, on the evidence of certain investigations made by him in the shell-heaps of southern Tunisia in 1931–32. M. Vaufrey, conducting his investigations in accordance with more exacting methods than those followed by previous investigators, arrives at the conclusion that the distinction between Lower and Upper Capsian based upon the rarity of microliths and the almost exclusive preponderance of the larger implement in the Lower Capsian, must be abandoned in view of the fact that the microlith, contrary to accepted opinion, is abundant and already highly developed in the earlier stage. The Capsian, he holds, is essentially one, but it is divisible into three stages, in which the microlith is the binding material. Of these the first stage is the typical Capsian, in which occur points, burins and scrapers with a characteristic microlithic industry. The second is an intero-getulian neolithic or Upper Capsian, in which the burin is exceptional and triangles and scalene points predominate among microliths; and the third is a neolithic of Capsian tradition, in which shell heaps are rare, the typical Capsian implement, the point *à dos rabattu* disappears and Saharan influence is introduced. The Capsian is thus shown to be late palæolithic or mesolithic and cannot be regarded as an ancestor of the Aurignacian culture.

THE NEOLITHIC AGE IN WESTERN EUROPE.—The implications of M. Vaufrey's reconsideration of the character and position of the Capsian industry are too numerous to consider here in detail. It will be noted, however, that while holding it to be essentially a late palæolithic or mesolithic culture, he carries it over into what he terms "neolithic," thus agreeing with recent tendencies to extend the mesolithic at the expense of neolithic. At the other extremity of what was at one time regarded as the most extensive in range of the prehistoric cultures, the earlier phases of the Bronze Age have also encroached on the province of the neolithic, so that in some views of the extent and relation of prehistoric cultures this phase has wellnigh disappeared. An attempt to redress the balance is made by Mrs. Jacquetta Hawkes in a review of the evidence for the distribution and character of the neolithic and chalcolithic cultures in western Europe, which appears in *Antiquity* for March last. Mrs. Hawkes takes as her starting-point an early culture which she identifies in south and east France. This she follows up the Rhône Valley to the western Swiss lakes, where it established itself during the first Danubian period, and passed to Britain, avoiding Brittany. This first period was followed by a period of differentiation, in which one offshoot, turning east, came under Danubian influence and acquired a distinctive character which is seen in the Michelsberg culture. Another offshoot towards the west came into contact with a stream of influence from southern France and, assuming a sophisticated character especially in its pottery, was responsible for the Chassey culture. Proceeding farther westward it then became one of the elements in the elaborate chalcolithic culture of Brittany. The Seine-Oise-Marne culture, of which the most characteristic feature is the vase with everted rim, well-marked shoulders and splayed foot, cuts across this culture from Belgium to the Channel Islands and shows affinities with the culture of the people who in the meanwhile after severe floods had resettled the Swiss lakes.

PREHISTORIC SETTLEMENT IN EAST ANGLIA.—Mrs. Hawkes' contribution to the study of the neolithic period on the Continent affords a starting-point for the discussion and interpretation of the pattern which may be developed from the confused and sometimes puzzling data of cultural and racial movement at a time when distributions are beginning to assume definition over relatively wide areas. The interpretation of the data of a more restricted field, which admits of intensive study in relation to geographical conditions, is essayed in two recent studies in regional archæology in the eastern parts of England. Of these, one is Dr. Cyril Fox's presidential address for 1933 to the Prehistoric Society of East Anglia (*Proceedings*, vol. VII, Pt. II), in which he deals with the distribution of prehistoric man in East Anglia between 2300 B.C. and A.D. 50. Postulating that soil is the chief factor in determining man's distribution, he shows that pervious soils, inducing an open type of country, favour the pastoral life and a scratch agriculture; while the heavier clay soils carrying forest with a dense undergrowth, are difficult to clear, and when cleared, difficult for primitive implements to cultivate. Another deterrent to human settlement, important in East Anglia, is a water-laden soil, such as that of the fens. Taking East Anglia as the country lying between the Wash and the Thames and bounded on the west by the line of 0·50 long. which includes the southern fens and the basin of the Lea, Dr. Fox shows that in this area the major factor is the plateau stretching in a curve parallel to the east and north-east coast, which is at its broadest and highest in the southern half of the region, and is linked in the south-west to the Chilterns. This forms the watershed of the whole area. Without attempting to follow Dr. Fox in detail it may be said that he demonstrates by distribution maps that settlement falls into six periods, extending from the "Axe Age" to Early Iron Age II. He shows further that the culture of each has a characteristic distribution, which can be related to the character of the soil. Thus in the "Axe Age" the areas of densest occupation are within the inner curve of the clay-covered watershed, and in the Thames lower valley and estuary. There is in fact in this period a close relation between distribution pattern and river valley, especially in the uplands. Even more interesting, however, is the distribution of the Bronze Age and its subdivisions in which the culture pattern distinguishes the intrusion of invaders and traders. In the later phases it is to be noted how the evidence, when submitted to Dr. Fox's analysis, points unmistakably to the utilisation of the heavier and more difficult lands as a more advanced tribal culture intrudes into, or is developed, in the area.

The first part of a study of the prehistoric archæology of Lincoln-shire on similar lines, by Mr. C. W. Phillips appears in *The Archæological Journal*, vol. XC, Pt. 1. Here the dominating features in the lie of the country are the Wolds, the oolite ridge known as Lincoln Edge, which running north and south half-way down the county is pierced at Lincoln Gap by the River Whitham, and two low-lying areas in the Fenland of the southern part of the county and the Isle of Axeholme on the west side of the Trent near its outfall to the Humber. The county is especially noteworthy as including within its boundaries at Risby Warren what is regarded as the type station of Great Britain for the Tardenoisian industry. The features previously noted in the cultural distributions in East Anglia, are repeated, in the occupation of the higher or more easily worked lands in the neolithic period and the gradual transfer of settlement, especially along the line of Lincoln Edge, to the heavier and more difficult if more productive lands in the later periods. That part of the record of the survey now published includes what has been one of the great archæological surprises of recent times within the county boundaries This is the discovery of nine, or ten, long barrows on the Wolds, of which the existence was not known before this survey was made Partial excavation of the Giants' Hills long barrow, Skendleby, has demonstrated that the builders were Windmill Hill folk and that Beaker folk arrived on the spot and lived in the ditch quite early in the history of its silting.

NOTES

The Hawke's Bay (N.Z.) Earthquake of February 3, 1931

(G. W. T.)

New Zealand is one of the recognised seismic regions of the earth, but until recently it had not suffered serious loss of life or damage to property through earthquakes. Severe earthquakes took place in 1848, 1855, 1901, but they occurred at times and in localities where the chances of serious destructive effects were small. The Buller Earthquake of 1929, which occurred in a sparsely-populated region, nevertheless cost seventeen lives and awakened New Zealanders to the possibilities of widespread disaster should the tremors occur in a populated area. These fears were unhappily realised by the world-shaking earthquake of February 3, 1931, which was centred on Hawke's Bay off the eastern coast of North Island in close proximity to the important towns of Napier and Hastings, and occurred at the unfortunate time of 11 a.m. when many people were in the streets and shops, business premises were full, and children were in school. It is therefore even a matter for surprise that the death-roll amounted only to 260 considering the magnitude of the shock. The damage to property, however, which was greatly increased by the fires that swept the devastated areas, is officially estimated at £2,500,000, although the total losses must have been much greater than this sum which merely represents the insured loss. These effects, although much smaller than those of some of the great earthquakes of the densely populated regions of Japan, China, and India, were without parallel in New Zealand, and constituted the greatest human disaster that the Dominion had experienced.

A comprehensive report on the Hawke's Bay Earthquake has been published as a special number of the *New Zealand Journal of Science and Technology* (XV, No 1, July 1933, 116 pp.), in which practically every aspect of the disaster has been discussed by a group of writers. This includes a General Description, by F. R. Callaghan (33 pp.) ; and papers on Geological Aspects, by J. Henderson (37 pp.) ; Effects of Earthquake on Coast-Line, by P. Marshall (13 pp.) ; Seismological Report, by C. E. Adams, M. A. F. Barnett, and R. C. Hayes (15 pp.); Damage to Buildings, by A. Brodie and A. G. Harris (8 pp.).

There is no need to recount here the gruesome details of this earthquake, but it is heartening to be able to record that while the people were still dazed and stunned by the paralysing suddenness of the shock, amidst the choking clouds of dust, and when great fires began to rage over the stricken towns, there was no panic and the work of rescue was begun at once by the citizens, aided 2½ hours after the disaster by bluejackets from H.M S. *Veronica*, and an emergency committee was constituted and was actively at work at 7.30 a.m on the succeeding day. Mr. Callaghan's report describes the shock in the principal towns affected, the rescue measures, the outside assistance (which ultimately became embarrassingly profuse), the effects of the earthquake on communications and public services, the emergency organisation that was set up, and the history of restoration and permanent rehabilitation of the devastated region. It is illustrated by effective photographs of Napier before and just after the catastrophe, and of a portion of the town in its ruinous condition compared with the same area after rebuilding.

In the next section Mr. J. Henderson, Director of the Geological Survey, deals with the geological aspects of the earthquake. He outlines the general geology of the Hawke's Bay area, and shows its relation to the great moving earthfold which forms the eastern border of North Island and constitutes the real margin of the South Pacific basin. A sub-oceanic ridge connects New Zealand with the Tonga Islands 1,600 miles to the north-east and, closely hugging the south-eastern side of this ridge, is a deep and narrow trough which includes the Tonga and Kermadec deeps, both descending more than 30,000 feet below sea-level. The conformation of this double structure in plan and in profile is such that all geologists regard it as a large asymmetrical earthfold. The repeated movement of this fold to the south-east finds expression in seismic and volcanic phenomena along its whole length, and leads to the view that a new mountain range is in process of formation.

The eastern part of North Island is built of the crest and eastern flank of an enormous asymmetrical geanticline consisting of rocks ranging in time from late Palæozoic to Mesozoic. These are covered by 40,000 feet of Miocene and Pliocene sediments which are themselves strongly faulted but not severely folded.

The present mountains of New Zealand were uplifted during the Pliocene and are thought to have been raised by pressure due to the subsidence of the earth segment flooring the South Pacific Ocean. This force is still in operation and, distributed by innumerable minor crustal adjustments, permeates the whole of the New Zealand geanticline. The Hawke's Bay region intervenes between the actively rising and subsiding sectors. The deep-seated rocks opposing the force from the subsiding Pacific sector are more or less held by the New Zealand mountains, whereas those underlying the Hawke's Bay region are more free to move upward when the accumulating energy is able to overcome gravity and friction.

Mr. Henderson makes a penetrating analysis of the mechanism of movement of the earth block which caused the Hawke's Bay Earthquake, and shows the resemblance of the phenomena to those which have been produced by similar forces in artificial sediments by T. A. Link. Mr Henderson also describes the surface geological effects of the earthquake, the uplifts and depressions, fissures, shears, and swellings which were caused by the movements of an earth-block 60 miles long by 10 miles wide Re-levelling data show that this block was tilted gently and uniformly towards the north-west.

Mr. S. W. S. Strong briefly describes the uplift of Sponge Bay, Gisborne, and the Hangaroa mud blow-out ; and Dr. P. Marshall, at greater length, describes the striking effects of the earthquake on the coast-line near Napier Landslides from the cliffs, and changes both in the outlines of the coast and in depths of water, were extraordinarily abundant , 6,500 acres of new land were added to the coast by the uplift of a lagoon near Napier (states a later account), and this land has been divided into 300 farms which have been allotted to unemployed married men.

The first part of the Seismological Report deals with the relation of the seismology of New Zealand to that of the eastern Pacific, the general seismology of the Dominion, past seismic activity in the Hawke's Bay region, and the foreshocks of the February 1931 earthquake. Part II gives the seismic data for the main earthquake and aftershocks. The distribution of seismic intensity is shown by a map of the isoseismals. The lines are arranged in the form of concentric ellipses elongated in a N.E. to S.W. direction with the axis passing through Hawke's Bay. Intensity X on the Rossi-Forel scale was reached in a small area around Napier and Hastings. The isoseismal of intensity II is drawn near Auckland in North Island, but not at all in South Island ; isoseismals III and IV, however, run near Christchurch in South Island. While in North Island the isoseismals are comparatively regular, they become disturbed and irregular near Wellington, and are markedly convoluted in South Island. The earthquake was felt at a maximum distance of about 450 miles from its epicentre.

From the seismographic records the epicentre appears to be situated near the coast-line of Hawke's Bay from 5 to 15 miles north of Napier, the geographical position adopted being Lat. 39° 20′ S.; Long. 177° 0′ E., which is congruent with the geological evidence. The focal depth of the main shock was probably between 10 and 15 miles, which agrees with the estimated values for focal depth of the numerous aftershocks.

The final section of the Report summarises the damage done to the principal types of buildings in Napier and Hastings. Many cases of damage to all types were undoubtedly due to the nature of the foundations, especially where the subsoil was silty and suffered considerable earth-movement. In general, solidly constructed wooden buildings stood the shock well, but were, of course, specially liable to damage from fire. The larger brick buildings failed badly and their general design was at fault. Heavy concrete ceilings, tiled roofs, and massive ornamental fronts had bad wrecking effects under the racking stresses of the earthquake movement, although in some cases poor mortar and consequent disintegration of brick walls was the chief cause of failure. That type of construction with reinforced concrete or steel frames proved most successful in resisting the earthquake.

The conclusion is arrived at that the main loss of life and property was directly due to obvious errors in design and construction of some of the buildings. A Buildings Regulation Committee was therefore set up by the New Zealand Government to make a thorough investigation of building design and construction in view of the liability of the Dominion to destructive earthquake shocks. As a result of the Committee's work a Building Construction Bill was introduced in the New Zealand Parliament in August 1931, and re-introduced in 1932. Unfortunately, however, it did not get beyond its first reading before Parliament adjourned. A leader article in the *Press* (Christchurch) of October 11, 1933, complains of continued delays in this very necessary legislation. It states that the Bill has been shelved session after session on one excuse or another, that nothing has been done to promote research into earthquake problems, and that, most unpardonably, buildings have been erected since February 1931 which do not embody any precautions against earthquakes.

Report of Indian Association for Cultivation of Science, 1932

The Annual Report of the Indian Association for the Cultivation of Science for the year 1932 is published in the *Indian Journal of Physics* for December 1933. The Report deals with the researches carried out under the auspices of the Association, and mentions in particular work on the Raman effect. In this branch of physics Bhagavantam, prior to his appointment as Professor of Physics at Waltair, developed a special technique for the study of the effect in gases under high pressures, and by special elimination of parasitic light he was able to study the intensities and states of polarisation of the Raman scattering of several gases. He showed that while the relative intensities and the state of polarisation of the various lines corresponding to rotational vibrations accompanying the Rayleigh scattering were in satisfactory agreement with existing theories, the ratio of the intensity of any Raman line to that of the Rayleigh, or unchanged line, was markedly different to that predicted by existing theories. To explain these discrepancies, the suggestion that the photon has an intrinsic spin of one unit, $h/2\pi$, has been made.

In the case of the Raman light from hydrogen, the fine structure was studied in the case of the vibrational line, which consists of well separated components Work on the Raman spectrum of methane shows the absence of rotational lines which would theoretically be expected. Experiments were also carried out on the magnetic double refraction of aqueous solutions of sodium chlorate and sodium bromate, where results in agreement with the known structure of the chlorate and bromate ions were obtained.

The structure of diphenyl crystals was examined by Dhar, who found that the benzene rings in the molecule have a plane hexagonal structure, that the two rings of the molecule are in the same plane and that the carbon atoms in the positions 4, 1, 1′ and 4′ are in a line.

Chlorinated Rubber (M. S.)

Chemical research has provided industry with a remarkable derivative of chlorine in the form of chlorinated rubber. This product has proved useful as a constituent of varnishes, paints, and of plastic materials for coating surfaces in order to impart a resistance to corrosion ; for example it can be used for coating steel pipes and it is an efficient substitute for nitrocellulose for certain special purposes. Among advantages over raw rubber not only is there this high resistance but the additional fact that larger proportions may be included in organic solvents without causing too high a viscosity for normal uses. A further favourable property which is to be expected from the chemical point of view is that the rubber has lost its high inflammability through chlorination ; while a second advantage also to be predicted from its chemical composition is the exceptional resistance towards chlorine gas and hypochlorites. The method of manufacture compares with the production of other addition and substitution compounds of chlorine and hydrocarbons, for although the rubber molecule is complex and rubber is a solid, yet the chlorination is carried out with little difficulty in the presence of a solvent like carbon tetrachloride. There is first the formation of an addition product as in the case of any other hydrocarbon of an unsaturated nature, and this is followed by a tendency for the chlorine to displace the hydrogen of the rubber molecule in the form of hydrogen chloride. It is this tendency to evolve hydrogen chloride gas which is so troublesome in practice, for the evolution goes on whenever the product is heated. By now the results of laboratory research are being used not only in improving manufacturing operations but in applying the new substance in directions in which raw rubber is inadmissible owing to its inflammability. The material is a yellow fibrous mass which dissolves in many organic solvents to give a clear colourless solution. A typical German product known as Tornesit has a chlorine content of 67·8 per cent. according to Fol and Bijl, although a value as high as 73 per cent. has been found in other industrial samples, this being due to the presence of carbon tetrachloride absorbed during manufacture. The above workers give the composition as approximating to $C_{10}H_{14}Cl_8$ when samples have been freed from this absorbed impurity, and they have found that on heating the material to 130° C. in an inert gas there is evolved a distillate of hydrogen chloride and carbon tetrachloride in the case of industrial samples, the loss at this point being approximately 8·6 per cent.; but at 250° C. there is considerable decomposition with an evolution of as much as 46·3 per cent. of hydrogen chloride. All products from Germany and those prepared in this country are very stable towards acids and alkalis, while prolonged exposure to boiling water causes little loss in chlorine content, O. Merz reporting that this loss is never greater than 0·27 per cent. for any sample. Dr. W. Krumbhaar, Director of the Institute for Varnish Research in Berlin, maintains that no form of chlorinated rubber is completely stable towards ultra-violet light nor even towards the light of an ordinary incandescent lamp. There has been some conflicting statements concerning the properties of the material which determine its use in varnishes and paints, but in many of such statements the presence or absence of any plasticising agent has not been indicated. Since it has been shown that the inclusion of such plasticising agents as tricresyl phosphate improves the elasticity of surfaces or coatings, there is room for further research on this interesting compound of chlorine. Two examples of the manner in which originality is being shown are (1) the use of scrap vulcanised rubber as raw material, it being claimed that chlorine displaces sulphur during preparation ; and (2) the rendering of rubber surfaces more resistant towards oxidising agents and oils by simple chlorination of the rubber surface.

Miscellanea

The following list contains the names of those recommended by the Council of the Royal Society for election to the fellowship of the Society : Mr. A. S. Besicovitch, Cayley lecturer in mathematics, University of Cambridge ; Prof. W. E. Curtis, professor of physics, Armstrong College, Newcastle-on-Tyne ; Dr. L. L Fermor, director of the Geological Survey of India ; Dr. P. Fildes, bacteriologist, London Hospital ; Dr. R. T. Grant, lecturer in cardiac pathology, University College Hospital Medical School, London ; Mr. M. A. C. Hinton, deputy keeper of zoology, British Museum (Natural History) ; Dr. E. L. Hirst, senior lecturer in organic chemistry, University of Birmingham ; Dr. E L. Kennaway, director of the research laboratory, Cancer Hospital, London ; Mr. A. G. M. Mitchell, consulting engineer, Melbourne ; Prof. W. A. Parks, professor of geology, University of Toronto ; Prof. H. Raistrick, professor of biochemistry, University of London ; Prof. A. O. Rankine, professor of physics, Imperial College of Science, University of London ; Lieut.-Col. R. B. Seymour Sewell, lately director of the Zoological Survey, India ; Prof. S. Sugden, professor of physical chemistry, Birkbeck College, University of London ; Mr. W. Taylor, managing director of Messrs. Taylor, Taylor, and Hobson, Ltd. ; Dr. H. Hamshaw Thomas, lecturer in botany, University of Cambridge ; Rev. A. Young, mathematician, rector of Birdbrook, Essex.

H.M. the King has consented to accept the Bessemer gold medal of the Iron and Steel Institute of which he has been Patron since his accession. He has approved the award of the Founder's medal of the Royal Geographical Society to Mr. Hugh Ruttledge for his journeys in the Himalayas and his leadership of the Mount Everest Expedition, 1933, and of the Patron's medal to Capt. Ejnar Mikkelsen for his explorations and work in the Arctic.

The Murchison grant has been awarded by the Council of the Society to Mr. J. Rymill for his recent exploration in Greenland and the leadership of the expedition after the death of Mr. H. G. Watkins.

Sir Henry Wellcome has been awarded the Croix de Chevalier of the Legion of Honour by the President of the French Republic.

The Neill prize of the Royal Society of Edinburgh for the period 1931–33 has been awarded to Dr. G. W. Tyrrell for his work on the geology and petrology of subarctic and subantarctic lands.

Prof. H. C. Urey of Columbia University has been awarded the Willard Gibbs medal of the American Chemical Society for his discovery of heavy water, and the Chemical Society of Washington has awarded the Hillebrand prize for 1933 to the late Dr. Edward Washburn for his discovery of the first method for separating the isotopes of hydrogen.

Sir Arthur Smith Woodward has been elected president of the Palæontographical Society, Sir Sidney Harmer president of the Ray Society; Mr. J Milton Offord president of the Quekett Club; Mr. J. F. N. Green president of the Geological Society; Mr. Edwin Thompson president of the Society of Chemical Industry; Mr. J. Evans president of the Society of Public Analysts; Dr. Janet Matthews chairman of the Microchemical Club; Sir Henry Lyons president of the Institute of Physics; the Rt. Hon. Lord Rayleigh president of the Physical Society and Prof F. J. M. Stratton president of the Royal Astronomical Society.

We regret to have to record the announcements of the death of the following well-known scientific men during the past quarter: William Barlow, F.R.S., crystallographer; Dr. F. A. Bather, F.R.S., one-time keeper of the Geological Museum; Dr. J. M. Bell, director of the Geological Survey of New Zealand; Prof D Black, F.R.S., of Peking; Mr. C. E. Borchgrevink, Antarctic explorer; Prof. R. Chodat, botanist of Geneva; Sir George Duckworth, one-time secretary of the Royal Commission on Historical Monuments; Dr. F. Ibbotson, metallurgist of Sheffield University; Prof. A. B. Macallum, F.R.S., professor of biochemistry in McGill University; Prof. C. Matignon, professor of chemistry in the Collège de France; Sir Thomas Muir, C.M.G., F.R.S., mathematician; O. von Muller, president of the Deutsches Museum at Munich; Sir Max Muspratt, industrial chemist; Prof. Sven Odén of Uppsala; Prof. S. F. Oldenburg, permanent secretary of the Russian Academy of Sciences; John Power of the Cape Observatory; Dr. Walter Rosenhain, F.R.S., metallurgist; Prof. A. Trowbridge of Princeton University; Prof. S. H. Vines, F.R.S., until 1919 Sherardian professor of botany in the University of Oxford; Dr. E. W. Washburn, chief chemist of the U.S. Bureau of Standards; Mr. W. G. Whiffen, an original fellow of the Institute of Chemistry.

The preliminary programme of the arrangements for the meeting of the British Association at Aberdeen during the week September 5–12 includes the titles of many of the presidential addresses and the subjects for the chief discussions. Prof. H. M. Macdonald will address Section A on "Theories of Light" and the subjects to be discussed include the ionosphere, celestial spectrophotometry, the structure of alloys and photo-electric cells. Prof. T. M Lowry (Section B), has chosen "Physical methods in Chemistry" as the subject of his address; Prof. W T Gordon will speak on "Plant life and the philosophy of geology" (Section C) and Dr. E. S. Russell will deal with "The study of behaviour." The usual excursions will be arranged—the majority to places of geological, historical or scenic interest.

The Academic Assistance Council has circulated an appeal signed by the Vice-Chancellors of nine universities on behalf of the thousand and more scientists and scholars who have been dismissed from their posts in Germany on grounds of religion, political opinion, or race. It is stated that the funds at the disposal of the Council are exhausted and that £25,000 will be required each year for two years to continue the maintenance grants and to contribute to "a final and constructive solution" of the problem. This appeal will be considered with active sympathy by all concerned to see scholarship in distress and academic freedom endangered, but there is an aspect of the matter which is perhaps treated in too little detail. "It is expected that, as the quality and academic distinction of the scholars becomes known during the next two years, permanent places will be found for them" "in countries which will welcome their services" and with this end in view "academic committees in many countries . . . are now engaged in a systematic investigation of openings throughout the world." It is unfortunate that the Council does not enlarge a little on the nature of these "openings" and give an assurance that no hardship shall be imposed on the nationals of the countries providing them. The plight of the German scholars is at once real and spectacular; the Council is faced with the need to provide a remedy which does not produce hardship equally real but less obvious and therefore less likely to attract helpful sympathy.

Two memorials to Richard Trevithick, a pioneer of the rail locomotive and the high-pressure steam engine, were unveiled during April. One, a monument at Pontmorlais, Merthyr Tydfil, is built of some of the stone sleepers used for the track from the Pen-y-darran Iron Works to Abercynon on which his first rail locomotive ran in February 1804. The other is a large bronze tablet let into the wall of the Gower Street face of the Engineering Department at University College, London, near the site of the circular tracks which Trevithick laid down to demonstrate his eight-ton locomotive Catch-me-who-can in 1808. The exact location of these tracks is uncertain. The first was probably situated near the corner of Gower Place and Endsleigh Gardens, but the ground there was soft and the track sank into it. (St. Pancras Parish Map for 1795 shows a stream running through this area with a deep loop down what is now Gordon Street.) Another track was then built farther south on the Bedford estate. Incidentally the site occupied by the College was known as the Homefield in 1795 and, in 1812, as Mortimer's Plot at which time the area now covered by Maple's contained a great number of small dwellings arranged in rows leading off Tottenham Court Road and was not improbably a slum.

In two letters to *Nature* (March 3, May 5), Dr. Aston summarises his latest work with the mass spectrograph. It appears that the only common elements which still resist analysis are palladium, iridium, platinum and gold. Hafnium appears to have five isotopes—176 (weak), 177, 178, 179, 180; thorium one—232; rhodium one—103, calcium—40, 42, 43 (both weak), 44 and possibly 41, titanium five—46, 47, 48, 49, 50, samarium—144, 150 (and possibly others), zirconium—91, 96 at least; dysprosium—161, 162, 163, 164; holmium—165; erbium—166, 167, 168, 170; thulium—169; ytterbium—171, 172, 174, 176; lutecium—175.

The February number of the *Journal of the Royal Horticultural Society* contains a paper by Dr. Tincker on the use of sodium chlorate for killing weeds. It has the advantage that it becomes harmless about seven months after application and it is, at the same time, very effective if used in water solution of the following strengths at the rate of 1 gallon for ten square yards:—large grasses and docks 10 per cent. (*i.e.* 1 lb. per gallon), herbaceous weeds and small grasses 5 per cent., small annual weeds 2.5 per cent.

The *Report of the National Physical Laboratory* for the year 1933, published on May 7 (H.M. Stationery Office, 13*s*. net) is a comprehensive volume of some 260 quarto pages describing in sufficient detail the multifarious labours of a staff exceeding 600 persons.

The routine test work showed a satisfactory increase in most departments after the somewhat heavy fall in 1932, and over 677,000 clinical thermometers were tested in the heat laboratory, the highest figure since 1922 The number of watches submitted for test fell from 290 in 1932 to 207, but a deck watch made by Louis Brandt et Frère, Omega Watch Co., Geneva, created a new record by gaining 97·4 marks. Details of the performance of the best fifty watches is given and it is noteworthy that the first forty-nine were made on the Continent and had Guillaume balances ; the fiftieth, made by V. Kullberg of London, had a brass-steel balance wheel and gained 91 5 marks. Work on the wave-length of the red cadmium line proceeded under the direction of Mr. J. E Sears and the mean value obtained was 6440 2510 Å *in vacuo*, or 6438 4711 Å in dry carbon dioxide-free air at 15° C. This measurement has now been made by four groups of observers · in 1895 by Michelson and Benoît, in 1905–6 by Benoît, Fabry and Perot, in 1927 by Watanabe and Imaizumi and in 1933 by Sears and Barrell, and the extreme difference in their results is only 0·0021 Å. Work on the International Temperature Scale showed that the effect of pressure on the boiling point of water is not known with sufficient accuracy, the present data being discordant to the extent of several thousandths of a degree. Experiments with platinum, platinum-10% rhodium thermocouples show that temperatures within the 660° C.–1,063° C. range can be reproduced in different laboratories to within 0·1° C. Above 1,063° C. the optical pyrometer is used and a new determination of the freezing-point of platinum gave 1,773·3° C., in excellent agreement with the value 1,773 5° C. obtained in 1931 at the Bureau of Standards. Turning to another aspect of thermometric work, it has been observed that the freezing-point of milk (– 0 55° C.) is constant to within two or three hundredths of a degree whether the milk is rich or poor in quality, so that a determination of the freezing-point may be used as a convenient test of adulteration.

The photo-electric photometers proved very satisfactory in use and all commercial measurements of luminous flux in 1933 were made with them ; for precision work they proved equally satisfactory provided that, in heterochromatic photometry, precautions are taken to avoid (or allow for) a Purkyně effect in the photo-cell.

Investigation of the structure of magnet steels indicated that high coercive force is associated with considerable internal strain, whilst low hysteresis loss accompanies the strain-free condition. The use of coal-gas for the propulsion of heavy road vehicles having become an established practice, tests were made on the nickel-chromium-molybdenum steel cylinders used to carry the gas to determine a suitable specification for Home Office regulations. Pressures up to 3,000 lb. wt. per sq. in. are used and no data for the safe " life " of the cylinders are yet available. A research in the metallurgical department dealt with the composition and preparation of dental amalgams. These consist of various proportions of silver, tin, copper, zinc and mercury, and the best amalgam is one which expands very slightly (*e g.* four ten-thousandths of an inch) in the first few hours after filling and thereafter remains invariable in size. The results show that the relative amounts of the constituents of the amalgam are less important than the way in which they are mixed

The preceding paragraphs deal with only a very minute fraction of the work carried out in the Laboratory but they may suffice to show that this work does not lack variety or importance whether judged from the viewpoint of the ordinary citizen or of the theoretical physicist.

We have received a copy of the *Second Supplement* to the *Catalogue of Lewis's Medical and Scientific Lending Library* which covers the additions to the library during the period 1931-3 (Lewis's Lending Library, 136, Gower Street, W.C.1, 2*s*. net or 1*s* net to subscribers). It contains about 2,700 entries arranged under authors' names together with an index of subjects under each of which appears the names of the authors who have written upon it. The list of books is very comprehensive and contains every book for which we looked to check its scope.

REVIEWS

MATHEMATICS

The Distribution of Prime Numbers. By A. E. INGHAM, M.A. [Pp. vii + 114, with 4 figures.] (Cambridge: at the University Press, 1932. 7*s*. 6*d* net.)

THIS book is No. 30 of the Cambridge Tracts in Mathematics and Mathematical Physics and is essentially a companion to Professor Titchmarsh's *The Zeta-function of Riemann* in the same series. Although published first, the latter is logically a sequel to the book under review. The two books, both written by authors with many valuable results in this field to their credit, will prove invaluable to those interested in this important and attractive branch of the analytic theory of numbers.

The first chapter of Mr. Ingham's book contains an account of the "elementary" theory of the distribution of primes, that is, those theorems which can be proved without resource to the theory of functions of a complex variable. In the next chapter, the Riemann zeta-function is introduced and the Prime Number Theorem is proved. The remainder of the book contains the "classical" theory of the zeta-function and those theorems concerning primes which may be deduced from it.

E. M. W.

Inversive Geometry. By FRANK MORLEY, M.A., Sc.D., and F. V. MORLEY, D.Phil. [Pp. xii + 273, with 67 figures.] (London: G. Bell & Sons, Ltd., 1933. 16*s*. net.)

THIS introduction to algebraic geometry lays special emphasis on the operation of inversion. It is an attempt to treat fairly elementary subject-matter from the standpoint of modern geometry, thus providing a suitable preliminary to the study of the work of contemporary geometers. The authors "believe that the tradition that simple geometric and mechanical questions are to be handled only as Euclid or Descartes might have handled them is very hampering; that the ideas of Riemann, Poincaré, Klein and others have pleasant reverberations in the investigation of elementary questions by students of proper maturity and leisure."

The impression produced by the book is that of an interesting experiment. Too little attention has been paid in the past to the task of presenting modern ideas in mathematics in an elementary form, and applying these ideas to the simplest types of problems. The book can be recommended to non-experts anxious to obtain a fairly clear idea of the standpoint and principles of present-day work in geometry.

E. M. W.

Mathematical Tables. Vol. III. Minimum Decompositions into Fifth Powers. [Pp. vi + 368.] (London: British Association for the Advancement of Science, 1933. 10*s*. net.)

This volume is the outcome of a bequest from Lieut.-Col. A. J. C Cunningham to assist in the production of tables connected with the theory of numbers. The calculations were carried out by Prof. Dickson and his assistants at Chicago.

The tables give the "minimum decomposition" of n into fifth powers for $n \leq 300{,}000$, that is, the least value of s such that

$$n = m_1^5 + m_2^5 + \ldots + m_s^5,$$

where $n, m_1, m_2, \ldots, m_s$ are positive integers Up to 150,000 the values of the m's for one such minimum decomposition are indicated. For example, opposite to the number 1606 appears the abbreviation 3 2 1 for a decomposition $3 \cdot 2^5 + 2 \cdot 3^5 + 1 \cdot 4^5$ of 1606 into fewest fifth powers. The minimum $3 + 2 + 1 = 6$ is not printed, but in its place appears the minimum 17 for the largest number 1617 preceding the next tabulated number 1618. Minimum decompositions of the omitted numbers 1607, . . , 1617 are found by adding 1, 2, . ., 11 ones to the tabulated minimum decomposition of 1606

The work was much shortened by a method perfected by the author by which it is possible to determine a minimum decomposition without a knowledge of other decompositions

The number 223 is a sum of 37 fifth powers, but of no smaller number. The last number found requiring 32 or more is 466. After 466 occur no peaks greater than 28. After 191,263 there are no peaks greater than or equal to 16. This indicates the rapid decrease of the number of powers required as n increases For the results obtained for all numbers the reader is referred to the "Notes on Recent Advances in Pure Mathematics" in this number.

E. M. W.

ASTRONOMY

The Universe Around Us. By Sir James Jeans, M.A., D.Sc., Sc.D., LL.D., F.R.S. [Pp. x + 380, with 30 plates and 23 figures] (Cambridge: at the University Press, 1933. 12*s*. 6*d* net.)

This is the third edition of a book which, when first published, was very favourably reviewed in these pages. Founded originally on lectures and wireless talks to audiences with no special scientific knowledge, it aimed at giving a popular and intelligible account of the methods and results of modern astronomical research, with special reference to the problems of cosmogony, evolution, and the general structure of the universe. Its success may be gauged from the fact that in the thirteen months after its first appearance in 1929 no less than 40,000 copies were sold and a new edition became necessary. Since then progress has been rapid both in atomic physics and in our knowledge of the cosmos, new theories having kept pace with and occasionally tended to outstrip the solid achievements of experiment and observation. The author has taken advantage of the issue of the present third edition to bring his book up to date as completely as possible, and it has been extended by 28 pages and 6 new plates. Chadwick's discovery of the neutron and the existence of the positron are connected with cosmic radiation in providing evidence for the annihilation of matter in outer space.

The supposition that the stars generate their energy by the spontaneous annihilation of their substance is fundamental to Jeans' theory of stellar evolution, and his "liquid star" hypothesis is naturally given prominence. It is, however, frankly stated that this is controversial ground, and adequate reference, occasionally caustic, is made to the various rival theories. Sir James is admirably equipped for popular exposition, and he is not afraid on occasion to give a little rein to his imagination. Any reader at all interested in the subject can be assured of deriving both enjoyment and profit.

R. W. W.

METEOROLOGY

The Drama of Weather. By Sir Napier Shaw [Pp. xiv + 269, with frontispiece and 92 figures.] (Cambridge: at the University Press, 1933. 7*s* 6*d*. net.)

Two years ago Sir Napier Shaw completed the last of the four volumes of his magnum opus, the Manual of Meteorology. The Manual is distinguished by insistence on the importance of an accurate knowledge of all available relevant observations as a prelude to theorizing and on the importance of diagrams in which statistics can be summarized and brought home to the reader: it is also distinguished by full treatment of the history of the subject and by the skill of a master of telling phraseology. Now we have before us *The Drama of Weather*, a little book which may be regarded as the manual in brief; most of the illustrations are reproductions from the larger work. These include numerous charming cloud studies as well as beautiful diagrams which will repay careful study. There is, however, a new point of view, for the title is significant. The author does bring out analogies suggested by the drama, or by the allied subject of music. Amongst the chapter headings we find "The Watchers, what they see and what they say," "The Score," "The Chorus, rhythmic aspects of the records." The last chapter "The Weather Map presents the Play" is divided into two sections, "Barometer solo, accompanied by wind and weather," and "The Norwegian Duet for Polar and Tropical Air with Cyclonic Accompaniment." In passing it may be remarked that many readers will regret that the last section has not been amplified, especially those readers who are anxious to learn how to answer the questions "Why did it rain yesterday?" and "Will it rain to-morrow?" Their needs are met, however, by other books, such as Shaw's own *Forecasting Weather* The latest edition of *The Weather Map*, written by J. S. Dines, gives a good idea of how these practical questions are faced at the Meteorological Office.

At the end of the Drama of Weather is the Epilogue "The Weather Map of the Future." Sir Napier Shaw has insisted for a long while on the incomplete view of the meteorological situation gained by maps of pressure reduced to sea-level. Now he writes, "So for the Weather Map of the future we need something that does not require pressure to be reduced, or in fact exaggerated, to sea-level, that uses actual temperatures and thinks of actual winds free from the impediments of the surface, where they came from, what made them and what is guiding them. The weather map of the future should have vertical cross sections as supplements to the ground plan. . . ."

One must not mind if the dream is vague; the wise prophet does not go into detail. It may be mentioned, however, that there are already enough observations available day by day to enable one to make tentative cross-sections of the atmosphere over Europe up to a height of some 5 kilometres. At present the Meteorological Office includes in the upper air supplement of the Daily Weather Report superimposed graphs of temperature as a function of height for several stations. To represent the same information by cross-sections would be a simple matter, though the value of the diagrams would be discounted owing to the need for interpolation. The diagrams would call attention to the absence of upper air observations over the sea. When funds are provided for daily upper air observations by aeroplane or by radio-sondes over the Atlantic Sir Napier Shaw's dream will come true.

The Drama of Weather is by no means a systematic textbook of meteorology, but the reader is given a good idea of all the activities of meteorologists. He is invited to consider the pageantry of the sky, to glory in the beauty and the significance of the clouds ; he is reminded of the allusions to weather in the Bible ; he is shown how instruments are used to record the sequence of weather changes ; he is introduced to maps and diagrams which summarise the climate of a single place or of the whole globe and finally he is instructed in the interpretation of the daily weather map. The book is easy to read, with touches of humour here and there. It should encourage many people to make a closer study of meteorology. Sir Napier Shaw is to be heartily congratulated on being able to carry through such an excellent piece of work in the eightieth year of his life.

F. J. W. W.

PHYSICS

Theoretical Physics. By W. Wilson, F.R.S. **Vol. II : Electromagnetism and Optics, Maxwell-Lorentz.** [Pp. xi + 315, with 78 figures.] (London : Methuen & Co., Ltd., 1933. 18s. net)

This book forms the second volume of Professor Wilson's important treatise on theoretical physics and deals with electricity, magnetism and light. Those readers who appreciate the vector notation which is used throughout the book will find it a very welcome addition to their sources of information ; and, possibly, many who do not ordinarily use that notation may be urged to acquire a taste for it by a little diligent perusal of this theory of electricity and magnetism. It is abundantly clear that the author has devoted much time and thought to the elucidation of those points which students find difficult in the theory of the subject. This is most marked, for example, in the discussion of the relation between mass and energy and in the neat way in which the extension of Kirchoff's laws to circuits containing inductance and capacity is discussed. Special attention is also given to Maxwell's electromagnetic theory.

The treatment of light is masterly, and shows a thorough insight into optical problems. Indeed, this insight is so patent that it is regretted that the author has not expanded this portion of his treatise to give readers more benefit of his special knowledge ; perhaps he will do this in a later edition. The main fault to be found in the production is the frequent occurrence of heavy type references to equations distributed throughout the text. This the reviewer found somewhat trying ; possibly this is purely a personal matter, and not likely to be a general cause for complaint.

L. F. B.

Intermediate Magnetism and Electricity. By J. E. PHILLIPS, M.A., B.Sc. [Pp. xii + 365, with 270 figures.] (London: Christophers, 1933. 6s. 6d.)

THE author gives a very good account of intermediate electricity and magnetism more or less along the normal textbook lines. Presumably, the inclusion of the theory of the ballistic galvanometer, the theory of magnetic shells, the capillary electrometer, and descriptions of the determination of e/m and of "v," is the justification for the statement in the preface that "in many parts the treatment is adequate for pass degree requirements." Some of the figures need serious amendment, *e.g.* figures 1, 30, 179 and 268 (*b*).

L. F. B.

Mass-Spectra and Isotopes. By F. W. ASTON, Sc D., F.R S. [Pp xii + 248, with 8 plates and 43 figures] (London · Edward Arnold & Co , 1933. 15s. net)

IT is very pleasing to record the appearance of a new book on isotopes by Dr. Aston, for he still remains unchallenged in his supremacy in this branch of physics and of chemistry, and the experimenters who have really advanced our knowledge of mass-spectra can still be counted on the fingers of one hand. So much new material has been collected during the nine years which have elapsed since the publication of the second edition of *Isotopes* that a new book is very welcome. Actually, *Mass-Spectra and Isotopes* cannot properly be regarded as a third edition of *Isotopes*, so great has been the modification in the treatment of the general theory and so great the elaboration of the results obtained by the mass-spectrograph in the new work.

It is divided into four parts. Part I contains the historical survey of the subject, the details of the radioactive isotopes, and a description of the first mass-spectrograph. Part II deals with the technique of the production and analysis of mass-spectra. Special attention is here paid to the precision measurements made with Aston's second mass-spectrograph and the apparatus of Costa and of Bainbridge, and to the recent important work on the photometry of mass-spectra. In Part III is given an account of the isotopic constitution of each element, *i.e.* a kind of nuclear chemistry is here completely set forth for the first time in a compact form. For convenience the elements are examined in the order in which they occur in the groups in the periodic table, and there is given a most valuable collection of data which must be of great interest to all who wish to follow the rapid development of the artificial transmutation of the elements.

Part IV is concerned with the theoretical aspects of the electrical constitution of matter, more particularly with the breakdown of the additive laws of mass, and with the interesting relations exhibited by the elements and their isotopes. In this part, Dr. C. P. Snow contributes two chapters on the isotope effect in molecular and in atomic spectra. It concludes with a chapter on the separation of isotopes, in which the only omission appears to be a reference to the possibility of separating the isotopes of lithium which is suggested by the work on the mobility of lithium ions in rare gases. Tables of packing fractions and of isotopes and a periodic table of the elements are given in three appendices.

The book is very pleasant to read, and the generous way in which the author acknowledges the contributions made by other workers, at a time when too many are occupied with claims of priority and with kindred subjects, reminds the reviewer of the equally generous way in which Dr. Aston has in the past given advice and loaned rare materials to others in the Cavendish Laboratory. The printing and illustrations are all that could be desired—the slight flaw on page x almost escapes notice.

L. F. B.

A Text Book of Physics. By E. GRIMSEHL. Edited by R. TOMASCHEK, D.Phil., Professor of Physics in the University of Marburg. **Vol. III: Electricity and Magnetism.** Authorized translation from the seventh German edition by L. A. WOODWARD, B.A.(Oxon.), Ph.D. (Leipzig). [Pp. xiv + 685, with a frontispiece and many figures.] (London: Blackie & Son, Ltd., 1933. 25s. net.) **Vol. IV: Optics.** Authorized translation by WINIFRED M. DEANS, M.A. (Cantab.), B.Sc. (Aberd.). [Pp. xii + 301, with 16 plates, 1 coloured plate and numerous figures.] (London: Blackie & Son, Ltd., 1933. 15s. net.)

THE third volume of this textbook is of honours standard on the descriptive side of the subject, but falls considerably short of that level on the mathematical side. The matter and treatment are quite modern, many lecture experiments new, at least to the reviewer, are described and the illustrations are beyond reproach.

The arrangement of the book is fairly orthodox. Part I deals with electrostatics and magnetism, Part II with steady currents in solids, liquids and gases, Part III with varying electromagnetic fields, including an account of dynamos and motors and of oscillatory circuits.

The descriptive treatment of electrostatics is admirable, and it is unfortunate that the translator has not adapted the terminology to accord with standard English practice. The term electric pressure is not an acceptable equivalent of potential difference and statements such as "The pressure in the electrostatic field is independent of the path" (p. 30) should not have appeared. Later in the book electromotive force is regarded as being synonymous with potential difference and we are told (p. 236) that the E.M.F. of a chromic acid cell is 1.8 volts on open circuit and 0.8 volt when the terminals are connected by a certain length of iron wire! The terms anode and cathode are applied to the plates of an accumulator, electrical conductivity is called specific electrical conductivity and the words resistivity and capacitance do not appear. An example of lack of precision in statement occurs on p. 420 in the description of the magnetic potentiometer where we read that "The induced E.M.F. between the leads of the instrument is given by the total change of flux through the coil." Incidentally this device is attributed to Rogowski and Steinhaus. Dye (*Dict. App. Physics*, II, p. 467) attributes it to Chattock in 1887 and gives references to the German workers dated 1912.

The weaknesses referred to in the preceding paragraph make the reviewer hesitate to recommend the book for student's use and this in spite of the fact that it contains a more complete account of the whole subject than any other known to him. It should be stated that the volt-second system of units is employed so that, *e.g.* the capacity of a spherical condenser is given as $4\pi k_0 r$ farads. The term electrostatic unit does not appear except in one paragraph wherein the difference between the two systems is explained.

Vol IV is of a standard appropriate to a modern pass degree and, like Vol. III, is distinguished for the excellence of much of the descriptive matter.

About half the book is devoted to various aspects of geometrical optics; there is a good account of the aberrations of lenses and of the effects of "stops" in optical instruments. Nine of the plates are used for photographs showing the passage of rays through lens systems and various effects due to aberration. Other sections deal with dispersion, interference, the measurement of velocity, polarization, atmospheric phenomena and physiological optics. There are a few more or less obvious errors (*e.g.*, in fig. 26, p. 51, and in the statement that the parallel rays incident on a Fabry-Pérot étalon, fig. 27, p. 194, must be coherent) and the sign convention employed is not, in the opinion of the reviewer, well chosen. These, however, are small matters and the book can be recommended for the use of both teachers and students, the more especially as it treats of many matters often ignored in English textbooks of equivalent standard.

Both volumes conclude with a few pages of examples (with answers) and with adequate indexes. The publishers' share in the production has been carried out with the technical perfection which past experience leads us to expect from them.

D. O. W.

Direct and Alternating Currents. By E. A. Loew. [Pp. xv + 656, with 486 figures] (New York and London: McGraw-Hill Book Company, Inc, 1933. 27*s*. net.)

It is claimed that this book covers a short survey course in the theory of the electric circuit and the operating principles of electrical machines. In the preface the author draws attention to his difficulty in deciding, as a matter of policy, how far to go with pure theory and to what extent the theory should be linked with applications in commercial machinery. He concludes that the laws and principles necessary to the understanding of the performance of most electric machines are few in number and that a good working knowledge of these principles can be gained without much difficulty. This statement indicates the type of book that the author has given us, a book wherein a number of important mathematical relations are quoted and used to explain the characteristics of various electrical appliances without any indication being given of the origin of these expressions. This is particularly noticeable in the early chapters. For example the expressions giving the magnetic force along the axis of a solenoid, the mechanical force on a conductor carrying a current in a magnetic field, the lifting force of an electromagnet, the energy loss due to eddy-currents all arrive on the scene without any explanation, a procedure which tends to give the reader a very superficial knowledge of the real essence of the problems involved. It may be argued that the limitation of space makes impossible the inclusion of a complete and reasoned argument built up step by step from basic ideas, but one cannot help feeling that a thorough treatment of fundamental principles should be the primary aim of any book of this kind. The author himself states that he favours the special emphasis of fundamental principles, but it is precisely in this respect that he has laid himself open to the strongest criticism. Otherwise the book has much to recommend it, the treatment in the part dealing more particularly with various types of machines both D.C. and A.C. being excellent.

The work is divided into two sections under the headings (i) direct currents and (ii) alternating currents. In the first section the reader is introduced to D.C. circuit theory, magnetism, electromagnetism, electromagnetic induction, condensers, dynamo electric machinery, electrolytic conduction, storage batteries and D.C. distribution. In the second section A.C. circuit theory, transformers, induction motors, alternators, synchronous motors and converters, A C. commutator motors, A.C. transmission and distribution, switches and fuses, thermionic valves are all considered in turn. There are a large number of useful numerical examples scattered throughout the book and a specially attractive feature is the chapter devoted to vector representation of A C. quantities. It is to be regretted that the symbols standardised by the International Electrotechnical Commission have not been used throughout. Thus the symbol " N " is employed for " turns " instead of " T "

The book should find a useful place among the many other contributions to this subject, but it can only be recommended safely to those who already have a knowledge of elementary principles

H M. Barlow.

Network Synthesis. By C. M Son Gewertz, E.E., Sc.M., Sc.D. [Pp. vi + 257, with 57 figures and unnumbered diagrams in Appendix.] (London: Baillière, Tindall & Cox, 1933. 23s. net.)

On the subject of network analysis there are many scientific papers and books wherein specific examples are taken and the electrical characteristics determined. For practical purposes this is like putting the cart before the horse since the communications engineer almost invariably has to design a network to fulfil certain given requirements. In the book under review a general form of network synthesis is developed. A considerable part of the work is original, particularly that concerned with the four terminal network.

The method of presentation and the discussion of the results obtained is a little difficult to follow, especially if one is not very familiar with the mathematical devices employed, but the author has done his best to elucidate the argument by giving clear statements, and in some cases proofs, of essential theorems. The treatment is almost entirely mathematical. With the help of complex matrices and determinants an astonishing degree of success has been attained in the synthesis of networks presenting no small difficulty.

In many cases the problem of finding equivalent networks having a different number of meshes has not yet been solved, but the author has succeeded in giving solutions for certain specific instances. An appendix containing twenty-nine numerical examples each illustrating a particular application of the method developed in the main part of the treatise gives valuable instruction from the practical point of view.

The communications engineer setting out to design an electric filter-circuit, an amplitude-corrective network or a phase-corrective network will find much useful information, but there are links in the process of arriving at a solution that, in all probability, he will find very difficult to interpret practically.

Information in a particular form is required regarding the admittance and impedance quantities and this cannot always be readily obtained. To make full use of the book a specialised mathematical knowledge is essential. An excellent bibliography covering the most important works on this and allied subjects is included at the end.

H. M. Barlow.

CHEMISTRY

A Textbook of Elementary Qualitative Analysis. By CARL J. ENGELDER, Ph.D. [Pp. viii + 243.] (New York: John Wiley & Sons, Inc; London: Chapman & Hall, Ltd., 1933. 13s. 6d. net.)

LIKE many of the smaller works on Qualitative Analysis, this book is an amplification of working notes used in the author's laboratory, a second edition now following a reprint in 1930. Part I consists of a simple account of the theoretical aspects of the subject, followed by Part II—Analysis of Cations, Part III—Analysis of Anions, Part IV—Systematic Analysis. The methods employed are straightforward and orthodox, and cover the usual basic and acidic radicals, rarer metals and unusual acids being omitted. The book is an elementary introduction to the subject and as such may be recommended, but the price is high judged by English standards. The Periodic Classification (p. 80) appears to have been overlooked in bringing the book up to date.

J. N. S.

Introduction to Physical Chemistry. By ALEXANDER FINDLAY. [Pp. vii + 492, with 124 text figures.] (London: Longmans, Green & Co., 1933. 7s. 6d. net.)

THE design of this book is fairly conventional, chapters on gases, liquids and solids being followed by a discussion of the colligative properties of electrolytes, after which the First Law of Thermodynamics is introduced. The Second Law of Thermodynamics is not discussed until some chapters later, so that in the earlier chapters the formulation of the mathematical relationships in such subjects as the colligative properties and equilibrium conditions is either assumed or deferred. It is possible that such thermodynamical reasoning as the use of Carnot's cycle is confusing to the weak student, but the reviewer felt that for most students there would have been some advantage in discussing the experimental work and its theoretical basis at the same time.

The chapters on Homogeneous and Heterogeneous equilibria, electromotive force, and the Phase Rule are very clear, and there are adequate introductions to Photochemistry, Catalysis, Adsorption and Colloids.

The personal and historical aspects of the subject are always prominent, a great advantage to the student. There is an unusually good index to the book, and an excellent collection of problems on the various chapters is added as an appendix. It is printed on very good paper and with excellent type, but the reproduction of some of the diagrams might have been improved. The price is very reasonable, and the book is strongly to be recommended.

O. H. W.-J.

An Introduction to Thermodynamics for Chemists. By D. JOHNSTON MARTIN, B.Sc., A.I.C., Ph.D. [Pp. vii + 343, with 41 figures.] (London: Edward Arnold & Co., 1933. 16s. net.)

UNDERGRADUATES will like Mr. Johnston Martin's *Introduction to Thermodynamics*. The Author modestly claims to have written an elementary textbook, but the fact is that those who wish to qualify for the Pass or Honours Degree at an English university will find the range and treatment of the subject-matter entirely adequate for their needs.

Thermodynamics is pre-eminently a useful subject of immense technical importance, though one would hardly gather that impression from the majority of textbooks. It is also quite certain that the surest way to stimulate a chemist's interest is to put the utilitarian aspect clearly before him, and for this reason the Author is to be congratulated upon his exceptionally careful treatment of the Free Energy function But there is a criticism to be made here. No table of Free Energy values is given. Such a Table ought to be a chemist's best friend, for when skilfully applied it is a veritable mine of accurate information and at worst should prevent him making an ass of himself. Dr. Johnston Martin has made such good use of Lewis and Randall's classic that he might have availed himself of the opportunity to revise and enlarge their Table of Free Energies which was compiled more than ten years ago.

The early chapters of the book owe much to Lewis and Randall, and the choice of symbols and sign conventions (even the signs of single electrode potentials) are those employed by the American writers. In contrast, however, theorems are developed by cyclical methods. The significance of the Entropy function is made exceptionally clear by a novel interpretation of "irreversible" processes. Forty pages are devoted to "The Activity Method of Treatment of Solutions," which provide an excellent introduction to the rather difficult sections in Lewis and Randall. Periodic excursions are made into the realms of statistical mechanics; thus, an outline sketch is given of Boltzmann's investigation of the Entropy, and of Butler's kinetic derivation of Nernst's electrode equation. Chapter X deals with the anomalous behaviour of strong electrolytes from the point of view of Debye, Hückel, and more recent workers. Finally, the chapter on the Third Law contains a fairly clear and up-to-date account of the determination and use of atomic entropies.

The book is admirably printed, and if the price seems rather high, at any rate the purchaser will get sound value for his money.

C. H. J.

Inorganic Colloid Chemistry. Vol. I: The Colloidal Elements. By HARRY BOYER WEISER, Professor of Chemistry at the Rice Institute. [Pp. xi + 389, with 54 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1933. 28*s*. net.)

COLLOIDAL chemistry has grown so extensively that books written by one author and embracing the whole subject are not likely to appear again in a great number. A thorough discussion of the bulk of knowledge, which some twenty years ago might have been compressed into one chapter, necessitates now a complete volume. It is obvious therefore that monographs are becoming more popular. Several important ones, especially on questions of capillary chemistry and the technical side of colloid chemistry, have been published already. Weiser's book on *Inorganic Colloid Chemistry* is a new and valuable work of this kind.

So far the first volume on colloidal elements has appeared. The subdivision used is a purely chemical one, based upon the contrast between metallic and non-metallic elements, the periodic table being the leading principle in both parts. All kinds of colloidal systems, sols, gels, xerogels, fogs, are in this way brought into close relationship for a given chemical substance. Theoretical discussions are introduced just where the subject-matter treated affords them. This way of treatment reminds one of Wilhelm

Ostwald's "Grundlinien der anorganischen Chemie" or Zsigmondy's textbook of colloid chemistry. Weiser's book might therefore very well be recommended as a textbook for advanced students. This character is also emphasised by the numerous references to practical applications of colloid chemistry and the consideration of material published in the patent-literature. For this reason many facts may be found in this book which are not so easily found elsewhere. The paragraphs on activated carbon, on colloidal fuel, on colloid lead and iodine seemed to me especially interesting.

There are only some points of minor importance in which the reviewer does not quite agree with the opinions given in Weiser's book.

H. FREUNDLICH.

Annual Review of Biochemistry. Vol. II. Edited by JAMES MURRAY LUCK. [Pp vi + 564.] (Stanford, California: Stanford University Press; London: H. K. Lewis & Co., Ltd., 1933. 25s. net)

THIS bulky volume of information, most of it highly condensed, cannot fail to give, even on casual inspection, a vivid impression of what a single year's research in biochemistry throughout the world amounts to. The editors have attempted to make the book as far as possible authoritative and international. For example, amongst the authors of the twenty-five articles, each of about twenty pages, one notes (making a more or less random selection), P. Karrer and A. Helfenstein, R. Robinson, P. A. Levene and A. L. Ramond, M. Heidelberger, D. L. Thomson and J. B. Collip, R. Hober, R. Wurmser, and N. N. Iwanoff and E. S. Swetkoff, so that both the old world and the new worlds have been freely drawn upon. Whilst most of the articles deal with subjects of essential biochemical interest some of them, such as Robinson's article on alkaloids and Karrer's article on Plant Pigments will appeal equally strongly to the organic chemist whilst the physical chemist will find his field touched upon in the first article by Hober on Permeability.

It is clearly impossible in a brief review to give detailed criticism of each section: even to mention all the titles and authors by name would take up a very considerable amount of space: it must suffice to say that the general standard is in accordance with the expectations raised by the reputations of the authors. Overlapping seems to have been reduced to a minimum, a fact which reflects credit on the general editor, J. Murray Luck, and the advisory committee, C. L. Alsberg, D. R. Hoagland and C. L. A. Schmidt. All biochemists ought to be grateful to the Chemical Foundation for rendering possible the production of this valuable volume at a very reasonable price. Every biochemist should have a copy available both for reference and recreation. For most of the articles are eminently readable and though the food is rich and at times very solid it is not often really indigestible. Besides, all tastes would seem to have been catered for.

W. O. K.

Solvents. By T. H. DURRANS, D.Sc., F.I.C. Third edition revised. [Pp. xviii + 205.] (London: Chapman & Hall, Ltd., 1933. 10s. 6d. net.)

A BOOK that has run into three editions in the same number of years has so obviously met with approval that the reviewer can only add his quota.

The title, however, should be modified since the book itself deals almost exclusively with the properties and behaviour of solvents employed in the cellulose lacquer trade. Cellulose lacquers have unquestionably established themselves in the motor car and furniture industries and are steadily increasing their hold and scope in spite of their inflammable characteristics. These latter have been cited as a contributory cause of the rapid spread of fires in French liners in recent times.

That the attractive and glossy finish associated with cellulose lacquers is not haphazard, but the result of careful study and research resulting in the scientific blending of various solvents and plasticisers is amply demonstrated by this book.

The present edition has been increased by 25 pages and contains details of a further 36 solvents, including, curiously enough, turpentine, rosin spirit and castor oil for the first time. The list of trade names has been augmented and a dosage table of plasticisers for both cellulose nitrate and acetate has been added.

M. B. DONALD.

Industrial Chemistry. Edited by ALLEN ROGERS, in collaboration with others. Fifth edition. **Vol. I : Inorganic.** [Pp. xiv + 641, with 266 figures.] **Vol. II : Organic.** [Pp. xii + 876, with 175 figures.] (London : Macmillan & Co., Ltd., 1931. 30*s.* per volume.)

A BOOK which has remained in popular favour for twenty years, necessitating the printing of five editions, leaves the reviewer an easy task unless he finds it necessary to pass any adverse criticism. In a book of this kind, it is most important that the contents be kept up-to-date, not only in respect of the subject-matter of the individual chapters but also in the subjects chosen for discussion. Many subjects treated in a book of industrial chemistry twenty years ago would not appear, or would not claim the same amount of attention, in a book published to-day. Processes change or become less important, and chapters dealing with these processes should be altered, or should even be replaced by others of more interest to the reader.

The present book fails in this respect. The second and third editions were published in 1915 and 1920, respectively. Since that time, considerable advance has been made in industrial chemistry. In the present edition, therefore, it is surprising to find more space allotted to the subject of lead poisoning, for instance, than to that of "synthetic fibres" (rayon, etc.). Fourteen pages—seven of which are almost full-page illustrations—serve to treat of the subject of nitrogen fixation. No reference is made to the manufacture of synthetic phenol and the subject of synthetic resins is barely mentioned. Compare this with the thirty pages allocated to the destructive distillation of wood, thirteen pages to laundering and eighty pages to the subject of leather. It would seem that the great post-War developments in some fields have been neglected, and in this respect the Editor has failed to maintain the high standard set in the earlier editions. We are led to expect much by the Editor's preface, in which he says : "The fifth edition has been completely modernised . . . by the additions of descriptions of newly developed processes whose importance merits inclusion in this book."

The present edition is in two volumes, which contain altogether some three hundred more pages than in the fourth edition. Many changes have been effected in arrangement, and a number of chapters appear under new authorship. Two new chapters are included, one on solvents and the other on industrial instruments and their uses. A chapter on brewing, malting, etc., omitted in the last edition, reappears. These chapters have on the whole been well done, although it is questionable whether, in a book of this kind, a 51-page treatment of industrial instruments is justifiable, when only 42 pages are devoted to "general processes"—a chapter describing chemical engineering plant and equipment.

In future editions, the reviewer would like to see much of the statistical matter eliminated, since the book is intended to give the student "a background knowledge of the subject." This omission will be necessary if the book is to be kept within reasonable limits of size and cost. For similar reasons, more attention might be paid to the bibliographies at the end of most chapters by making a more critical and recent selection of references.

The above comments have been inspired by an editorial request in the preface of the book for constructive criticism. It is in this spirit that they are offered To attempt to cover in two volumes the whole field of industrial chemistry is impossible. While one may disagree with the choice of subjects, the industries treated have been well described. The treatment is mainly descriptive, but does not omit the necessary theoretical considerations for a proper understanding of the subject The book can be recommended as a reference book for technical schools and colleges, and for those readers who wish to acquire a working knowledge of the many processes described.

J. P. M.

Technique of Modern Welding. By PROFESSOR P. BARDTKE. Authorised translation from the second German edition by HAROLD KENNEY, B.A. [Pp. xii + 299, with 315 figures.] (London and Glasgow: Blackie & Son, Ltd., 1933. 15*s*. net.)

THE author of this book is Works Manager of the State Railway Repair Workshops, Wittenberge, Germany, and welding experience in Germany is almost exclusively dealt with.

This excellent survey of welding covers a wide field: not only does the author outline the various methods and applications of welding in modern practice, but, with each method, the various metals most commonly worked are dealt with, giving the variations in treatment necessitated by the differences in properties of the metals.

Gas and electric arc welding are described under the heading of "Fusion Welding." The author deals comprehensively with the plant preparation and execution of the processes, and at the same time gives his views as to the nature of the chemical and physical actions which take place.

Dealing with "pressure welding," forge, electric resistance and spot welding methods are described, and a brief account is given of the aluminium thermit process.

The various methods which have been put in practice for the inspection and testing of welding are reviewed. The economics of welding are discussed with reference to material, energy and labour costs, comparison being made with the cost of riveted construction. On the subject of stresses set up by temperature changes, further elucidation would be welcome, as a great deal of uncertainty still exists as to the nature of such stresses.

For those who wish to acquire a knowledge of the scope of modern welding, this work will be found to be a most valuable treatise. Others—particularly Works Engineers—will find much of value in the accounts of the actions which take place, and also in bringing to their notice applications and processes with which they are not conversant. In the matter of design, in view of the rapid strides being made in the application of welding to bridge work, shipbuilding and steel structures generally, further examples of the technique of welding detail are desirable.

DAVID ANDERSON.

GEOLOGY

The Poetry of Geology. By KENNETH KNIGHT HALLOWES, M.A. (Cantab), F.G.S., Member, Royal Society of Literature, Professional Geologist (1905–26) H.M. Geological Survey of India. [Pp. xii + 61.] (London: T. Murby & Co., 1933. 6s. net.)

MR. HALLOWES pleads with attractive enthusiasm a larger place for science in modern poetry. "The world has so far," he says, "seen little Poetry of Science," though Science can supply Poetry with a leading and living theme. Its "dry facts" could be transfigured so as to appeal to and charm the passing man in the street. This argument the volume finally would reinforce by some thirty illustrative sonnets and verses of the author's own. They invoke the science with which the author is specially familiar, geology, as their central motive. Their earnest and often picturesque verse sets forth certain of the vast vicissitudes of Earth's secular story. But that their quality entitles them to rank as poetry in the full sense, we doubt. They have pictorial quality rather than poetical. To define poetry is indeed difficult; yet all its attempted definitions regard as essential its drive to emotion—it must be able to move us. Geological knowledge, charged with remote, and with prehuman and superhuman, immensities as it is, seems humanly too impersonal to imply emotion for the general reader. Such poetry would somewhat resemble religious poetry, which for the general reader conspicuously fails as poetry. Keats, passionate admirer of Milton's genius as he was, yet felt the Miltonic epics would lose in appeal at a future day because their main theme was not essentially poetical.

As to Mr. Hallowes' general thesis, the effort to make the teachings of Science a central theme of poetry has been less rare than he seems to suppose; witness Lucretius onward to Mr. Noyes. That, however, is a different story from the enlistment of scientific fact and fancy into Poetry to strengthen and enrich the background and other setting of a theme drawn from the perennial poetical springs of human fate and feeling. In reference to Tennyson Mr. Hallowes seems scarcely to distinguish that with "In Memoriam" this latter is the use of science which its success sanctions.

C. S. SHERRINGTON

Treatise on Sedimentation. By W. H. TWENHOFEL and collaborators. Prepared under the auspices of the Committee on Sedimentation, Division of Geology and Geography, National Research Council of the National Academy of Sciences (U.S.A.). Second Edition. [Pp. xxix + 926, with 121 figures.] (London: Baillière, Tindall & Cox, 1932. 46s. net.)

THE first edition of this work was reviewed in SCIENCE PROGRESS, January, 1928, p. 536. The new edition has been greatly enlarged and contains 926 pages as against 661 in the earlier edition, due to the addition of new matter which has been brought forward since the date of the first edition. The general plan of the work and the relative sizes of the various contributions, however, remain much the same as before. The eight chapters have been enlarged as follows: Sources and Production of Sediments, from 23 to 31 pp., Transportation, Deposition, Diagenesis, and Lithification of Sediments, from 58 to 81 pp., Important Conditions modifying Sedimentary Processes, from 22 to 33 pp., Sediments and Organisms, from 33 to 40 pp., Products of Sedimentation, from 279 to 416 pp., Structures, Textures, and Colours of Sediments, from 119 to 180 pp., Environments or Realms of Sedimentation, from 67 to 89 pp., Field and Laboratory Study of Sediments, from 15 to 19 pp. The number of illustrations, still somewhat inadequate, has been increased from 61 to 121. It is pleasant to be able to record that the citations from European literature do not disclose the glaring omissions of the earlier edition, and that misspellings and misprints are noticeably fewer. Analysis B in Table I, however, is erroneously stated on p. 2 to be that of the average sedimentary rock, and there are a few misspellings of foreign place-names and surnames.

This book is a vastly improved production, and now forms by far the most complete existing study of sediments and sedimentation. With its lucid discussions, wealth of references, and adequate indexes, it will prove an indispensable aid to workers in this branch of petrological science.

G. W. T.

On the Mineralogy of Sedimentary Rocks. A Series of Essays and a Bibliography. By P. G. H. BOSWELL, O.B.E., D.Sc., F.R.S. [Pp. ix + 393.] (London: Thomas Murby & Co.; New York: D. van Nostrand Co., Inc., 1933. 21*s*. net.)

PROFESSOR BOSWELL'S valuable book is made up of the summaries of the literature of sedimentary petrography which he has compiled from time to time, together with a covering series of essays, and not least (but unmentioned in the title) a number of useful indexes. Over a thousand items, arranged alphabetically under authors' names, are given in the bibliography. With each there is a succinct statement of the minerals described, their localities and stratigraphical horizons, with a notice of figured specimens and chemical analyses where these are available. It is to be noted that these references deal solely with the mineralogy of sediments; there is a far wider field still untouched in the petrology of sedimentary rocks and in sedimentation, which we hope Professor Boswell, or a colleague inspired by him, will now take up and thus complete this great task. The prefatory essays are slight as they are only intended to be introductory, as "investing the abstracts with some significance by showing what has been done in the field of the petrology of sediments and what is the trend of thought at the present time." They deal with such subjects as: history of investigation, individuality of sediments, stability of detrital minerals, minerals as clues to the sources of sediments, correlation by means of minerals, shore sands and dune sands, deep-sea sediments, authigenous minerals, mineral composition of clays, and detrital minerals in relation to the origin of metamorphic rocks. The indexes comprise a general index, stratigraphical horizons, localities, minerals, figured minerals, and technique, and are not the least valuable feature of a book for which all petrographers dealing with sediments will be profoundly grateful.

G. W. T.

Minerals and the Microscope. An Introduction to the Study of Petrology. By H. G. SMITH, A.R.C.Sc., B.Sc., F.G.S. Third Edition. [Pp. xiii + 124, with frontispiece, 12 plates, and 33 figures in the text.] (London: Thomas Murby & Co.; New York: D. Van Nostrand Co., Inc., 1933. 5*s*. net.)

THE continued popularity of this book, as shown by the call for a third edition, is well deserved. It is designed as an introduction to practical petrology, in which the first stage is a working knowledge of minerals as seen under the microscope. The author deals with the optical properties of minerals in 62 pages, and while following familiar lines his expositions are wonderfully clear and concise, and are well adapted to the student beginning the subject. There follow detailed descriptions of the common rock-forming minerals, and we are again compelled to pay tribute to the beauty and excellence of the photomicrographs that accompany this section. The third part of the book, on the Study of Rocks, has been entirely re-written. In it an attempt is made to bridge the gap between practical instruction in mineralogy and the subsequent examination of thin sections of rocks under the microscope, by giving a succinct account of petrological theory and classification. The treatment of sedimentary and metamorphic rocks is somewhat slighter than that accorded to the igneous rocks, but within the restricted limits of only 36 pages Dr. Smith has accomplished a good performance in this section. We think that this book remains the best introduction to the mineralogical side of petrology for the elementary student.

G. W. T.

Elements of Optical Mineralogy. An Introduction to Microscopic Petrography. **Part II: Descriptions of Minerals, with Special Reference to Their Optic and Microscopic Characters.** Third Edition. By A. N. WINCHELL. [Pp. xviii + 459, with 362 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1933. 37*s* 6*d*. net.)

THE second edition of this work was reviewed in SCIENCE PROGRESS, April 1929, p. 715. It was published under the name of N. H. Winchell, the original author, coupled with that of A. N. Winchell who had entirely re-cast and re-written the book. In the present edition Prof. A. N. Winchell's name appears alone on the title-page, which is entirely fitting, as he is solely responsible for what is practically a new work. Since the date of the second edition the most remarkable advance on the chemical side of mineralogy has been made by the X-ray study of crystals, which has led to re-examination of the relations between optical properties and chemical composition of minerals. The new data thus obtained make a new classification of minerals imminent, especially in regard to silicates; and Prof. Winchell has tentatively adopted a new classification on this basis for the important silicate minerals, while leaving non-silicate minerals classified under the Dana system as before.

In its third edition this book maintains its position as the most comprehensive compilation of optical data for minerals in general. As before it is generously illustrated by diagrams and figures. Some of the diagrams, however, are so overloaded with curves and numerical data as to be difficult to read. The price of the book has unfortunately gone up by ten shillings.

G. W. T.

A Descriptive Petrography of the Igneous Rocks. Vol. II: The Quartz-bearing Rocks. By A. Johannsen. [Pp. xxxi + 428, with frontispiece and 119 illustrations.] (Chicago: University of Chicago Press; London: Cambridge University Press, 1932. 33*s*. net.)

This is the second volume of Professor Johannsen's treatise, to be completed in four volumes. The first volume, which dealt with introductory matter, textures, and classification, was published in 1931. In reviewing this book it will be advisable to disregard the intrinsic merits or demerits of Johannsen's system of classification, and concentrate on the matter and its mode of presentation. It may be said at once that both are excellent. The present volume deals with the Quartz-bearing Igneous Rocks only. These are fully described by families in Johannsen's classification, an exhaustive history of the rock name being first given. These are most valuable sections which help the petrographer to realise what interesting and important material lies buried and forgotten in the older literature. Then come descriptions of the constituent minerals with analyses and modes if available, followed by consideration of the chemical composition and genesis, illustrated by tabulating all analyses that have been accompanied in the literature by statements of the actual quantitative mineral composition (mode) of the rock in question. Plutonic types are dealt with first, then the extrusive rocks, and finally the hypabyssal phases. The most important rock dealt with in this volume is granite, and its consideration occupies 135 pages. As an example of the thoroughness with which the work has been done it may be stated that under "Granite" the following topics are treated. definition and history, megascopic characteristics, microscopic characters including detailed consideration of a long list of minerals with analyses, modes, chemical composition, genesis, modes of occurrence, age, alteration and weathering, and varieties.

The book thus contains an immense quantity of data the accumulation of which must have entailed an incredible amount of labour. Petrographers will be extremely grateful to Prof. Johannsen for assembling this valuable material which they can and will use notwithstanding their opinions as to the merits of his classification. Inevitably there are some misprints, mistakes, misstatements, and omissions, but remarkably few considering the scope of the work. Prof. Johannsen has missed some important foreign literature, but perhaps we are expecting too much even from his omniscience. The book is beautifully produced, printed on fine paper, and well illustrated. As was the first volume this book also is adorned with portraits of petrographers who have been prominent in investigation of the rocks described. The writing is exact, lucid, scholarly, and the book is a pleasure to read and handle.

G. W. T.

Metamorphism. A Study of the Transformations of Rock-Masses. By A. Harker, M.A., F.R.S. [Pp. ix + 360, with 185 figures.] (London: Methuen & Co., Ltd., 1932. 17*s*. 6*d*. net.)

This remarkable book by the doyen of British petrologists is a model of what a scientific work should be in form, style, and balance. It is a study of metamorphism in rock-masses conceived, not as a status, but as a process, as the response in rocks to changing conditions of temperature and stress.

For the most part Dr. Harker has dealt with data which he himself has contributed to the science, wrought into generalisations which are peculiarly his own. Consequently his work owes little to recent foreign studies in metamorphism. He makes no use, for example, of the Niggli-Grubenmann treatise although he makes full reference to Grubenmann's original work. The recent Alpine and Finnish schools of thought receive but scant mention, and Eskola and Stillwell do not even figure in the index. Much of the material has been derived from British fields of study, and within these limits this book represents absolutely the last word that could be said by a great master in his own subject.

In the first part of the book the thermal factor in metamorphism is treated as the ruling condition. This is regarded as the natural line of approach to the more complex problem, dealt with in the second part, in which the dynamic factor enters in conjunction with the thermal. The first part contains nine chapters dealing successively with general considerations, structures, thermal metamorphism of non-calcareous sediments, calcareous sediments, igneous rocks, and with pneumatolysis and metasomatism in thermal metamorphism. The second part begins with a chapter on stress as a factor in metamorphism, goes on to pure dynamic metamorphism (cleavage, cataclasis, etc.), regional metamorphism, structures of crystalline schists, regional metamorphism of non-calcareous sediments, calcareous sediments, and igneous rocks, plutonic intrusion in relation to regional metamorphism, repeated and retrograde metamorphism, eleven chapters in all.

Descriptive matter is reduced to a minimum and its place is taken by a series of beautifully drawn and most instructive figures of thin sections. Genetic considerations, chemical and physical, are always to the fore, and the principle of phase equilibrium is applied to the phenomena as far as is possible.

The book is certainly the most penetrating study of metamorphism that has ever been made, and its value is greatly enhanced by its luminous, exact, and dignified literary style. We believe that Dr. Harker's work provides the greatest stimulus to the study of metamorphism that petrologists have received during the present century. Our younger research workers in this field are indeed fortunate in having this book at their command.

G. W. T.

History of the Theory of Ore Deposits. With a Chapter on the Rise of Petrology. By T. CROOK, F.G.S. [Pp. 163.] (London: Thomas Murby & Co.; New York: D. van Nostrand & Co., Inc., 1933. 10*s*. 6*d*. net.)

THIS is a type of book which might well be repeated for other branches of geological science. It is salutary for scientific men to realise how much and how often they are indebted to their predecessors for the very theories which they regard as entirely new. Mr. Crook reviews most instructively and entertainingly the great controversy between Neptunists and Plutonists regarding the origin of ore deposits, especially mineral veins, which has continued in various guises from the eighteenth century down to the present time. After an introductory chapter Mr. Crook deals successively with the views of the ancients, the writers of the sixteenth and seventeenth centuries, first three-quarters of the eighteenth century and the last quarter in separate chapters, the four quarters of the nineteenth century in separate chapters, the twentieth century, with a final chapter on the Rise of Petrology.

Mr. Crook is a convinced supporter of the view that most mineral veins have been due to the action of sedimentary and metamorphic agencies, and that the modern igneous theory of metalliferous veins is fallacious. One of the arguments by which this conclusion is reached, namely, that 85–90 per cent. of the world's total annual output of minerals is from sedimentary and metamorphic concentrations, strikes us as a *non-sequitur* based on a confusion between the commercial and scientific importance of mineral deposits. Further if, as maintained, circulating exogene waters can leach out metallic salts from rocks, much more can the hot waters undoubtedly disengaged from igneous magmas do so, especially as they are in much more vital and intimate contact with the metalliferous constituents than are the colder and less chemically active exogene waters, and are moreover of nascent quality. The theory outlined in the first paragraph of p. 132 assumes that there has been but one irruption of primary sulphidic and metalliferous igneous matter into the earth's crust.

The chapter on the Rise of Petrology is excellent, and makes the much-needed point that petrology cannot claim completeness until it includes ore deposits as true rocks; but the author who, above all, has insisted on this point of view, Niggli of Zürich, does not even find a place in the index of authors. Mr. Crook prefaces his work with a valuable summary of views and arguments.

G. W. T.

BOTANY

Researches on Fungi, Vol. V. By A. H. REGINALD BULLER, F.R.S., D.Sc., Ph.D., LL.D., Professor of Botany in the University of Manitoba. [Pp. xvi + 416, with 174 figures.] (London, New York, Toronto: Longmans, Green & Co., 1933. 25*s*. net.)

IN this new volume of researches Prof. Buller considers the mechanism of spore dispersal in some non-hymenomycetous basidiomycetes and returns to the question of hyphal fusions, some aspects of which he had dealt with in his last published volume, while he also describes the movements of protoplasm and vacuoles through the pores commonly present in the cross-septa of hyphæ, the formation of pore-plugs and of intrahyphal hyphæ.

In the chapter on hyphal fusions, attention is drawn to the extraordinarily interesting recent observations of R. Vandendries and H. J. Brodie on the mutual repulsion which they find takes place between certain haploid mycelia of Lenzites betulina and which can be suppressed by a partition of lead, but not by one of mica or of glass. It would seem that some radiation is responsible for the observed phenomena, but while no other suggestion fits the facts, no known radiations have the properties required by the conditions of the experiment.

An examination of Sporobolomyces has shown that this fungus forms and discharges its spores in a manner typical of a basidiomycete, and here Prof. Buller would seem to have definitely settled the systematic position of this fungus, a matter which has been the subject of much discussion since its discovery. As the result of another set of observations reasons are given for regarding the so-called secondary conidium of Tilletia as the primary basidio-spore and the conidia produced on the mycelium formed from the so-called secondary conidium as secondary basidiospores. Prof. Buller gives reasons, including a comparison with allied forms, for regarding the structure termed originally by Brefeld the primary conidium as a sterigma. The arguments brought forward are certainly strong, but while it is difficult to suggest any other interpretation for these structures, the reasons given do not quite convince, especially as it is difficult on this view to account for the sterigmatic structures borne by the primary conidia.

The volume concludes with a study of the structure and discharge of Sphaerobolus. As usual the illustrations and the general get up of the book are admirable.

E. M. C.

Recent Advances in the Study of Plant Viruses. By Kenneth M. Smith, D.Sc., Ph.D. [Pp. xii + 423, with 1 coloured plate and 67 text figures.] (London: J. & A. Churchill, 1933. 15*s*. net.)

It is surprising that until this book of Kenneth M. Smith appeared no general survey of the literature of the virus diseases of plants had been put forward. The output of papers has been so large, and the accumulation of knowledge so progressive, that one might have expected long ago a collected account of the work of 40 years; but only memoirs on special sections and brief summaries in the technical journals have appeared hitherto. The book therefore meets a want, which had become almost a need. It is agreeably produced, of convenient size and clear type, and is well supplied with illustrations. Eleven chapters are devoted to a general account of the symptoms, properties, transmission and physiology of the viruses themselves and the diseases they cause, and to a detailed consideration of the relations between the insect vectors and the viruses they carry. These are followed by three chapters describing the various diseases, classified according to the hosts attacked; and to every chapter is attached a bibliography, which is more extensive and has been more utilised in the text than is often the case. A special feature, and one which is particularly valuable to plant pathologists to whom the relevant literature is not ordinarily accessible, is the attention given to the recent advances made by workers on the virus diseases of man and other animals. The book is intended to serve not only as a general survey, but as a reference book for both the general student and the specialist worker in plant virus research; and so far as it is really possible to combine these aims, it achieves its purpose. It can be heartily commended as a trustworthy, readable, comprehensive account of its subject-matter, and will be indispensable in every laboratory where plant viruses are studied.

J. Henderson Smith.

Plants Useful to Man. By Wilfred William Robbins, of the University of California, and Francis Ramaley, of the University of Colorado. [Pp. viii + 428, with 241 figures.] (Philadelphia: P. Blakiston's Son & Co., Inc., 1933. $3 post paid.)

The title of this book indicates a very wide scope and this indication is borne out by its contents which include ornamental garden plants, timber, resins, etc. A detailed account of any one particular aspect of the subject or of any one plant is therefore entirely out of the question, but an astonishing amount of information is contained concerning the history, introduction, geographical range and use of economic plants. The illustrations are numerous, well chosen, and, as a rule, well executed, although unfortunately there are a few crude drawings. The work is a very good introduction to economic botany and a useful supplement to the more usual systematic works.

E. M. C.

Plants and Human Economics. By R. GOOD, M.A. [Pp. vii + 202, with 8 maps.] (Cambridge: at the University Press, 1933. 5s.)

THIS is a useful little handbook to a subject upon which the literature is all too meagre. The plant products derived from over six hundred species are considered under such groupings as: Cereals and Pulses, Vegetables, Fruits, Timbers, Beverages, Drugs, Fodders, etc. Despite the number of species cited the text is not a mere catalogue and, though many are necessarily mentioned by name only, the salient facts are dealt with regarding most of the more important economic species.

There are some errors such, for example, as that pollination and the setting of seed are synonymous, that Chinese Artichokes are root tubers, and that Celeriac is a root vegetable. Having regard to the inclusions there are some curious omissions as, for instance, Cardoon amongst the vegetables and Pecan nuts amongst the fruits. An appendix gives a list of the species cited in the text with paginal references, but owing to its taxonomic arrangement this list has a very limited value for the non-botanical reader and does not serve as a substitute for the totally inadequate index. These minor defects will, however, doubtless be corrected in future editions of a work that is a handy introduction to a much neglected subject.

E. J. S.

ZOOLOGY

Animal Biology. By ROBERT H. WOLCOTT, Professor of Zoology in the University of Nebraska. [Pp. xvii + 615, with 335 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1933. 21s. net.)

THE compression of much matter into a small compass is, as any physicist or any writer will testify, a difficult task. Prof. Wolcott is certainly brave, for he has attempted to compress into a moderately sized volume an enormous mass of knowledge. In his first part, entitled "Fundamental Principles" he presents the relevant laws of physical chemistry (in 7 pages, starting with definitions of matter, atom, element and so forth) and of energetics (in a page and a half), and passes on to discuss life, protoplasm, cells, reproduction, mitosis and other similar topics. Then, in the second, third and fourth parts, he reviews in some detail the whole animal kingdom, phylum by phylum. Finally, in part five, he has a number of chapters on such questions as embryology, general physiology, behaviour, ecology, health and disease, animal distribution, fossils, evolution, genetics, classification and the history of zoology. All of this material, according to his introduction, is covered in a first-year course consisting of three recitation periods a week. What the course in question is like, the reviewer cannot tell, but as for the book, although its table of contents is most impressive, its text is disappointing. It is a book in which everything is mentioned but nothing is discussed. One gets from it an impression of breathless haste; topic after topic is taken up, glanced at and then immediately dropped to make way for the next.

The ideal first-year course in zoology has yet to be devised. Most of the students who take such a course will ultimately specialise in some other science and have only one year to spend on zoology, so it is only fair that they should be given a bird's-eye view of the animal kingdom, an insight into biological principles and method, and some idea of the main directions in which research is now proceeding. But the technical problem of how to present this material without continuous superficiality is an exceedingly difficult one. It is the reviewer's opinion that such courses should be given, but not set down in books. A lecturer can run along as he chooses—delaying and expanding where his subject is interesting, hurrying and cutting where it is dull or when time is short, and, if he knows his job, he can cover the ground adequately and also be stimulating at least part of the time. But the result of writing such a course down as a book may be bad. It depends whether you believe that graduates should be hand-made or mass-produced. The implication is obvious; between two covers you write the whole first-year course, and for a guinea the student can get all he needs to know. That savours of mass-production. But excellent texts are already in existence, describing the animal kingdom, or genetics, or evolution, or ecology, and it may well be urged that the student will get a much better idea of zoology by using several such texts, under the direction of a competent teacher, even if he only reads small parts of them, than by solemnly ingesting the contents of a single tabloid volume.

G. P. WELLS.

The Ecology of Animals. By CHARLES ELTON, M.A. Methuen's Monographs on Biological Subjects. [Pp. x + 97.] (London: Methuen & Co., Ltd., 1933. 3*s*. 6*d*. net.)

THE inclusion of a book on Animal Ecology in Messrs Methuen's series of Monographs on Biological Subjects is in itself a tribute to the author's pioneer work on this subject in this country.

After a brief introductory chapter, Mr. Elton treats with the methods and some of the more important results of ecological surveys. He then deals with the interrelationships between animals and the physical characters of habitats, leading up to a discussion in which animal numbers, in their main aspects, including the bearing of fluctuations in numbers upon evolution theories, are briefly but clearly considered. In the last chapter a survey of economic ecological problems is given.

Constant reference is made in the text to original work and a full list of recent publications is to be found at the end of the book, followed by a useful index.

This book thoroughly fulfils its purpose and should prove of great interest to teachers in all branches of Zoology. It explains the scope and interest of ecological work to the student and, to anyone wishing to work on ecological lines for the first time, it gives an excellent idea of the methods and problems involved.

N. H. H.

Mimicry. By G. D. HALE CARPENTER and E. B. FORD (Methuen's Monographs on Biological Subjects). [Pp. x + 134, with 2 illustrations.] (London: Methuen & Co., Ltd., 1933. 3*s*. 6*d*. net.)

THERE is an old and I believe a true story of a great scientific genius who, although he had invented something of great commercial value, could get no one to take up his invention. Heart-broken, he told a friend who happened to be a barrister and the friend, although having no technical knowledge of the subject, by means of questioning, made himself acquainted with the details of the invention. The next day the apparatus was taken up by the first firm to whom the barrister went, because he was able to explain its details in non-technical terms.

It seems to me that in all essays and lectures upon Mimicry, more than in any other subject in Zoology, the general reader with a taste for Biology, and indeed most Zoologists, are in the position of an audience listening to a foreign lecturer who, although speaking English, is frequently at a loss for a word and introduces one of his own which is unknown to the listeners. Those who tell us about Mimicry rely too much upon examples and they introduce them so generously that, meaning nothing to us, they seriously lessen the effect of the whole story. This book is no exception ; its argument is based too much upon examples unknown to the general reader. Nevertheless, admitting this handicap, I should describe it as a first-class refresher course for those already possessing a knowledge of the subject and with plenty of good stuff for less advanced students.

The book is divided into three parts, the senior author being responsible for the first two. Part I deals with the historical side and Part II, occupying the bulk of the volume, 80 pages, is entitled " Modern Evidence " and is mainly a series of field observations interpreted in the light of the observer's predilections as proofs of Mimicry, in which some may see too much proof. One is sometimes inclined to wonder whether the animals and insects themselves know the laws of Mimicry. I am not suggesting that all this is mere phantasy or that the extreme evidence is absurd, because every specialist must see more than the ordinary being. Among Entomologists, for instance, there are systematists who separate species where twelve good men and true among ordinary Entomologists would deny the existence of distinguishing specific characters. The real question is whether the specialist's judgment has become warped by intensive study or whether in the course of time, the jury will acquit him.

Part III is scarcely to be described as suitable for the general reader with a taste for Biology ; it is an essay on Genetics to Geneticists and in that light is very well developed.

FRANK BALFOUR BROWNE.

An Economic Entomology of the West Indies. By GEORGE N. WOLCOTT. [Pp. xviii + 688, with 111 figures.] (San Juan : The Entomological Society of Puerto Rico, 1933. $2.00 post paid.)

THIS book is intended for a somewhat varied public, including both students and planters. It is written, therefore, in a popular style, but gains rather than loses in consequence.

The volume starts with seven useful and simply written chapters on general considerations which include external anatomy, internal anatomy and physiology, development, names and classification, ecology and the chemical control of insects.

Then the pests are considered not in any order based on the systematic groupings of the insects themselves but according to the crop they attack. This arrangement of the material may be considered slightly unorthodox nowadays but probably in the future will be found the most usual and logical. It is the natural method of approach from the planters' point of view and also has much to be said in its favour on the basis that economic entomology is an integral part of applied ecology.

The pests of economic grasses first receive attention under such chapter headings as insects attacking the roots of sugar-cane, the root-boring weevils, insects attacking the stalk, caterpillars which attack cane leaves, insects which suck cane juice, those attacking grass and forage crops, etc. Then there is a section on pests of fibre crops. This includes chapters on insects attacking cotton and sisal. The next section treats of pests of trees including coffee, cacao and the coconut palm. The penultimate section deals with the pests of fruits which include citrus, banana, pineapple, papaya, mango, avocado and other fruits. Finally there is a short section on pests of garden crops, *e.g.* tobacco and vegetables.

The actual method of dealing with the individual pests does not follow any hard and fast outline, but where the injury is more obvious than the insect, the damage is described before the insect. Attempts have been made to indicate the points where present-day knowledge is lacking. The illustrations are adequate and form a valuable part of the text. The references to literature, which are not claimed to be complete but only selected, are numbered and placed either at the end of the several chapters or immediately after the paragraph to which they refer.

The book is apparently the first economic entomology covering the entire group of the West Indies, and so on this account alone must be of considerable importance. In addition the refreshing way in which it is written and arranged greatly enhances its value. Undoubtedly this book is a landmark and should be of immense help especially to students and planters for whom it was primarily designed. The price is reasonable and suitable.

H. F. B.

Nature and Nurture. By LANCELOT HOGBEN, M.A., D.Sc. [Pp. 144, with 2 figures and 21 tables.] (London : Williams & Norgate, Ltd., 1933. 6*s*. 6*d*. net.)

THIS book is intended as an introduction to the methods which have so far been devised for the study of human genetics. The investigation of heredity and variation in plants and animals can as a rule be conducted by means of direct experiments. In the human species, however, which is slow breeding, has few offspring, and cannot be mated at will, special methods must be employed. Clearly it is possible only to work backwards, subjecting various family trees to statistical analysis. A knowledge of appropriate statistical methods is thus indispensable to the student of human heredity. Many of those likely to be of value are here presented, together with a discussion of their limitations and cautions as to their use.

The advertisement on the dust-cover states that "the author has attempted to interpret the new methods of research in human genetics in a way which will be intelligible to readers with little mathematical knowledge." It is to be feared, however, that such readers are not likely to find that the attempt has been entirely successful.

The first chapter is devoted to a survey of the problems which are analysed later, and the book contains information of great interest on the relation between heredity and environment both in man and other organisms. Unfortunately its value is considerably reduced by a totally inadequate index and by the extraordinary omission of any bibliography whatever. A few references are given at the foot of the pages where the work in question is cited, though they are nowhere collected. More often, however, even these are omitted. Serious readers are certain to find this a source of annoyance and disappointment.

A number of valuable methods are, however, elaborated and brought together in this work. Furthermore, it contains ideas which are both original and interesting, and it could be read with profit both by geneticists and clinicians.

E. B. F.

MEDICINE

Handbook of Physiology. By the late W. D. HALLIBURTON, M D., LL D , F R C P , F.R S., formerly Professor of Physiology, University of London, King's College, and R. J. S. MCDOWALL, M.B , D Sc., F.R C P (Edin), Professor of Physiology, University of London, King's College Thirty-third edition. [Pp. xii + 971, with 382 figures and 4 plates.] (London : John Murray, 1933. 18s. net.)

THE form of this textbook has been determined by the requirements of the medical student, and the size, presumably, by tradition ; bearing in mind these limitations, the book is good. Since the last edition the first third has been arranged, purged, and brought up to date, while the remainder has been adequately revised. It should be pointed out to those unfamiliar with the work, that its implied dedication to the medical student carries with it certain corollaries. First, it is written with the object of giving instruction, and reads like the words of a teacher ; an interesting teacher, and one who has no doubt as to what he wishes his pupils to learn. In accordance with this, we find new or unfamiliar terms, and figures which should be memorised, emphasised by heavy black type. There is also a profusion of illustrations--nearly one to every two pages. Secondly, it is not a comprehensive book of reference, and the reader will find some parts of the subject treated much less fully than others. Thus chemistry and physical chemistry are reduced to a minimum, and the section on muscle and nerve is cut down to proportions almost commensurate with its clinical significance. The balance of the book has been carefully adjusted with respect to its place in the medical curriculum, but not with respect to the total amount of knowledge available in different branches of physiology. Thirdly, while the presentation of the subject-matter is sensible and up to date, and the book self-sufficient for ordinary purposes, its limitation as to size means that the reader must take much for granted. For instance, selecting at random, only three pages of print and three illustrations are devoted to a consideration of the thyroid gland ; this gives no scope for any sort of historical treatment, nor for anything more than a brief presentation of the conclusions "arrived at by observations, both on man and animals."

On the whole, the author is to be congratulated on having successfully completed a difficult task, in that he has carefully sifted and incorporated much new work, while preserving the original aims and size of the book. From the point of view of the teacher only one section—that on the nervous system—is open to adverse criticism, and even this applies to most textbooks of physiology. It is time that a complete break was made with the traditional anatomical approach to the nervous system. The student is taught to visualise how the circulation, for instance, "works," but it is doubtful whether he ever forms a satisfactory mental image of the nervous system in action. The recent work of Sherrington and others, who have demonstrated the play of influences on the "motoneurone pool" and their reflection in muscular contraction, should make an excellent basis for teaching, in that it gives the mind something concrete to grasp. The properties of the reflex arc are lifted out of the realm of mystery, and their importance in all forms of nervous activity becomes apparent. It is only fair to state that in the present work, the physiology of reflex action is treated as simply and clearly as one has seen it treated in a textbook, but it is to be regretted that the opportunity has not been taken to crystallise it in the form of the latest conceptions

As regards general features of production, the book maintains its previously high standard. The paper and binding are good, and the coloured plates well done. Blank pages have been inserted for notes at the end of every chapter, and if this turns out to be a feature worth repeating, one would suggest the addition of at least one such sheet at the end of the index, which, it should be remarked, is excellent.

W. H. N.

Indigenous Drugs of India. Their medical and economic aspects. By LIEUT.-COL. R. N. CHOPRA, M A., M.D. [Pp. xxii + 656.] (Calcutta: The Art Press, 1933. Rs. 15)

THIS is a work of exceptional interest in that the author not only describes the pharmacological and therapeutic actions of a wide range of drugs but also discusses economic problems connected with the drug supply of India.

In the first place the purely scientific merits of the volume are high, for the author has collected a mass of information regarding the properties of the drugs used in the systems of medicine indigenous to India. A large proportion of the scientific information provided in this chapter is based on systematic researches on the indigenous drugs which the author has conducted for many years. This information has not been collected previously and is now made available in a compact form, hence the volume forms an important contribution to the science of pharmacology.

Although the scientific merits of the volume are high, yet the discussions on the economic aspects of the drug supply of India are of even wider interest because it is rare to find any general consideration of such subjects. The nature of the problem that requires solution is well illustrated by the account given regarding the supply of anti-malarial drugs to India. The consumption per head per annum of quinine in India is only 3¼ grains as compared with corresponding figures of 24 grains in Greece and 16 grains in Italy. The reason for the low Indian consumption is that the great majority of sufferers from malaria do not receive treatment, indeed it is estimated that over 100 million persons in India suffer from malaria but that only 8 million receive even partial treatment. The annual consumption of quinine in India is about 200,000 lb. but the actual needs of the population are estimated to be at least one million lb. The reason for this failure in the treatment of malaria is that the cost of quinine is greater than the indigenous population can afford, and the author shows that the only remedy is for the Government to concentrate on trying to organise a supply of a potent anti-malarial drug at the cheapest possible price.

The author shows that a similar problem arises regarding many other essential drugs and draws the general conclusion that Western medical science has developed along lines that are not wholly suitable for the poverty-stricken masses of the Indian population. The general trend of Western medicine has been to develop therapeutics without much regard to the cost of treatment, for this is always low in comparison with the capital value of a life. The substitution of pure alkaloids for crude galenicals is one example of this tendency. The mass of the Indian population is however so poor that any expensive remedies are completely out of their reach and therefore useless as far as they are concerned. The problem in India is not so much to find the remedy that is best from the point of view of therapeutic science, but rather to find an effective remedy that is sufficiently cheap to be available to the patients.

Col Chopra points out that if medical treatment of the Indian population is to be developed every effort must be made to reduce its cost, and the main purpose of the volume under review is to encourage the utilisation of locally-produced drugs manufactured in a form suitable to Indian needs.

The author points out that India has such a wide variety of climate that practically all vegetable drugs can be grown there, but yet the drug imports of India are nearly five times the value of the exports.

Another purpose of the volume is to discover the value of the Ayurvedic remedies. This is a difficult problem and the difficulty is increased by the fact that the value of the Ayurvedic system has become to some extent a political issue. The author tactfully points out that this system reached its zenith previous to the Christian era and has stagnated or declined for the last 1,500 years. Consequently it is ridiculous to try to establish the Ayurvedic system as a rival to modern medical science. On the other hand the system deserves careful investigation because it was evolved by men of high talent, and probably many valuable drugs, unknown to Western medicine, are contained in its pharmacopoeia.

Col. Chopra has carried out researches into these drugs for many years and mentions a few of the difficulties attendant on such work. Almost every plant and shrub growing in India has had ascribed to it some medicinal value. Furthermore the mere identification of drugs is often a matter of extreme difficulty, for a number of different names are often applied to a single drug whilst sometimes several drugs share a common name. The recent discovery of the therapeutic properties of ephedrine suggests that the Oriental pharmacopoeias probably contain valuable drugs still unknown to the West. Unfortunately such valuable drugs form a very low percentage amongst a mass of useless preparations, and the task of finding them is severe in the extreme.

Col. Chopra is to be congratulated on his devotion to this self-imposed task and on this book which summarises and justifies his labours.

A. J. C.

Constitution and Health. By Raymond Pearl, Professor of Biology in the Johns Hopkins University, Baltimore. [Pp. 97, with 5 plates and 4 figures.] (London : Kegan Paul, Trench, Trubner & Co., Ltd., 1933. 2*s*. 6*d*. net.)

This little miniature is an inspiring account by an enquiring biologist searching for a relation between constitution and health.

The author regards "constitution," the total make up or "is what he is," as the result of combined hereditary and environmental factors; it is known directly from measurement of the individual and indirectly from the measurement of his kin. Owing to the number of variables involved and their ephemeral nature, it is difficult in the present state of our knowledge to make a comprehensive valuation of the individual make up.

To study the relation between human form and human nature "bodily habitus," regarded as an index of the constitution, was selected, and measurements were made of the primary and secondary axes of the body indicating respectively the types known as "asthenic" and "pyknic," with innumerable forms between the extremes of these types.

A sample of 290 white women distributed according to age, bodily habits, and general health, gave 0·2 asthenic, 0 25 pyknic and over 0·5 intermediate. In 78 6 per cent. the general health was excellent, fair in 13 1 per cent., and poor in only 8 3 per cent But none of these differences in general health in the several bodily habitus types could be regarded as significant; they are not greater than might be expected in random sampling.

The author insists that the science of constitutional medicine and pathology is statistical and requires the observation and measurement of the human organism in respect of both structural and functional characters.

P. J.

The Spread of Tumours in the Human Body. By R. A. Willis, M.D, B S., D.Sc, Melbourne [Pp. x + 540, with 103 illustrations.] (London: J. & A. Churchill, 1934. 25*s*. net.)

This volume is a valuable addition to the literature on cancer. It comprises an extensive review of previous work on the spread of cancer and relevant problems, interpreted in the light of the author's own work, which has included 323 autopsies in cases of malignant disease. These were performed "with the special object of ascertaining as completely as possible the modes of extension of the tumours and the situations of their metastases."

The book is divided into two parts. The first deals with more general aspects of the subject, and the second describes the secondary neoplastic disease of each individual organ or tissue. Apart from the value of the book as a work of reference, many of the topics discussed are of general interest to all students of malignant growth. Thus there are sections devoted to the discussion of the peculiarities of the distribution of metastatic neoplasms: the relation between malignant cells and their stroma: the problem of nerves in tumours and their significance, also the possibility of the innervation of tissues influencing tumour growth in them. Then we are reminded again of the challenge presented by those rare, inexplicable, yet highly significant cases of spontaneous regression of a primary tumour and its metastases.

As Dr. W. J. Penfold says in the general preface, this monograph "is suitable for systematic study by the pathologist, the surgeon, and the cancer research worker."

R. J. Ludford.

Inherited Abnormalities of the Skin and its Appendages. By E. A. Cockayne, D.M., F.R.C.P., Physician to the Middlesex Hospital. [Pp. x + 394.] (London: Oxford University Press, 1933. 32*s*. net.)

The nature of the "seed," *i.e.* the extrinsic causes of disease, and devising means for overcoming them, have been the main task of medical research since the pioneer work of Pasteur. The many benefits to humanity which have resulted from this concentration of effort are universally acknowledged, but, until recently, its imperfections have not been widely appreciated even in the medical profession. Owing to the work of some physicians, headed by Sir Archibald Garrod, the medical profession is recognising that an effort must be made to fill these gaps in its knowledge by studying the intrinsic causes of disease, the "soil," in correlation with those of the "seed." In attacking this problem one method, which gives great promise of success, is to study the influence of heredity in the cause and process of disease.

Owing to their visibility, the inherited defects of the skin are readily detected by the patient and diagnosed by the doctor; thus knowledge of the influence of heredity in disease of the less accessible internal organs will be built upon the accurate measurements of the dermatologist. Dr. Cockayne has performed a most valuable service in compiling, analysing and classifying a large store of information of human ectodermal defects, the origin of which is known or alleged to be due to heredity. The description of each lesion bears the imprint of the author's personal experience and critical mind, is illustrated by tables of statistics and pedigrees, and is furnished with an adequate bibliography. When the data preclude a definition of the plan of inheritance, Dr. Cockayne indicates what additional information is required and how it may be procured. To guide the dermatologist in recording accurate and detailed family histories the author explains the plan of mendelian inheritance in an intelligible and liberally illustrated introduction of 49 pages. For this purpose Dr. Cockayne doubtless would have incorporated the valuable pamphlet issued by the Eugenics Society, "How to prepare a family pedigree" (price 6*d*.), had its publication preceded that of his own book. A comprehensive subject-index completes a pioneer work of high merit, in the construction and furnishings of which the author and the publishers are to be warmly congratulated.

TOM HARE.

PHILOSOPHY AND THE HISTORY OF SCIENCE

The Diffusion of Culture. By G. ELLIOT SMITH, M.A., M.D., Litt.D., D Sc., Ch.M., F.R C.P., F.R.S. [Pp. x + 244, with 19 figures.] (London: Watts & Co., 1933. 7*s*. 6*d*. net.)

PROFESSOR ELLIOT SMITH is like the celebrated sailor giving evidence in a court of law: "Did I say the horse was fifteen feet high? Then if I said so, I sticks to it." Professor Elliot Smith once said that all civilisation came from Egypt, therefore in the face of overwhelming proof to the contrary he sticks to his original statement. Though it is possible sometimes to admire the tenacity which refuses to acknowledge defeat, that tenacity can be carried too far in intellectual matters. Professor Elliot Smith has never appeared to realise that archæology has always recognised intercourse between nations and countries; but no archæologist, least of all an Egyptologist, can admit that civilisation emanated from Egypt alone. Egypt contributed her share, but she received as much as she gave, and the large number of foreign arts imported into Egypt, even in prehistoric times, are proof of this. The inaccuracy of the author's statements, which he offers as proof, annihilates his arguments. Take, for example, his statement as to mythical monsters on p. 188, "The earliest evidence of a composite animal, the age of which can be estimated, is the representation of the goddess Hathor on an Egyptian slate-palette of about 3400 B.C. She is shown as a woman equipped with cow's ears and horns. On the same palette the king is shown as a falcon with a human head." In these three sentences there are at least four mistakes or misleading statements. The earliest examples of the "composite animal," and they are many, come from the palæolithic caves in France and England. The head on the slate-palette has a buffalo's (not a cow's) horns, and there is no proof that it represents either a woman or the goddess Hathor; the identification is conjectural and still uncertain. The falcon has a human arm, not a human head; the human-headed falcon does not appear till many centuries later. When the evidence for the author's favourite dogma rests on inaccuracies so glaring, comment is needless

M. A. MURRAY.

A Record of the Scientific Work of Sir James Dewar. Printed for private circulation at the Chiswick Press, London, 1933. [Pp. 68.] 5*s*.

MR. HENRY YOUNG has performed a useful service in compiling this classified bibliography of the scientific writings of the late Sir James Dewar. The list of Dewar's papers in the Royal Society's *Catalogue of Scientific Papers* is incomplete, as the Catalogue ends with the year 1900; and although the papers themselves can be consulted in the *Collected Papers of Sir James Dewar* (1927) and the *Collected Papers on Spectroscopy* (Liveing and Dewar, 1915), these volumes are not always accessible. A separate hand-list of the titles of the papers, with the references to them, has obvious uses.

Mr. Young has added some brief biographical notes and references to the writings of others on Dewar and his work While the full biography of Sir James Dewar remains unwritten, these notes are likely to be particularly useful. They serve as a guide to the scattered sources of information on the life of this distinguished experimenter, whose researches constitute one of the brilliant chapters in the history of the Royal Institution, where he lived and worked for forty-six years.

T. M.

Richard Trevithick. The Engineer and the Man. By H. W. DICKINSON and ARTHUR TITLEY. Trevithick Centenary Commemoration Memorial Volume. [Pp. xvii + 290, with 18 plates and 41 figures.] (Cambridge: at the University Press, 1934. 10*s*. 6*d*. net.)

THE Trevithick Centenary Commemoration Committee decided as one of its aims to publish a memorial volume, and the joint authors of this work are to be congratulated upon the painstaking care with which they have approached their task, assembled the data and presented it in a most readable form: indeed it appears to have been in reality a labour of love.

Francis Trevithick, the son of Richard, had published in 1872 a two-volume work dealing in considerable detail with the life of his illustrious father, but the new book, though based largely on the earlier one, will appeal to a far wider public—to students of human nature as well as to the engineer. It is as impossible to summarise briefly Richard Trevithick's inventions as to weigh up with accuracy his personal character, and though, in regard to the former, Trevithick has frequently been spoken of as the father of the locomotive, it would in reality be more true to claim him as the father of the high-pressure steam engine: it was for steam he lived, fought and died. It is correct to state that he designed the first locomotive, the successful trials of which took place at Pen-y-daren on February 21, 1804, and that the first satisfactory trial with a road locomotive of Trevithick's design was at Camborne on Christmas Eve, 1801, but Richard Trevithick, himself the son of a mining engineer, was first and foremost a mining engineer, and neither his first nor his last love was locomotion. His pumping engines, far superior to those of Boulton and Watt, with which firm his relationship was always strained, were responsible for saving the Cornish mining industry at a crucial time, whilst his efforts in developing the silver mines of Peru which ended so disastrously thanks to the overthrow of the Spanish rule by Bolivar, and his chance meeting with young Robert Stephenson in the Cartagena inn, form what to many will doubtless be the most fascinating section of this attractive book

Trevithick's experiments in ship propulsion by paddle wheels and later by a screw, as well as his activities in portable house heaters, dredging, salvage, ordnance, factory and agricultural power plants, unloading ships' cargoes and the Thames Tunnel will probably come as a surprise, both as to their variety and as to their number With many faults, of which perhaps the worst was overpowering optimism, Richard Trevithick possessed a most attractive personality : large in stature, generous and with an extraordinary aptitude for taking decisions quickly, he had but little patience and could not suffer fools gladly Rich at many times but destined to lose everything as often, his petition to Parliament for financial aid in 1831 was unavailing; "I find that Mr. Spring Rice cannot get the Lords of the Treasury to agree to remunerate or assist me in any way" are the pathetic words in one of his letters, and in another in regard to the refusal to provide steam gauges, "So obstinate is John Bull that nothing short of an Act of Parliament will compel him to keep his brains safe in his own skull."

The authors tell Trevithick's story largely in his own words by his correspondence—they could not have made a better choice. This book, admirably produced, with excellent reproductions of his rough sketches, a good index and bibliography, together with a pedigree of the family, connected with engineering for five generations, should be read by all who desire light on the inventive and scientific mind: by that means alone can the country be safeguarded against the possibility of the present inventor receiving as little consideration, encouragement and financial aid as did Trevithick. He died almost in poverty a hundred years ago, but his descendants have kept bright the name of Trevithick as notable engineers.

C. E. R. SHERRINGTON.

The Heroic Age of Science. By W. A HEIDEL. [Pp. viii + 204.] (Baltimore: The Williams & Wilkins Company, for the Carnegie Institution of Washington, 1933; London: Baillière, Tindall & Cox. 12*s*. 6*d*. net.)

IN this book, the author has sought "to illustrate by examples, drawn generally from the broad field of Greek thought, the conception, ideals, and methods of science." In those brave days, when the world of scientific thought was young, the pioneers were less trammelled than their modern successors in framing gallant theories to explain difficult problems. One would wish to know how far these theories were the result of brilliant inspiration, how far the outcome of logical thought, to what extent based on or checked by experiment. Such questions Professor Heidel attempts to answer.

The distinction between philosopher and scientist was less frequently drawn in ancient Greece than it is to-day, and almost half the book is occupied with considerations about the laws and processes of thought. The author seems to have some slight misgivings (p. 87) about the length of this introduction, which will not prove easy reading for those who are not trained in philosophy.

The final section, on " Experimentation," is likely to appeal to the working scientist as the most interesting part of the book. The author justly points out that the desire of the Greek writers to present a rounded-off account in logical form usually involved an omission of most of the experimental details. " The records of experimentation among the Greeks are clearly not representative of their practice." He has been remarkably successful in drawing forth what is hidden, and we are prepared to agree with his conclusion that " in the later centuries of ancient Greek history experiments in physics and other branches of science were being constantly made." The work impresses the reader as being the result of long and profound study of the scattered literature of Greek science.

A table of the approximate dates of the writers quoted would have added to the utility of the book. It is well produced, and misprints are rare, although, oddly enough, two of those noticed by the reviewer occur in a single line on p. 100.

K. C. B.

The Lamarck Manuscripts at Harvard. Edited by W. M. Wheeler and T. Barbour. [Pp. xxi + 202, with four plates.] (Cambridge, Mass : Harvard University Press. London : Humphrey Milford, 1933. 12*s*. 6*d*. net.)

This book contains the French text and English translation of six holographic manuscripts by Lamarck in the library of the Museum of Comparative Zoology at Harvard. The first is of a lecture on Gall's system of phrenology , the second consists of two articles subsequently published in a *Dictionary of Natural History*, one headed " idea " and the other " imagination." They are partly psychological and partly epistemological. The third manuscript is entitled *Appercu Analytique des Connoissances Humaines*, and also forms part of a subsequent publication. The fourth consists of eleven " zoological questions " and the fifth is a draft of a portion of the first edition of Lamarck's history of invertebrates. The sixth and last manuscript is a fragment of a report of a botanical excursion.

The editors have added an introduction in which the history of the manuscripts is discussed and some particulars of Lamarck's life are given. It also includes a long quotation from an article by F. G. Crookshank in which the psychological peculiarities of Darwin and Lamarck are speculated upon and compared.

J. H. W.

MISCELLANEOUS

Heredity and the Social Problem Group. By E. J. Lidbetter. Vol. I. [Pp. 160, with 26 diagrams.] (London : Edward Arnold & Co., 1933. 21*s*. net.)

This book is the first volume of a series presenting detailed records of a number of family groups of the class which seek Poor Relief. They have been collected with special reference to the hereditary aspect underlying destitution; the object being to show that such exists, and to provide the information necessary for studying it, rather than to prove any particular contention with respect to the relative importance of nature and nurture. For this reason all but the briefest attempt to draw conclusions from the facts has been postponed until the series is complete.

The work is based on a vast mass of material collected by the author and a number of voluntary helpers in a selected poor law area in East London between 1910 and 1928. Though brought to a standstill during the War, the immensity of the task actually accomplished by Mr. Lidbetter cannot fail to strike every reader of this book very forcibly. There is no doubt that he has amassed a mine of information to which social workers, eugenists, and geneticists will be able constantly to turn; nor will they easily exhaust its resources.

The present volume contains twenty-six pedigrees These have been arranged with short introductory notes and tabulated statistics; but the main body of the work consists of the diagrams, printed on large folders, which illustrate each of them. These embody an excellent system of symbols from which it is possible to determine not only the relationship within each group, but a considerable amount of other necessary information. Thus the sex, nature of chargeability, the mental state, and the occurrence of certain diseases and disgenic tendencies can be ascertained at a glance for every individual tabulated. Those about whom no particulars are known are carefully marked, and their relative rarity is a tribute to the thoroughness of the investigation.

Among the more important results achieved, is the demonstration that families in which mental deficiencies appear tend to intermarry. As pointed out by the author, this may help to explain the striking variety of forms which the inherited taint assumes in related individuals. For the drift towards homozygosity which follows from inbreeding will cause the segregation of various recessives. It is also to be noticed that even the same genetic factor may have different effects in different internal environments.

The data presented in this book can leave no doubt that heredity plays some part in bringing members of the community to the asylum and the workhouse. Those who believe that all men would be equal if given equal chances would do well to consult it before disseminating such a doctrine.

E. B. F.

Inland Waters of Africa. By S. and E. B. WORTHINGTON. [Pp. xix + 259, with 40 plates, 13 figures and a map.] (London: Macmillan & Co., Ltd., 1933. 15*s*. net.)

DR. E. B. WORTHINGTON has taken part in two expeditions to the lakes of Central and East Africa, each lasting for a whole year; on the second, which he led, he was accompanied by his wife. The primary object of the expeditions was to study the fisheries of these lakes and to see what improvements could be effected, but in order to understand the fishes the whole fauna of the lakes had to be investigated, and in order to understand the fauna the physical and chemical features of the waters and even their past histories were explored. Moreover, as the success of a fishery depends very largely on the fishermen, a large amount of information was collected about the native populations of the regions in question. In the volume under review, the Worthingtons give a popular account of their travels. The most valuable chapters are those in which the authors present the scientific results of the expeditions. They describe very fascinatingly how all the different animals in a lake depend on each other, being linked together into "food-chains" in which each link devours the next; they explain clearly how the past history and geological entourage of a lake determine its fauna; they show that new species of fish can evolve in less than 15,000 years. But the scientific story is enlivened by countless little anecdotes that give the picture additional colour—a herd of elephants tearing up banana trees for fun—a little ship caught in a storm on Lake Victoria with her port-holes open—a local chief in "a garment looking like a night-shirt but reaching to the ground, over which he wore a black-and-white check Norfolk jacket"—a hitherto unexplored island on Lake Rudolf, whose basking, fish-gorged crocodiles "scarcely deigned to look at us unless we kicked them." Forty pages of photographs, mostly taken by the authors, complete a very amusing account of a first-rate piece of ecological investigation.

G. P. Wells.

The Romance of Research. (A Century of Progress Series) By L. V. Redman and A. V. H. Mory. [Pp. x + 149, with 6 figures.] (Baltimore: The Williams & Wilkins Co. London: Baillière, Tindall & Cox. 1933. 5s. net.)

This little volume, written largely from the point of view of a citizen of the United States, gives a brief outline of the origins and development of scientific research mainly as applied to the improvement of the material welfare of humanity. It is attractively written, free from technicalities and more for the general reader than the scientific specialist. Perhaps the most interesting part of the book is Chapter X, which deals with the economics of research, explaining the appreciable lag between increased expenditure on the investigation and adaptation of a new process and the returns consequent thereon. As Lord Rutherford said in a recent address, Americans are more scientifically minded than the people of this country, and this book is therefore unlikely to come into the hands of those in Britain who should read it, namely the manufacturers and producers who, with some notable exceptions, have but the vaguest ideas of the influence of research on every aspect of life, not excluding that of making profits.

O. L. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Analytical Geometry of Three Dimensions. By D. M. Y. Sommerville, M.A., D.Sc., F.N.Z. Inst., Professor of Pure and Applied Mathematics, Victoria University College, Wellington, N.Z. Cambridge: at the University Press, 1934. (Pp. xvi + 416, with 49 figures.) 18*s*. net.

Introduction to General Topology. By Wacław Sierpiński, Professor of Mathematics at the University of Warsaw. Translated by C. Cecilia Krieger, Lecturer in Mathematics at the University of Toronto. Toronto: The University of Toronto Press. London: Humphrey Milford, 1934. (Pp. x + 238.) 17*s*. net.

Exploring the Upper Atmosphere. By Dorothy Fisk. With an Introduction by Henry Leopold Brose, D.Phil., D.Sc. Illustrated by Leonard Starbuck. London: Faber & Faber, Ltd., 1934. (Pp. 166, with 5 figures.) 6*s*. net.

The General Principles of Quantum Theory. By G. Temple, Ph.D., D.Sc., Professor of Mathematics in the University of London at King's College. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1934. (Pp. viii + 120.) 3*s*. net.

Lehrbuch der Physik in elementarer Darstellung. By Arnold Berliner. Fifth edition. Berlin: Verlag von Julius Springer, 1934. (Pp. viii + 736, with 847 figures.) RM.19.80.

The Riddle of the Universe To-day. By Joseph McCabe. London: Watts & Co., 1934. (Pp. x + 250.) 5*s*. net.

Wave Mechanics. Advanced General Theory. By J. Frenkel, Professor at the Physico-Technical Institute, Leningrad. The International Series of Monographs on Physics. Oxford: at the Clarendon Press. London: Humphrey Milford, 1934. (Pp. viii + 525, with 3 figures.) 35*s*. net.

Electricity. Book I. By W. F. F. Shearcroft, B.Sc., A.I.C., King's School, Peterborough, and E. E. Whitehead, M.Sc., Leyton County School. Modern School Physics. London: George G. Harrap & Co., Ltd., 1934. (Pp. 178, with frontispiece and 104 figures.) 2*s*.

Electricity. Book II. By W. F. F. Shearcroft, B.Sc., A.I.C., King's School, Peterborough, and E. E. Whitehead, M.Sc., Leyton County School. Modern School Physics. London: George G. Harrap & Co., Ltd., 1934. (Pp. 240, with frontispiece and 123 figures.) 2*s*. 6*d*.

School Certificate Test-Papers in Physics. By A. H. Cooper, M.Sc., B.A., Senior Science Master, Watford Grammar School. London: George G. Harrap & Co., Ltd., 1934. (Pp. 93.) With Answers, 1*s*. 6*d*.; Without Answers, 1*s*.

Resonance Radiation and Excited Atoms. By Allan C. G. Mitchell, Ph.D., Assistant Professor of Physics, New York University, and Mark W. Zemansky, Ph.D., Assistant Professor of Physics, College of the City of New York. The Cambridge Series of Physical Chemistry. Cambridge: at the University Press, 1934. (Pp. xvi + 338, with 84 figures.) 18*s*. net.

The Sub-Atoms. An Interpretation of Spectra in Conformity with the Principles of Mechanics. By William Mayo Venable, Consulting Engineer to Blaw Knox Company, Pittsburg, Pennsylvania. Baltimore: The Williams & Wilkins Company. London: Baillière, Tindall & Cox, 1933. (Pp. viii + 148, with 11 figures.) 9*s*. net.

The Physics of Electron Tubes. By L. R. Koller, Ph.D., Research Laboratory, General Electric Company. International Series in Physics. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xiv + 205, with 67 figures.) 18*s*. net.

Industrial Radiography. By Ancel St. John, Ph D., and Herbert R. Isenburger. New York. John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1934. (Pp. x + 232, with 70 figures) 21*s*. 6*d*. net.

Fundamentals of Hydro and Aeromechanics. By O. G. Tietjens, Ph.D. Based on Lectures of L. Prandtl, Ph.D. Translated by L. Rosenhead, Ph D. Engineering Societies Monographs. New York and London: McGraw-Hill Book Company, Inc, 1934. (Pp. xvi + 270, with 186 figures.) 24*s*. net.

Applied Hydro- and Aeromechanics. By O. G. Tietjens, Ph.D. Based on Lectures of L. Prandtl, Ph.D. Translated by J. P. Den Hartog, Ph.D. Engineering Societies Monographs. New York and London: McGraw-Hill Book Company, Inc., 1934. (Pp. xvi + 311, with 226 figures and 27 plates.) 24*s*. net.

A Text Book of Applied Hydraulics. By Herbert Addison, M.Sc., Assoc.M.Inst.C.E., Professor at the Royal School of Engineering, Giza, Egypt. London: Chapman & Hall, Ltd, 1934. (Pp. xii + 409, with 315 figures.) 21*s*. net.

The Principles of Heating and Ventilation By H M Vernon, M.A., M.D., Member of the Institution of Heating and Ventilating Engineers. London: Edward Arnold & Co., 1934. (Pp. viii + 232, with 52 figures.) 14*s*. net.

Atomic Theory and the Description of Nature. Four Essays, with an Introductory Survey. By Niels Bohr. Cambridge: at the University Press, 1934. (Pp. vi + 119.) 6*s*. net.

A Text-Book of Inorganic Chemistry for University Students. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at East London College. Fourth edition. London: Macmillan & Co., Ltd., 1933. (Pp. viii + 1062, with 390 figures.) 15*s*. net.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc., F.R.S. Vol. XIII. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. viii + 948, with 381 figures.) 63*s*. net.

Handbuch der Anorganischen Chemie. Abegg, Auerbach, Koppel. IV. Band, 3. Abteilung: Die Elemente der 8. Gruppe des periodischen Systems. Dritter Teil: Kobalt und seine Verbindungen. Edited by Dr. I. Koppel, Berlin. Leipzig: S. Hirzel, 1934. (Pp. xvi + 626, with 171 figures.) RM.58, paper covers.

The Atom. By John Tutin, D.Sc. (Durham). With an Introduction by Professor Frederick Soddy, F.R.S. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. 109.) 6*s*. net.

Introductory Colloid Chemistry. By Harry N. Holmes, Professor of Chemistry at Oberlin College. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1934. (Pp. xiv + 198, with 34 figures.) 15*s*. 6*d*. net.

Laboratory Manual of Colloid Chemistry. By Harry N. Holmes, Professor of Chemistry in Oberlin College. Third edition. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1934. (Pp. xviii + 229, with 60 figures.) 20*s*. net.

A Treatise on the Catalytic Action of Surfaces. By J. E. Nyrop. Copenhagen: Levin & Munksgaard. London: Williams & Norgate, Ltd., 1934. (Pp. 75.) 7*s* net.

Conductometric Analysis. Principles, Technique, Applications. By Hubert T. S. Britton, D.Sc., B.Sc., D.I.C., F.I.C., Lecturer in Physical and Inorganic Chemistry, University College of the South-West of England, Exeter. Series of Monographs on Applied Chemistry, Vol. VIII. London: Chapman & Hall, Ltd., 1934. (Pp. xii + 178, with 49 figures.) 12*s*. 6*d*. net.

The Phenomenon of Superconductivity. Edited by E. F. Burton, Ph.D., F.R.S.C., Head of the Department of Physics and Director of the Physical Laboratory, University of Toronto. Toronto: The University of Toronto Press. London: Humphrey Milford, 1934. (Pp. 112, with 28 figures.) 8*s*. 6*d*. net.

Free Radicals. A General Discussion held by the Faraday Society, September 1933. London: Gurney & Jackson for the Society, 1934. (Pp. iv + 248, with 5 plates and numerous figures.) 12*s*. 6*d*. net, postage 6*d*.

The Thermodynamics of Electrical Phenomena in Metals. By P. W. Bridgman. New York and London: Macmillan & Co., Ltd., 1934. (Pp. viii + 200, with 33 figures.) 16*s*. net.

The Alloys of Iron and Tungsten. By J. L. Gregg, Metallurgist, Battelle Memorial Institute. Alloys of Iron Research, Monograph Series. New York and London: McGraw-Hill Book Company, Inc., for The Engineering Foundation, 1934. (Pp. xii + 511, with 184 figures.) 36*s*. net.

A Manual of Foundry Practice. By J. Laing, A.M.I.Brit.F., Foundry Superintendent, Messrs. J. Stone & Co., Ltd., Deptford, and R. T. Rolfe, F.I.C., Chief Metallurgist, Messrs. W. H. Allen, Sons & Co., Ltd., Bedford. London: Chapman & Hall, Ltd., 1934. (Pp. viii + 276, with 150 figures, including 40 plates.) 15*s*. net.

Tautomerism. By John William Baker, D.Sc., Ph.D.(Lond.), A.R.C.S., F.I.C., Lecturer in Organic Chemistry in the University of Leeds. London: George Routledge & Sons, Ltd., 1934. (Pp. viii + 332, with 4 figures.) 25*s*. net.

Organic Syntheses: Vol. XIV. W. W. Hartman, Editor-in-chief. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1934. (Pp. viii + 100.) 10*s*. 6*d*. net.

Soil Analysis. A Handbook of Physical and Chemical Methods. By C. Harold Wright, M.A., F.I.C., formerly Senior Agricultural Chemist, Nigeria. London: Thomas Murby & Co. New York: D. van Nostrand Co., Inc., 1934. (Pp. viii + 236, with 6 figures.) 12*s* 6*d*. net.

Technical Gas Analysis. By George Lunge, Ph D., Dr. Ing. Revised and rewritten by H. R. Ambler, Ph.D.(Lond.), F.I.C., Chemist, Research Department, Royal Arsenal, Woolwich. London: Gurney & Jackson, 1934. (Pp. xvi + 410, with 138 figures.) 21*s*. net

Volumetric Analysis By H P. Starck, M.A.(Cantab.), Head of the Science Department, The Technical College, Kingston-on-Thames. London: Baillière, Tindall & Cox, 1934. (Pp. viii + 228, with 11 figures.) 7*s*. 6*d*. net.

Physico-Chemical Practical Exercises. By William Norman Rae, V.D., M.A.(Cantab.), F.I.C., Professor of Chemistry and Physics, Royal College of Surgeons in Ireland, and Joseph Reilly, M.A.(Cantab), D.Sc., Sc.D., D.-ès.-Sc., F.Inst.P., F.R.C.Sc I., F.I C., Professor of Chemistry, National University of Ireland, Cork. London: Methuen & Co., Ltd., 1934. (Pp. xvi + 276, with 74 figures.) 7*s* 6*d*. net

Elementary Chemical Theory and Problems. By N. M. Shah, M.Sc., Department of Chemistry, Karnatak College, Dharwar. Dharwar: V.Y. Jathar, Karnatak Printing Works, 1933. (Pp vi + 146, with 6 figures.) Re 1.

Dip and Strike Problems mathematically surveyed. By Kenneth W. Earle, D.Sc. (Lond.), F.G.S., late Senior Assistant in the Department of Geology, University College, London. London: Thomas Murby & Co., 1934. (Pp. x + 126, with 122 figures and 1 folding plate.) 12*s*. 6*d*. net.

Les Ressources minérales de la France d'outre-mer. Part I: Le Charbon. Paris: Société d'Editions géographiques, maritimes et coloniales, 1933. (Pp. vi + 245, with 33 figures.) Frs. 24.

Les Ressources minérales de la France d'outre-mer. Part II: Le Fer. Paris: Société d'Editions géographiques, maritimes et coloniales, 1934. (Pp. iv + 436, with 59 figures.) Frs. 36.

The Dinosaurs. A Short History of a Great Group of Extinct Reptiles. By W. E. Swinton, Ph.D., F.R.S.E., of the British Museum (Natural History). London: Thomas Murby & Co., 1934. (Pp. xii + 233, with 25 plates and 20 figures.) 15*s*. net.

Ocean Waves and Kindred Geophysical Phenomena By Vaughan Cornish, D.Sc. With additional notes by Harold Jeffreys, M.A., D.Sc., F.R.S. Cambridge: at the University Press, 1934. (Pp. xvi + 164, with 26 plates.) 10*s*. net.

Manometric Methods as applied to the measurement of cell respiration and other processes. By Malcolm Dixon, M.A., Ph.D., Sc.D., Rockefeller Lecturer in Biochemistry in the University of Cambridge. With a Foreword by Sir F. G. Hopkins, President of the Royal Society. Cambridge: at the University Press, 1934. (Pp. xii + 122, with 19 figures.) 5*s*. net.

Mikroskopische Technik. By Heinz Graupner. Taschenbuch der biologischen Untersuchungsmethoden 1. Leipzig: Akademische Verlagsgesellschaft m b H., 1934. (Pp. viii + 157, with 31 figures.) Paper covers RM. 5.20; stiff board RM. 5.80.

The Generic Names of British Insects. Prepared by the Committee on Generic Nomenclature of the Royal Entomological Society of London, with the assistance of the Department of Entomology of the British Museum (Natural History). Part 1. Recommendations relating to the publication of the committee's reports. Part 2: The generic names of the British Rhopalocera with a check list of the species. London: Royal Entomological Society of London, 1934. Part 1 (pp. 6), 6*d.*; Part 2 (pp. 40), 3*s.* 6*d.*

Insects as Material for Study. Two Inaugural Lectures delivered on 17 and 24 November, 1933. By G. D. Hale Carpenter, D.M., Hope Professor of Zoology (Entomology). Oxford. at the Clarendon Press London: Humphrey Milford, 1934. (Pp. 38, with 1 plate) 2*s.* 6*d.* net.

The Elements of Experimental Embryology. By Julian S. Huxley, M.A., Honorary Lecturer in Experimental Zoology, King's College, London, and G. R de Beer, M.A., D.Sc., Fellow of Merton College, and Jenkinson Lecturer in Embryology, Oxford Cambridge: at the University Press, 1934. (Pp. xiv + 514, with 221 figures.) 25*s.* net.

Introduction to Cytology. By Lester W. Sharp, Cornell University. Third Edition New York and London: McGraw-Hill Book Company, Inc., 1934. (Pp. xiv + 567, with 230 figures.) 30*s.* net

An Introduction to Biology. By L M Parsons, D Sc., D.I.C., Science Master, Westminster City School. London: Macmillan & Co., Ltd., 1934. (Pp. viii + 178, with 87 figures.) 2*s.* 6*d.*

Report on the North Sea Cod. By Michael Graham, M.A., Naturalist on the Staff of the Ministry of Agriculture and Fisheries. Fisheries Investigations Series II, Vol XIII, No. 4, 1933. London: H.M. Stationery Office, 1934. (Pp. 160, with 33 figures.) 7*s.* net.

Anatomy of Animal Types. For students of Zoology. By E. A. Briggs, D.Sc., Assistant Professor of Zoology in the University of Sydney. Sydney: Angus & Robertson, Ltd. London: The Australian Book Company, 1934. (Pp. xx + 250.) 10*s.* 6*d.* net.

The Anatomy of the Rhesus Monkey (Macaca mulatta). Edited by Carl G. Hartman, Department of Embryology, Carnegie Institution of Washington, and William L. Straus, Jr., Department of Anatomy, Johns Hopkins University, Baltimore. London: Baillière, Tindall & Cox, 1933. (Pp. x + 383, with frontispiece and 128 figures, including 6 plates.) 27*s.* net.

The Behaviour of Animals. An Introduction to its Study. By E. S. Russell, O.B.E., D.Sc., F.L.S. London: Edward Arnold & Co., 1934. (Pp. viii + 184, with 6 plates and 26 figures.) 10*s.* 6*d.* net.

Man's Place among the Anthropoids. Three Lectures on the Evolution of Man from the Lower Vertebrates. By William King Gregory, Ph.D., F.G.S., F.Z.S., Professor of Vertebrate Palæontology, Columbia University. Oxford: at the Clarendon Press, London: Humphrey Milford, 1934. (Pp. vi + 119, with 11 figures.) 6*s.* net.

Adam's Ancestors. An up-to-date outline of what is known about the origin of man. By L. S. B. Leakey, M.A., Ph.D., F.S.A., F.R.A.I. With a foreword by Sir F. Gowland Hopkins. London : Methuen & Co., Ltd., 1934. (Pp. xx + 244, with 12 plates and 30 figures.) *7s.* *6d.* net.

Psychology and Psychotherapy. By William Brown, D.M.(Oxon.), D.Sc. (Lond.), F.R.C.P., Wilde Reader in Mental Philosophy in the University of Oxford. Third edition. London : Edward Arnold & Co., 1934. (Pp. viii + 252.) *12s.* *6d.* net.

The Mammalian Red Cell and the Properties of Hæmolytic Systems. By Eric Ponder, Washington Square College, New York University. Protoplasma-Monographien, Vol VI. Berlin : Gebruder Borntraeger, 1934. (Pp. xii + 311, with 52 figures) RM. 22.50.

Clinical Studies on the Physiology of the Eye. By J. Grandson Byrne, M.A., M.D. London : H. K. Lewis & Co., Ltd., 1934. (Pp. x + 144, with 49 figures) *10s* *6d* net.

Vital Cardiology. A New Outlook on the Prevention of Heart Failure. By Bruce Williamson, M.D.(Edin.), M R C P.(Lond.), Physician, Royal Northern Hospital, London. Edinburgh . E. & S. Livingstone, 1934. (Pp. viii + 344, with 7 figures.) *15s.* net.

The Origin of Cancer. By J. P. Lockhart-Mummery, M.A., M.B., B.C. (Cantab.), F.R.C.S. (Eng), Senior Surgeon, St. Mark's Hospital. London : J. &. A. Churchill, 1934. (Pp x + 150, with 29 figures.) *10s.* *6d.* net.

The Chances of Morbid Inheritance. Edited by C. P. Blacker, M.C., M.A., M.D., M.R.C.P., General Secretary of the Eugenics Society. London : H. K. Lewis & Co., Ltd., 1934. (Pp. xii + 494, with illustrations and charts.) *15s.* net.

The Influence of Heredity on Disease. Buckston Browne Prize Essay, 1933. By L. S. Penrose, M.A., M.D. London : H. K. Lewis & Co , Ltd., 1934. (Pp. viii + 80, with 4 figures.) *5s.* net.

Life in the Making. By Alan Frank Guttmacher, Associate in Obstetrics, Johns Hopkins University. London : Jarrolds Publishers (London), Ltd., 1934. (Pp. 288, with 8 figures.) *10s.* *6d.* net.

Recent Advances in Sex and Reproductive Physiology. By J. M. Robson, M.D., B.Sc. (Leeds), F.R S.E., Beit Memorial Research Fellow, Institute of Animal Genetics, University of Edinburgh. London : J. & A. Churchill, 1934. (Pp. x + 249, with 47 figures.) *12s.* *6d.* net.

Keith Lucas. By various contributors. Cambridge : W. Heffer & Sons, Ltd., 1934. (Pp. 131, with 2 illustrations.) *5s.* net.

Science in an Irrational Society. By H. Levy, Professor of Mathematics at the Imperial College of Science, University of London. Being the Conway Memorial Lecture delivered 25th April, 1934. London : Watts & Co., 1934. (Pp. x + 82.) *2s.* net.

The Progress of Science. An account of Recent Fundamental Researches in Physics, Chemistry and Biology. By J. G. Crowther. London : Kegan Paul, Trench, Trubner & Co., Ltd., 1934. (Pp. x + 304, with 12 plates and 31 figures.) *12s.* *6d.* net.

A Leechbook or Collection of Medical Recipes of the Fifteenth Century. Edited by Warren R. Dawson, F.R.S.E., F.R.S.L., F.S.A. (Scot.). London : Macmillan & Co., Ltd., for the Royal Society of Literature of the United Kingdom, 1934. (Pp. vi + 344.) 20*s*. net.

Combustion from Heracleitos to Lavoisier. By Joshua C. Gregory, B.Sc., F.I.C., Lecturer in Chemistry at Leeds University. London : Edward Arnold & Co., 1934. (Pp. viii + 231.) 10*s*. 6*d*. net.

Smoke and the Atmosphere. Studies from a Factory Town. By J. R. Ashworth, D.Sc., formerly Honorary Research Fellow of the University of Manchester. Manchester : at the University Press, 1933. (Pp. xii + 131, with 30 figures.) 7*s*. 6*d*. net.

Unbalanced Budgets. A Study of the Financial Crisis in Fifteen Countries. By Hugh Dalton, M.A., D.Sc. (Econ.), Brinley Thomas, M.A., Ph.D., J. N. Reedman, B.A., T. J. Hughes, B.A., and W. J. Leaning, B.A. London : George Routledge & Sons, Ltd., 1934. (Pp. xii + 468.) 15*s*. net.

Hints for Self-Culture. By Har Dayal, M.A., Ph.D. London : Watts & Co., 1934. (Pp. x + 366.) 5*s*. net.

The Restrictive Law of Population. By Professor Johan Hjort, Ph.D., Sc.D., For. Mem.R.S. Huxley Memorial Lecture, 1934. London : Macmillan & Co., Ltd., 1934. (Pp. 46, with 2 figures.) 1*s*. net.

The Twilight of Parenthood. A Biological Study of the Decline of Population Growth. By Enid Charles, M.A., Ph.D. London : Watts & Co., 1934. (Pp. viii + 226, with 7 figures.) 7*s*. 6*d*. net.

Adult Education in Practice. Edited by Robert Peers, M.A., Head of the Department of Adult Education, University College, Nottingham. London : Macmillan & Co., Ltd., 1934. (Pp. xiv + 301.) 7*s*. 6*d*. net.

An Introduction to the Teaching of Science. A Revision of Teaching Science in the Schools. By Elliot Rowland Downing, Department of Education University of Chicago. U.S.A. : University of Chicago Press. Great Britain and Ireland : Cambridge University Press, 1934. (Pp. viii + 258.) 9*s*. net.

The Laboratory : its Place in the Modern World. By D. Stark Murray, B.Sc., M.B., Ch.B., Assistant Pathologist, Archway Group Laboratory, L.C.C. The Fen Series No. 8. London : The Fenland Press, 1934. (Pp. 117.) Paper, 2*s*. ; Cloth, 3*s*. net.

Catalogue of Lewis's Medical and Scientific Lending Library. Second Supplement 1931–1933. London : H. K. Lewis & Co., Ltd., 1934. (Pp. 112.) 2*s*. net.

Creation's Doom. By Desiderius Papp. Translated by H. J. Stenning. London : Jarrolds Publishers (London), Ltd., 1934. (Pp. 286, with 8 plates and 17 figures.) 12*s*. 6*d*. net.

SCIENCE PROGRESS

1932—AND AFTER: THE NEW PHYSICS OF THE NUCLEUS

By N. FEATHER, Ph.D.
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To those instructed and kept informed almost entirely through the medium of the popular press, 1932 must doubtless have appeared as the first year of a new era in physical science. There is, of course, a measure of truth in this judgment, but there is a great deal more of exaggeration. Since 1932 fundamental research has been abundantly fruitful of new discoveries over a wide field—herein is the element of truth, the exaggeration lies in the over-emphasis on novelty and the failure to appreciate the extent to which successful scientific research, in almost all cases, draws upon the past for its inspiration. New discoveries appear in the course of time, very rarely with a "Lo here!" or, "Lo there!", but rather by diffused advance from many sides, until somewhere or other the offensive breaks through and all available forces are thrown in to consolidate the position newly gained.

In this article then, although its chief concern is with the more important results of recent experimental work in the domain of nuclear physics, it will be necessary to devote some time to a consideration of work of an earlier date and a wider variety of content. If these excursions are not suggested by our title, the need for brevity there must be the excuse, only the title which has been adopted must not be taken as implying in the writer that variety of epochism which is apt to afflict popular historians—of science, or of any other branch of human activity. Against the falsity of such epochism we have already inveighed.

January 1932, without being epochal, does, however, provide a most convenient date from which to follow the ever-quickening advance of nuclear research, and our first concern will therefore be to sketch as briefly as possible the state of affairs at that time, by presenting, in little, the conceptual scheme in terms of which scientists sought to describe—or explain—the phenomena of atomic and sub-atomic order as they were then known. To these scientists matter in the gaseous state, in the ultimate chemical analysis, consisted of molecules, themselves formed each by the union of a small number of atoms according to established rules. In the solid state and to some extent in the liquid state, also, there was not the same simplicity, that is to say the same absence of grosser structure, but again chemical atoms might be regarded as the ultimate structural units for most purposes. Although some two hundred or more different species of atom had been recognised—and this was obviously a very incomplete list, from the point of view of the chemist there were natural groupings of different species so that only about 90 distinct chemical "elements" were known—and here, by contrast, there was some reason to suppose that 92 represented the maximum number of distinct elements likely to be discovered. The physicist, extending the analysis one stage farther, regarded the atom as an electrical structure with which mass was associated, one general type of structure being common to all atoms. The greater part of the atomic mass was concentrated in a minute nucleus having a resultant positive charge, whilst a distribution of negative "electrons" in motion around this nucleus accounted for the remainder of the mass. These negatively charged particles—the electrons—were identical in nature from atom to atom, their number in any atom being just sufficient to balance the positive charge on the nucleus, which charge, on this account, was restricted to integral multiples of the fundamental electron-unit. The maximum charge found, on the heaviest known nucleus, was 92 times this unit, the minimum charge exactly equal to the unit. Here, then, was the "explanation" of the 92 chemical elements. The nuclear charge determined the number and arrangement of the extra-nuclear electrons—the "atomic number" —and, almost identically, their energies of binding to the nucleus. Chemical properties were determined by the arrangement and, in quantitative detail, by the binding energies of the outermost electrons; but, as stated, this arrangement and, to a very good approximation, the binding energies were independent of the mass of the nucleus, for a given charge. The distinction between the different species which occurred grouped together in nature in a single chemical element—as "isotopes" of that element—was primarily this chemically unimportant distinction of mass as between the nuclei of atoms of one species and those of atoms of another. Moreover, it was found possible to choose a unit of mass such that the nuclear masses of all atomic species were very nearly integral multiples of the chosen mass. The discrepancy, however, was in many cases well beyond experimental error and it became convenient to discriminate between nuclear mass and nuclear "mass number." The former was the exact mass in terms of the chosen unit, the latter number was the integer to which the number expressing the exact mass very nearly approximated. Given the occurrence of groups of isotopic species and the nearly integral nuclear masses, it was natural to regard the nuclei themselves as built up, on a common structural plan, from positively charged constituents identical with the nucleus of the hydrogen atom (as a simplest assumption) and electrons or else particles of a single kind (again as a simplest assumption) having mass—necessarily very nearly equal to, or very nearly a submultiple of, the mass of the hydrogen nucleus—but possessing no charge. Moreover, since there was no direct experimental evidence at the time for the existence of these hypothetical uncharged particles, the former supposition alone was generally current. It was modified only by the assumption that there might occur in complex nuclei nuclear sub-groups themselves constituted of the elementary particles. There was experimental evidence, so it seemed, not only for the presence of electrons and hydrogen nuclei as constituent particles in the nuclei of other elements, but also for the occurrence of at least one such sub-group as a structural unit. This sub-group appeared to be identical with the nucleus of the helium atom, and thus to be constituted of four hydrogen nuclei and two electrons. It was found to be emitted spontaneously with great velocity from the nuclei of certain heavy "radioelements" and, under suitable circumstances, to be capable of assimilation in the nuclei of a number of lighter elements from which, in general, a hydrogen nucleus was in consequence released. Likewise cases of the spontaneous emission of high-velocity electrons from heavy and from certain medium-heavy nuclei were well established. It is when particles, satisfactorily named for other purposes, possess high velocity and so in virtue of their kinetic energy are endued with exceptional properties that it becomes convenient to assign other and more individual names to them. This contingency had arisen, long before 1932, in the case of the nuclei of hydrogen and helium, as may be seen from the above account. The names adopted for these particles in this connection were, for the hydrogen nucleus, "proton"; for the helium nucleus, "α-particle." Electrons spontaneously emitted from nuclei fall into much the same category. Such electrons were renamed β-particles. Returning to the heavy nucleus, one necessary condition for the acceptance of any proposed structure in terms of smaller units was that the mass of the nucleus should be less than the sum of the masses of the constituent particles in the free state. This "mass defect" was a measure of the total energy of binding of the particles in the nucleus, the conversion factor as between mass and energy in absolute units being the measure of the square of the velocity of light in the system of units employed. The condition of a positive mass defect was easily fulfilled by the suggested structures which employed protons, electrons and α-particles as structural units.

One portion of the conceptual scheme has now been described, the entities involved having had this in common, that so far they have been characterised by mass and electric charge, only. Now the scheme was designed to describe phenomena of atomic and sub-atomic order and these phenomena involve, in many cases, transformations of energy which cannot be formalised entirely in terms of the kinetic and potential energies of material particles. An additional concept was required for their description. From rather vague beginnings in the early years of the century the concept of the quantum of electromagnetic radiation had developed and become definite, again long before 1932. According to this view, whenever the potential energy of an isolated atom was in excess of the minimum value characteristic of that grouping of structural units, then eventually the additional energy would be radiated as a quantum (or several quanta) of radiation. It was possible, further, to localise the region of excess energy and to speak of the quantum as emitted from the extra-nuclear electron structure or from the nucleus. Certain cases were known of nuclei emitting quanta spontaneously within a short interval of time after the ejection of an α-particle or a β-particle. These quanta, and generally all quanta emitted by nuclei, under whatever circumstances, were termed quanta of γ-radiation. The energy of a quantum was propagated with the velocity of light, and although the propagation had many of the characteristics associated with wave motion on classical theory—in that frequency and wavelength had a definite meaning in this connection—there was the corpuscular aspect, also, that at any stage in the propagation the total amount of energy which was emitted in the original atomic radiation process was available for transformation, through interaction between the quantum and an atom, into kinetic or potential energy of particles. Likewise it had come to be accepted that, when energy was propagated as the mass of particles, the same two aspects of the propagation were again relevant; the corpuscular aspect alone had been considered in classical mechanics, the wave aspect became important for sub-atomic particles under certain conditions of separation and relative velocity.

At this stage, when the conceptual scheme has been developed until most questions concerning the physics of atoms and radiation can be dealt with within its framework, we may well leave it, emphasising again the reasons which have prompted its presentation in this form. It was necessary to indicate, as briefly as possible, the situation to which research in the field of atomic and sub-atomic physics had led by the end of 1931. It was obviously impossible to describe, from the experimental side, the various lines of investigation which had been pursued and it was therefore decided to suggest, instead, the current modes of thought and expression adopted by physicists, making some attempt to define those entities in terms of which they most frequently discussed the significance of the direct results of their experiments—spectra obtained upon photographic plates, readings of electrometers, oscillograph records or cloud tracks in expansion chambers. The conceptual scheme to which a scientist holds is at no time final, he does not make the claim that the structure of Nature is after such and such a pattern, but merely insists that he can satisfactorily correlate the results of most, if not of all, his experiments by the help of this pattern. At this stage, then, we may proceed to particularities.

Radioactive elements provided the first evidence for the break-up of atomic nuclei and a study of their radiations indicated the order of magnitude of the energy changes involved in such processes. The successive transformation theory of Rutherford and Soddy, put forward in 1903, long antedated the nuclear model of the atom, but it fixed upon the essential truth that an atom of a distinct chemical species invariably results when a radioactive atom disintegrates with the emission of a particle—whether α-particle or β-particle. This fact we may now formalise by writing a nuclear equation to represent the disintegration. Thus for the disintegration of an atom of radium we should write

$$\mathrm{Ra}^{226} = \mathrm{Rn}^{222} + \mathrm{He}^{4} + \mathrm{E} \quad . \quad . \quad . \quad . \quad . \qquad \text{(i)},$$

where superscripts are the mass numbers of the nuclei involved (atomic numbers might be inserted as subscripts, but are here omitted, since they are unambiguously specified by the chemical symbols themselves) and E indicates the liberation of energy in the process. This energy is in the form of kinetic energy of the α-particle and recoil nucleus, except in a small fraction of the cases in which about 4 per cent of the energy is emitted in the form of a quantum of γ-radiation. If the symbols with superscripts represent the exact masses of the nuclei then (i) becomes a true equation between quantities, when E represents, in mass units, the amount of energy liberated. In what follows nuclear equations will always be understood in this sense. Equation (i) is that of a "first order" nuclear reaction—radioactive disintegration is spontaneous.

In like manner by use of the high energy particles emitted in radioactive transformation the first example of the disintegration of a normally stable nucleus was observed. In 1919 Rutherford detected the protons produced in the disintegration of nitrogen by α-particles. We should now write

$$N^{14} + He^{4} + E_{\alpha} = O^{17} + H^{1} + E_{R} + E_{P}$$

or

$$N^{14} + He^{4} = O^{17} + H^{1} + Q \quad . \quad . \quad . \quad . \quad . \quad (ii),$$

since it is found that for all such disintegrations $E_R + E_P - E_\alpha$ has a constant value. These three quantities are the kinetic energies of the recoil nucleus (oxygen, of mass number 17), the proton and the α-particle, respectively. It will be noted that equation (ii) is that of a second order nuclear reaction. With present resources it seems quite impossible that third order nuclear reactions should be studied in the laboratory.

Between 1919 and 1932 a great deal of attention was paid to the cases of artificial disintegration produced by α-particles. With a few important exceptions, all the elements of atomic numbers 5 to 18 (that is, the nuclei of one species, at least, for each element) proved susceptible of disintegration. In each case a proton was emitted, but the conditions of disintegration were not always as simple as those found to hold for the disintegration of nitrogen. In particular we may say that the quantity Q in the nuclear equation

$$X^{A} + He^{4} = Y^{A+3} + H^{1} + Q \quad . \quad . \quad . \quad . \quad . \quad (iii),$$

instead of having a single value only, with certain elements would assume one or other of a small number of discrete values depending upon the individual disintegration process for which it was calculated. Since the sum of the masses on the left-hand side of equation (iii) is a definite quantity, different values of Q must imply different values of Y^{A+3}, the mass of the resultant complex nucleus, if it be assumed that the mass of the proton is always the same. In other words, the nuclei Y^{A+3}, as formed in this process, must sometimes possess energy over and above the normal. Now it was not to be supposed that such an "excited state" could persist for long in a complex nucleus and evidence was soon forthcoming that, where excited nuclei were formed as a result of disintegration, nuclear γ-rays were also emitted. Rough measurements on the absorption of this radiation seemed to show that the energy differences $Q_1 - Q_2$, etc. deduced from observations on the proton emission were, in fact, the quantum energies appropriate to the γ-rays observed. This agreement strengthened the supposition that those nuclei, which, following the disintegration, had been left in a state of excitation, very quickly got rid of their excess energy in the form of radiation. This behaviour is exactly analogous to that of radioactive nuclei left excited in the course of their spontaneous transformations. Now, because the close study of the γ-rays emitted by such nuclei had provided a very extensive body of data, which, if it did not at once suggest a quantitative theory of nuclear structure, was at least available as a stringent test for any theory which might be proposed, about this time physicists were beginning to show an active interest in the γ-rays emitted from the lighter nuclei under the conditions which have been described. Due mainly to the smallness of the effects, such investigations are extremely difficult; they were, however, destined to lead to very important developments.

Before describing those developments one further point must be made. Between 1919 and 1932 artificial disintegration experiments had been carried out entirely through the use of radioactive sources of α-particles. There was the convenience of simplicity in this procedure and the very real advantage that the kinetic energies available were much greater than could be obtained by other means. A potential difference of some millions of volts applied to a discharge tube is necessary to produce projectiles artificially of comparable energy, and this was obviously impracticable at the time. But there were other considerations which were having an important influence upon events. It appeared probable that the large kinetic energy necessary before an α-particle was at all effective in producing disintegrations was intimately bound up with two factors. Firstly, on account of its charge, it suffered repulsion in its approach to the nucleus to be disintegrated, and in this way was frequently unable to get close enough to that nucleus to influence its stability. The proton, possessing only one-half the charge, would penetrate farther—roughly to one-half the previous minimum distance—for the same kinetic energy. Secondly, the α-particle itself, being a tightly bound structure, carried with it very little internal energy available for disintegration purposes, even if other conditions for disintegration were satisfied. The mass of the proton, on the other hand, being greater than the quotient (exact nuclear mass) ÷ (mass number) for any other nucleus, there was available energy in the form of mass which might make for a very efficient projectile, for the purpose in hand. As early as 1929, therefore, experiments were in progress at the Cavendish Laboratory in an attempt to develop methods of applying potentials of the order of half a million volts to suitably designed discharge tubes. After three years of continued endeavour these experiments bore fruit.

However, if even approximate chronological order is to be maintained, we must return to the investigations concerning nuclear γ-rays which have already been mentioned. As previously suggested, it was absorption measurements, indicating great penetrating power, which confirmed the suspicion that radiation of the γ-ray type was emitted from those elements—such as boron and aluminium—which gave rise to more than a single group of protons under α-particle bombardment. But in the same series of experiments Bothe and Becker showed that radiation of a similar penetrating power was emitted when other light elements were similarly bombarded. Thus lithium and beryllium, from which no protons had been detected, gave rise to a penetrating radiation These observations were made in 1930. In the following year Curie and Joliot confirmed this result, whilst experiments being performed by H. C. Webster in Cambridge were leading to the same conclusion. In the case of beryllium, for example, it was thought that simple capture of the α-particle must be taking place with the formation of the carbon nucleus, C^{12}, and the emission of radiation, but different estimates of the quantum energy did not give concordant results and there were obviously unexplained features in the observations confronting any such hypothesis. Then, in January 1932, Curie and Joliot showed that this same radiation was capable of communicating large amounts of energy to hydrogen nuclei, so that protons of several million electron volts energy were set in motion by it. For some time longer the quantum hypothesis was adhered to, but difficulties only multiplied, until about a month later Chadwick suggested that a particle, not a quantum, was involved. This conclusion was rendered probable as the result of experiments showing that the nuclei of other light atoms might also receive considerable energy through the action of the radiation. This new suggestion was accorded immediate and wide acceptance.

Quantitative considerations fixed the mass of the particle at about unity on the atomic scale and the fact of its great penetration at the energies involved showed that it could not possess any resultant charge. This particle was the neutron, very similar indeed to the hypothetical unit discussed by Rutherford in 1920. The nuclear equation representing its production by the action of α-particles on beryllium may be written

$$Be^{9} + He^{4} = C^{12} + n^{1} + Q \quad . \quad . \quad . \quad . \quad . \quad . \quad (iv),$$

where, again, further research has shown that the energies of the neutrons can only be explained by assuming a number of discrete values for Q, and thus certain possible excited states for the resultant carbon nucleus. The γ-rays corresponding to the emission from these excited states have now been detected in the general radiation produced in beryllium by α-particle bombardment, and it may similarly be stated that the penetrating radiation from lithium, boron, fluorine, sodium, magnesium and aluminium consists in part of γ-radiation and in part of neutrons. The problem of investigating the γ-rays accompanying artificial disintegration in light elements has thus become enormously complicated, but this alone does not invalidate our original assumption, that eventually this investigation will yield information of considerable importance.

Alternative schemes of nuclear structure have already been indicated With the discovery of the neutron there occurred an overwhelming swing of competent opinion, away from the electron and proton as structural units, in favour of a scheme in which neutron and proton were so employed. Whilst it is not obvious that such a scheme will be successful in taking count of the spontaneous emission of electrons from nuclei, we may, for the present, shelve this particular difficulty, pointing out, in passing, that it still remains possible to retain the α-particle as a subsidiary structural unit. According to the new scheme the α-particle itself is constituted of two protons and two neutrons, whilst the simplest complex nucleus would contain one each of these units. It is interesting to observe that the discoveries of the neutron and of the atomic species possessing this nucleus (" heavy hydrogen," H^2, discovered by Urey, Brickwedde and Murphy) were announced within two months of one another. The relative abundance of H^1 and H^2 in ordinary hydrogen is probably about 10^4 to 1.

The α-particle was regarded as a sub-unit in nuclear structure because it was ejected from some nuclei and, under suitable conditions, could be made to enter into the structures of other nuclei. There are now equally good reasons for accepting the neutron as a nuclear constituent. Certain cases of the emission of neutrons have already been discussed , evidence for the reverse process, in which a neutron is captured by a complex nucleus, was first obtained by the writer towards the end of February 1932. When the radiation from a polonium-beryllium source (polonium in order to provide α-particles with the minimum of accompanying γ-radiation) is passed through an expansion chamber the cloud photographs occasionally show short tracks due to heavily ionising particles—these are the nuclei which have been set in motion by elastic collision—and, less frequently, pairs of heavy tracks having a common origin. Plate I, Fig. 1, is a reproduction of one of the first of such photographs. It was the observation of these pairs of tracks which prompted the suggestion that in certain cases the neutron was, in fact, causing the disintegration of nuclei. Measurement and calculation showed this to be the case. The original experiments were with nitrogen; there most of the results can be explained in terms of the nuclear equation

$$N^{14} + n^1 = B^{11} + He^4 + Q \quad . \quad . \quad . \quad . \quad . \quad \text{(v)}.$$

The energy release, Q, once more appears to be a multiple valued quantity and the assumption is involved that more powerful methods of investigation will eventually demonstrate the emission of γ-ray quanta from the excited boron nuclei formed in the reaction. Similar expansion chamber experiments have since been made by Harkins, Gans and Newson and by other workers. In addition to nitrogen, carbon, oxygen, fluorine and neon have been shown to suffer disintegration by capture of the neutron and emission of an α-particle, whilst there is some evidence that certain nuclei may disintegrate in other ways, say with the emission of a proton or a heavy hydrogen nucleus. In the case of carbon there is very direct evidence that an alternative mode of disintegration is possible. Chadwick, Feather and Davies have recently published one pair of photographs showing three heavy tracks originating in a single point in space. It appears likely that in this collision the neutron has caused the break-up of the carbon nucleus into three α-particles without itself being captured, although the explanation involving capture of the neutron is not entirely excluded. A reproduction of the two photographs concerned is given in Plate I, Figs. 2 and 3.

If the extent of our knowledge of disintegration by neutrons, as derived from direct observation, is as yet somewhat meagre, there is abundant evidence of an indirect nature that almost all nuclei are susceptible of disintegration in this manner. Attention has already been drawn to the effect of the charge of the α-particle and the (proportionately smaller) effect of the charge of the proton in preventing these particles, except at high energies, from approaching sufficiently near to heavy nuclei to be effective for disintegration. The neutron is afflicted with no such disability. We must, however, postpone our discussion of the disintegration of heavy nuclei by neutrons until the present chronicle has been brought up to date in other directions.

A fortnight before the neutron hypothesis was put forward to account for the properties of the beryllium radiation, Cockcroft and Walton announced the successful production of an intense beam of protons from a discharge tube to which a potential difference of 700,000 volts was applied. Eleven weeks later they published the results of their first attempt to produce nuclear disintegration by means of these particles. This attempt, too, had been singularly

successful. Lithium had been chosen as the element to be bombarded and subsequent experiments have confirmed the wisdom of this choice and the correctness of the suggestion which was then put forward to explain the results. We write the nuclear equation

$$\mathrm{Li}^7 + \mathrm{H}^1 = \mathrm{He}^4 + \mathrm{He}^4 + Q \quad . \quad . \quad . \qquad . \quad \text{(vi)}$$

to show that capture of the proton in this case results in the production of two α-particles which together possess kinetic energy in amount Q over and above the kinetic energy of the proton. As far as experiment has shown, Q is a single valued quantity ; it has, in fact, very closely the value which might be predicted from the accurate knowledge now available concerning the nuclear masses of the species H^1, He^4 and Li^7. In the language of chemistry, the nuclear reaction represented above is strongly exothermic ; energetically it would be possible even when the kinetic energy of the proton became vanishingly small. Cockcroft and Walton obtained indications that disintegration still occurred when the potential difference across the discharge tube was reduced to 100,000 volts and later work with more powerful sources has given a positive result below 30,000 volts accelerating potential. The reaction remains possible but the probability that any individual proton is effective falls off rapidly and continuously as its energy is decreased. This is just the behaviour predicted by the wave theory of matter ; here is a case where the wave aspect of the interaction of particles becomes relevant at the expense of the corpuscular aspect. From the experimental point of view the result itself, and the manner of its discovery, form an interesting commentary on the megalomania of the age—for it penetrates into scientific research also—very obviously the lesson of David and the five smooth stones has yet to be taken to heart! Here, however, we must return to our subject—the disintegrations produced by high-velocity protons, remarking that, if higher and higher velocities are not absolutely essential for further success, those who trust to this method must reflect with satisfaction that their colleagues who do not are faced with the problem of producing greater and greater numbers of protons to keep pace with their investigations. We must return to our subject—though in our digression we have already suggested the course which its development has taken. In those laboratories where high voltage research was already under way the general results of Cockcroft and Walton were rapidly confirmed ; in many where it was not the necessary apparatus was installed with all convenient speed. Now one is kept continually speculating whether the next results will be announced from California or Kharkov—or from any one of a dozen other centres of research. Up to the present only hydrogen and helium nuclei accelerated "artificially" have been used for disintegration experiments, but there appears to be no reason why future developments should be confined to the use of these particles. With helium nuclei very little work has been done and no nuclear processes shown to occur which had not already been established with the α-particles from a radioactive source. With protons, on the other hand, there has been considerable progress. Cockcroft and Walton first showed that the results with lithium were not as simple as they had originally announced and later experiments of Oliphant, Kinsey and Rutherford and of Dee (using an expansion chamber in conjunction with a high voltage discharge tube) made very probable the conclusion, which has since been confirmed by Oliphant, Shire and Crowther, through direct separation, that not only the heavier but also the lighter isotope of lithium suffers disintegration under proton bombardment. The nuclear equation

$$Li^6 + H^1 = He^4 + He^3 + Q \quad . \quad . \quad . \quad . \quad . \quad (vii)$$

indicates the production of an unknown species of helium and the determined value of Q, the energy balance in the process, permits of an accurate deduction of its nuclear mass. It has yet to be discovered whether this type of helium nucleus is permanently stable or not.

Even more interesting, however, than the results with high velocity protons are those which have recently been obtained using heavy hydrogen in the discharge tube. The initial success here is to the credit of Lewis, Livingston and Lawrence, who obtained α-particles from lithium bombarded by heavy hydrogen nuclei. [A situation thus arises, as it did for α-particles and protons, making it convenient to assign a distinctive name to the particle concerned. "Deuton," "deuteron" and "diplon" have been suggested for the heavy hydrogen nucleus, but usage has not as yet, standardised any one of these to the complete exclusion of the others.] Because this α-particle emission from lithium was found to be complex various complementary lines of investigation have been required in order to afford a satisfactory analysis. As might be expected, the capture of H^2 by Li^6, on the one hand, and that of H^1 by Li^7, on the other, lead to essentially the same disintegration. Two α-particles are emitted, in very nearly opposite directions. Similarly, $B^{10} + H^2$ and $B^{11} + H^1$ give the same resultant. In each case three α-particles are set free. But the above is not the only reaction between heavy hydrogen and lithium nuclei. It has been found that protons and neutrons are emitted. Plate II, Fig. 4, shows a pair of oppositely

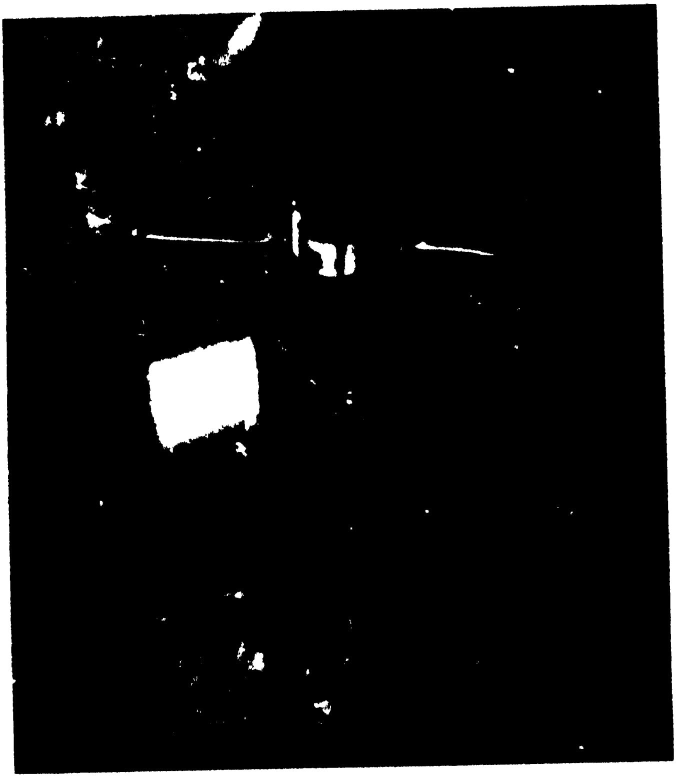

Fig. 4

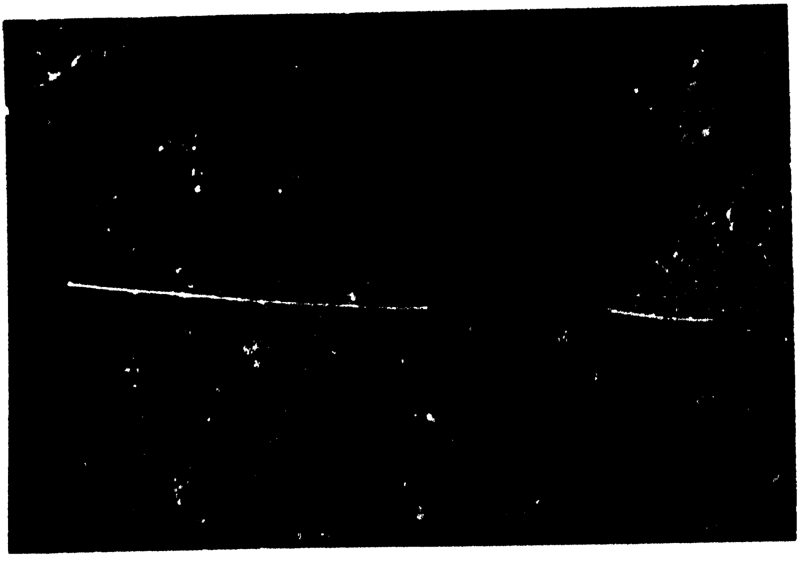

Fig. 5.

directed α-particle tracks and a proton track. The two modes of disintegration of Li^6 by H^2 are here illustrated in a single photograph.[1] For the protons and neutrons we may write

$$Li^6 + H^2 = Li^7 + H^1 + Q_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(viii)}$$

$$Li^7 + H^2 = He^4 + He^4 + n^1 + Q_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(ix).}$$

The former of these equations exhibits the formation of a heavier from a lighter isotope (simultaneously, of course, the nucleus of the heavier isotope of hydrogen disappears and that of the lighter isotope is released). A similar reaction probably takes place when many elements are bombarded; already it has been followed in some detail with boron and carbon. And, which is most peculiar of all, this reaction takes place when heavy hydrogen itself is the substance bombarded. It will be seen from equation (x) that the production of a further unknown species of hydrogen nucleus is involved,

$$H^2 + H^2 = H^3 + H^1 + Q \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(x).}$$

Plate II, Fig. 5, is an expansion chamber photograph taken by Dee, showing the tracks of the two particles resulting from this disintegration. Recent investigations by Bleakney and his co-workers seem to show that naturally occurring hydrogen contains about one atom in 10^9 of this new species, whilst a knowledge of its mass also suggests that it is stable. Production of the isobaric nucleus, He^3, postulated to explain the proton-on-lithium disintegration results, also occurs in an alternative mode of synthesis-disintegration of two heavy hydrogen nuclei. The reaction

$$H^2 + H^2 = He^3 + n^1 + Q \quad . \quad . \quad . \quad . \quad \text{(xi)}$$

has not yet been followed in all its particulars, but the emission of neutrons has been established and their energies agree closely with what might be expected. It is almost impossible, therefore, to escape the conclusion that the nucleus He^3 is simultaneously released.

It has already been remarked that nuclei other than those of hydrogen and helium may well prove to be useful projectiles with which to carry out disintegration experiments, but it is now obvious that, even within these categories, completely unexplored possibilities remain, for there can be no certain prediction what the properties of H^3 or He^3 will prove to be, if these particles can be accelerated in the present type of apparatus.

Avoiding speculation, however, we have now indicated various types of disintegration which are known to occur when neutrons, protons and heavy hydrogen nuclei are the particles effective. All these processes have been investigated entirely within the period of our survey. Within that period, also, there has been steady progress towards a full description of many cases of disintegration produced by α-particles. Much of this work has been in continuation of that carried out prior to 1932, and, since its general results follow the broad lines suggested by our discussion of equation (iii), we shall not be concerned with further details here. There is one aspect of these results, however, of which equation (iii) takes absolutely no account. This formal statement shows what happens when disintegration takes place ; it gives no indication how the probability of disintegration (per unit length of path) depends upon the kinetic energy of the α-particle concerned. We have already discussed the parallel question in regard to proton-produced disintegrations. Here the evidence points to a rapid and continuous falling off of disintegration probability as the kinetic energy of the proton is reduced. In the α-particle case there is the same general rapid decrease in efficiency with decreasing energy, but the decrease is not continuous. For certain discrete energies, characteristic of the nucleus bombarded, the disintegration probability rises to a maximum value, to fall rapidly to normal again as the energy of the α-particle is further reduced. This phenomenon of " resonance " interaction is one of the more successful predictions of the wave theory of matter in this field ; it is quite unintelligible from the purely corpuscular viewpoint. Resonance interaction between protons and nuclei has yet to be established, but present evidence is quite insufficient to prove that it does not occur. Even in the parallel experiments with accelerated helium nuclei, for example, no trace of the phenomenon was found.

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[1] The writer is greatly indebted to Mr. Dee for permission to reproduce this photograph, and also Fig. 5, Plate II.

At this stage, then, it might be imagined that all known types of disintegration of light nuclei had at last been discussed—and, if our survey did not include 1934 in its compass, this conclusion would be broadly correct. As it is, however, it is very far from the truth. For we have tacitly assumed throughout our discussions that the complex nucleus resulting from each disintegration process is a stable nucleus. We have been forced to the conclusion that in many cases it is initially formed with excess energy, but we have imagined that, when this energy has been liberated as a quantum of γ-radiation, no further spontaneous change is possible. This is manifestly true in all save perhaps one of the cases we have described (for He^3 the mass number is less than twice the atomic number—a relation pointing to instability), but recent research has shown it to be very obviously untrue of others. In these cases the nucleus formed as the result of the primary disintegration is definitely unstable. In all particulars it behaves precisely as the nucleus of a radioactive species which has hitherto remained undiscovered on the earth because of its own short lifetime and the fact that it is not a member of a radioactive series originating in a parent species of slow decay. Equations (xii) and (xiii) refer to a definite example. Curie and Joliot have shown that a radioelement of half life about 2·4 minutes is formed when magnesium is bombarded by α-particles and that electrons are emitted in the subsequent radioactive decay. We write

$$\mathrm{Mg}^{25} + \mathrm{He}^{4} = \mathrm{Al}^{28} + \mathrm{H}^{1} + \mathrm{Q}_1 \quad . \quad . \quad . \quad . \quad \text{(xii)}$$
$$\mathrm{Al}^{28} \longrightarrow \mathrm{Si}^{28} + e \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(xiii)}$$

because a product of identical behaviour is obtained when silicon and phosphorus are bombarded by neutrons, thus

$$\mathrm{Si}^{28} + n^{1} = \mathrm{Al}^{28} + \mathrm{H}^{1} + \mathrm{Q}_2 \quad . \quad . \quad . \quad . \quad . \quad \text{(xiv)}$$

and

$$\mathrm{P}^{31} + n^{1} = \mathrm{Al}^{28} + \mathrm{He}^{4} + \mathrm{Q}_3 \quad . \quad . \quad . \quad . \quad . \quad \text{(xv)}$$

—and we replace the "equals" sign in (xiii) by an arrow in order to indicate that this is not a mass-energy equation, since the electrons emitted in the disintegration possess energies distributed continuously over a wide range. Thus the old problem of the conservation of energy in radioactive β-disintegration reappears, and, although theorists are now fairly unanimous in the explanation which they offer, since that explanation involves the assumption of a particle apparently calculated to elude the keenest scrutiny of experiment (zero rest-mass and zero resultant charge), we may be excused further consideration of it in the present discussion. There is one problem, however, which we cannot avoid. The original experiments of Curie and Joliot were carried out with aluminium, a "pure" element which was known to give rise to protons and neutrons under α-particle bombardment. Curie and Joliot found, in addition, subsequent activity of an electron type and magnetic deflection experiments showed that the disintegration electrons were positively charged. Up to that time the positive electron had never been found except it had been produced, together with a negative electron, as one constituent of an electron pair; indeed any knowledge of the existence of this particle was then of recent date, the first observations having been made by Anderson in 1932. But pair-production occurred by quite another mechanism. We may write

$$\mathrm{X}^{\mathrm{A}} + \mathrm{E}_{\gamma} = \mathrm{X}^{\mathrm{A}} + e + \text{ɘ} + \mathrm{Q} \quad . \quad . \quad . \quad . \quad . \quad \text{(xvi)},$$

where $\mathrm{E}\gamma$ is the energy of a γ-ray quantum, e and ɘ represent the masses of negative and positive electrons, respectively, and Q is the sum of the kinetic energies of these particles and of the nucleus X^{A} whose mass occurs on both sides of the equation to emphasise the fact that for this process to be at all probable requires the presence of an intense electric field such as is only to be found in the neighbourhood of an atomic nucleus. In such an electric field it may also be possible for a fraction of the kinetic energy of a fast particle to be absorbed in a similar manner—in the creation of a pair of positive and negative electrons moving apart with a certain amount of energy. (Thus the experimental evidence for pair-production by fast electrons seems to be satisfactory.) But, in whatever way a positive electron is created, it has only a very brief existence. Sometimes before it has expended the whole of its kinetic energy in ionising encounters, more often shortly after it has effectively been brought to rest, it suffers annihilation by interaction with a negative electron from the material through which it has been passing. Mass-energy and energy of motion reappear as energy of radiation, either two quanta or one being emitted according to the annihilation conditions. These, then, are the peculiar properties of positive electrons as directly determined. In view of them we can only conclude, concerning the positive electrons liberated in the radioactive disintegration of artificially produced radioelements, that there can be no sense in thinking of them as existing in the nucleus prior to the act of disintegration, but rather it must be assumed that they are created at the moment of ejection. Without entering more deeply into a consideration of the matter, we may say that associated with the emission of the positive electron there occurs an intra-nuclear transition by which a proton disappears from one state and a neutron appears in another. If this be accepted, then the emission of a negative electron may be similarly regarded. A nuclear neutron disappears and a proton appears; simultaneously an electron is liberated from the nucleus. The matter may, in fact, be more complicated than this, but that will not invalidate this partial description of the process.

All this theorising, then, has been occasioned by the discovery that in certain disintegrations light nuclei may be formed which are radioactive, in that they subsequently emit electrons—either positive or negative—in transforming to a stable species. It only remains to indicate the stage to which investigations of this effect have been carried in the very short time since the first indications of its occurrence were obtained. Disintegrations resulting in the production of unstable species have now been established with α-particles, protons, neutrons and heavy hydrogen nuclei. With neutrons, the subsequent radioactivity constitutes the indirect evidence, to which we have already alluded, for the disintegration of heavy nuclei—Fermi and his co-workers having obtained positive results with nearly 50 elements, from fluorine to uranium. In all these cases it is more difficult to determine the details of the primary disintegration process than to investigate the main features of the β-particle emission which follows, but chemical tests have made it abundantly clear that at least three different types of transformation may occur. The neutron may simply be captured by the complex nucleus with the emission of radiation only, or a proton, or an α-particle may be emitted.

Thus far the experiments: we have indicated their contribution towards a new knowledge and understanding of the physics of the nucleus since January 1932. Possibly—or even probably—the next three years will witness an equal increase in knowledge, an even greater increase in understanding. But this is of the nature of speculation; only the passage of time can tell.

THE FISHERIES OF EGYPT

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LIKE other tropical and semi-tropical countries, Egypt possesses fisheries that are not to be compared with those of North-West Europe, and the supply of fish is very much less in proportion to the population. In 1927 the price of fish at the place of landing was 43 milliemes per kilogramme, but in England and Germany the prices translated into milliemes would be 18 and 12 respectively. Similarly, as we go along the Mediterranean coast from Algeria to Egypt, the price of fish increases. For instance, in 1925, the wholesale price of a kilogramme of fish calculated in shillings was ·42 in Algeria, ·76 in Tunis, and ·82 in Egypt.

In 1930 it is computed that there were 44,745 men, and 18,231 children wholly or partly engaged in fishing. These figures exceed by several thousand the comparable total for England and Wales. In the latter the fishery is a highly mechanised one, chiefly marine and roughly fifteen times as great as that of Egypt. In these circumstances, it is not surprising to find that the result of a survey in 1928 showed it unlikely that the earnings of the ordinary fisherman of Egypt exceeded 26s. a month. The poorest-paid English or Welsh fisherman on the other hand receives at least four times this amount.

Speaking broadly, the total of fish from Egyptian waters as shown by the statistics collected since the War, can be put at between 30,000 and 40,000 metric tons annually. The total yield in 1930 amounted to 44,269 metric tons, estimated to have been worth £E.1,372,401 at the place of landing. The state collects certain sums as boat licences, and from the sale of concessions, amounting in the same year to £E.87,671, roughly the equivalent of a tax of 6 per cent.

The yield just mentioned comes from three sources, the lakes, the sea and the Nile (Fig. 1). It is further differentiated in the table on page 212. The greater part, about two-thirds, is from the lakes, and of the remainder, the sea and the Nile contribute approximately equal parts. Actually, owing to the general drainage policy of the

Department of Irrigation, the Egyptian lakes are slowly being reduced either in level or extent, and the fishery, already beginning to diminish, will finally become much reduced, unless intensive culture can be arranged. On the other hand, the maritime yield has shown a welcome and considerable increase of late, mainly

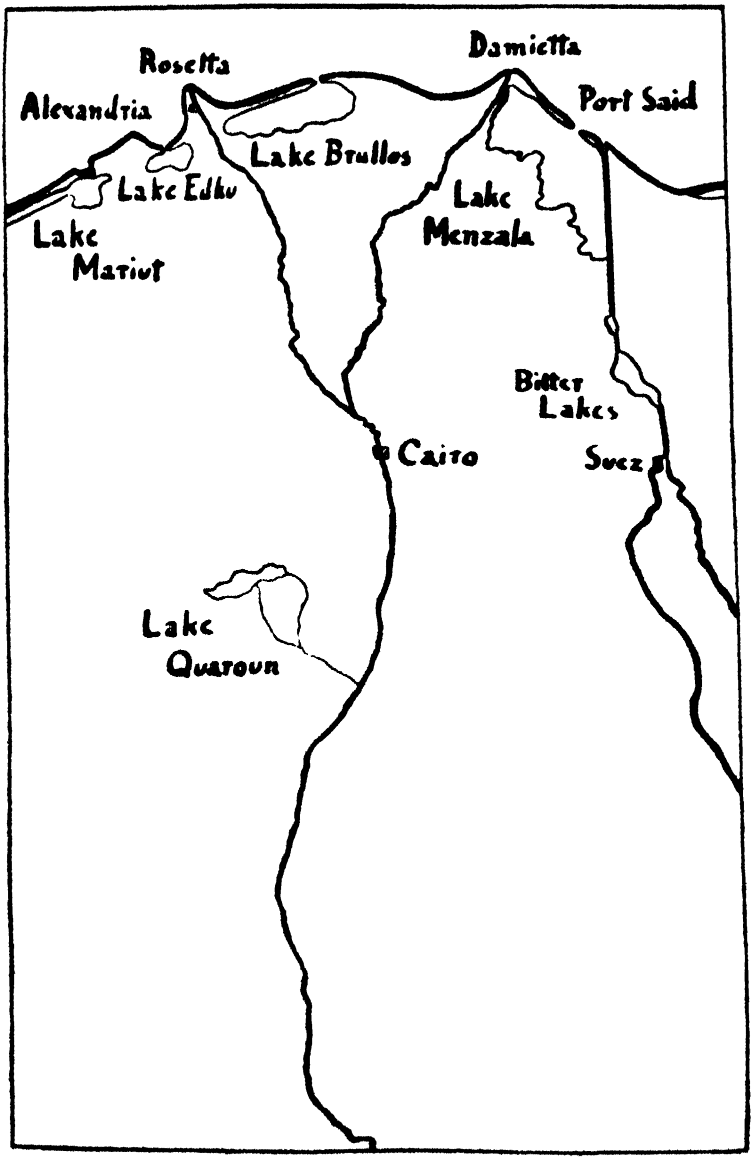


FIG. 1.—Map of Egypt showing the principal towns and lakes.
(Scale 1 in. = 60 miles approx.)

owing to the more efficient exploitation of the Alexandria, Port Said and Suez trawl fisheries by using motor-vessels and otter trawls, instead of sailing vessels and "Bull nets." The catch from the Nile does not appear to vary much, nor does it seem likely that it can be controlled in such a way as to secure an increased yield.

TOTAL YIELD OF FISH IN EGYPT DURING THE YEARS 1932, 1931, 1930, 1929 AND 1928.

	1932	1931	1930.	1929 *	1928
	metric tons	metric tons.	metric tons	metric tons	metric tons
Sea Fisheries ·					
Trawl Fishery . .	1,906	2,092	2,100	966	1,030
Sardine Fishery .	5,042	3,826	7,867	2,938 (1,392)	2,220
Shore Fishery . .	3,196	4,234	3,741	2,722 (4,877)	2,370
Total	10,144	10,152	13,708	6,626 (7,235)	5,620
Lake Fisheries .					
Menzaleh . .	11,325	9,742	12,155	11,384	15,156
Brullos . . .	3,128	3,409	4,585	4,251	3,370
Edkou . . .	3,987	5,610	7,290	3,387	7,885
Maryut . . .	958	1,003	832	1,041	1,988
Quarun	1,579	1,750	817	483	2,544
Bardaweel . .	320	360	335	—	455
Total	21,297	31,874	26,114	20,546	31,398
Nile, Canals and Drains	4,690	5,930	4,447	5,000	4,980
Total yield of Egyptian Waters . . .	36,131	37,956	44,269	32,172 (3,781)	41,998
Excess of Imports over Exports	3,213	3,511	5,178	4,885	5,120
General Total of Fish Consumed . . .	39,344	41,467	49,447	37,057 (37,666)	47,118

* The method of collecting certain statistics was altered in this year. Figures in brackets are based on the methods in use prior to this year.

Figure 2 shows the outline of a few of the more important Egyptian food fishes. The row on the right consists of marine species, the row on the left of river and fresh-water species, while that in the middle consists of those characteristic of the lakes. In each row the fish are arranged in the order of importance that they took by weight in the total local catch in 1930. The lake fish are by no means confined to the lakes, for *Tilapia zillii* occurs on the sea coast in their neighbourhood and also forms a considerable part of the Nile catch. Some of the species of *Mugil* also occur in the Nile, and all go to the sea to spawn. Most of the Nile species may be found in the lakes at certain times of the year. The marine species are more exclusive, and of those figured, only the "Loot" (*Sciæna aquila*) invades the more brackish parts of the lakes.

In Fig. 3, the 1929 catch is given as a circle divided into segments corresponding to percentages by weight of all the species amounting to over 5 per cent of the total. It will be seen that the *Tilapia* species are the most important category, followed by the mullets and the marine sardines. Though in 1930 the sardines formed the most numerous category, the sardine landings in this year were exceptional, and there is little doubt that the year 1929 can be taken as typical.

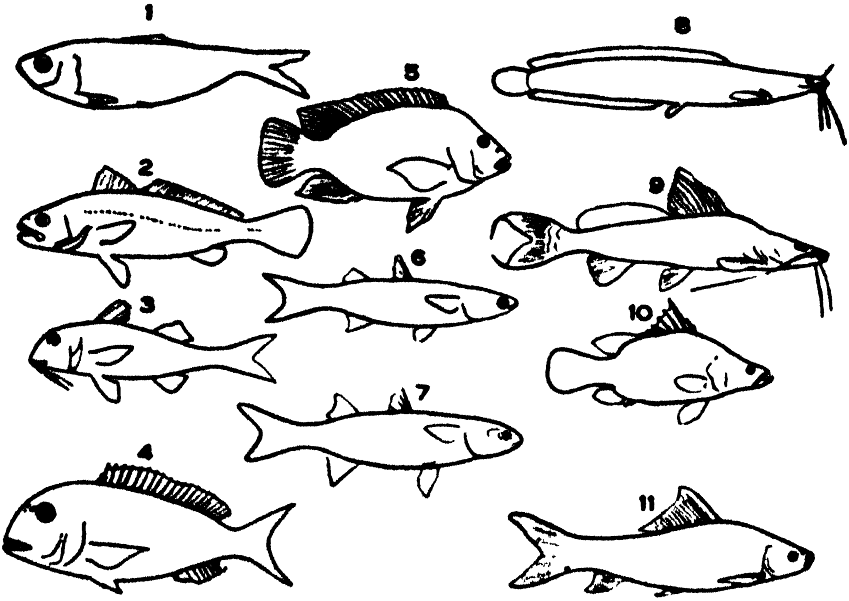


FIG. 2.—Outlines of the principal Egyptian food fishes.

1	A Sardine	*Sardinella granigera*
2	"Loot"	*Sciæna aquila*
3.	"Barbone"	*Mullus barbatus*
4	"Murgan"	*Pagrus pagrus*
5.	"Bolti"	*Tilapia zillii.*
6.	"Tobar"	*Mugil capito.*
7.	"Bouri"	*Mugil cephalus*
8	"Armoot"	*Clarias anguillaris.*
9.	"Bayad"	*Bagrus bayad*
10	"Ishr," "Hommar," "Laffa	*Lates niloticus.*
11.	"Labees"	*Labeo niloticus.*

It may be of interest to attempt a comparison of the value of Egyptian fish of other times with those of our day. About 400 B.C., when Herodotus visited Egypt, 800 *Mugil capito* ("Ad") were worth a sum which Radcliffe (*Fishing from the Earliest Times*) considers to be the equivalent of three-quarters of a gold pound. This would make the value of one fish ·225*d*. Assuming that the size of fish in ancient times was what it is to-day, there would be an average of six fish to the pound avoirdupois, and the price would therefore have been 1·35*d*. To compare with this we find that in our own time the price per pound of this species was estimated to have been 2·95*d*. in 1913, but in 1930 it was 7·00*d*.

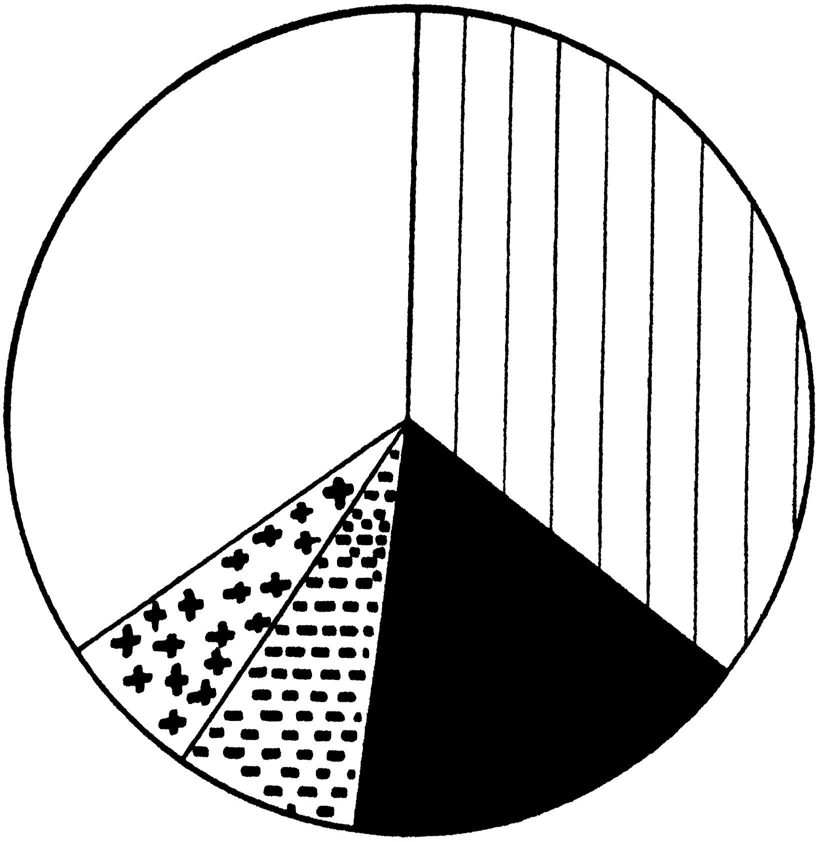


FIG. 3.—Diagram showing the percentage by weight of all categories of fish amounting to over 5 per cent in 1930, and expressed as segments of a circle.

The vertical lines represent " Bolti," black " Tobar," short dashes " Bouri," and crosses " Sardine."

It must be borne in mind that this comparison is somewhat speculative, as it is based on the assumption that the monetary unit has been correctly estimated, and that the number of fish to a pound was the same in both cases. In regard to the last assumption, it may be said that under the optimum conditions of growth of an unfished stock, it would be unlikely to obtain *Mugil capito* bigger than two to the pound. In these circumstances the value per pound in Herodotus's time would have been a third of my estimate.

FRY INTRODUCTIONS

The extensive and very shallow lakes of the Egyptian delta were once casually or permanently connected with the sea (Fig. 1). Now, the largest of them only, Lake Menzala, situated between Port Said and Rosetta, is permanently connected with the sea by the Gameel channels. Lake Brullos, in the middle of the delta, and Lake Edku, between Rosetta and Aboukir, lose their sea connections during the late summer, when their surfaces fall a little below mean sea level. The last of the big delta lakes is Lake Mariut. This lake, situated behind the town of Alexandria, is maintained at a level of about 3 metres below sea level, and acts as a huge drain-reservoir for the north-west delta farm land. A channel at sea level runs in from the sea to the lake-side at Mex. Here enormous pumps raise the water from a canal on the lake-side to that of the sea.

The Mex pumps work from August to April, during the rest of the year the evaporative power of the sun is sufficient to keep the lake at the required level. The effect of these arrangements is that there is no contact between the sea and the lake from April to August. From August to April great quantities of water are pumped out and flow down the marine canal at a considerable velocity. With this water there can, and do, pass numbers of fish and other elements of the lake's fauna. Once past the pumps, however, it is impossible for any living creature to stem the current and force its way back.

An important part of the catch in these delta lakes is formed by a certain species of grey mullet that enter from the sea as fry. Here they feed and grow, and then, at the time of maturity, crowd out through the narrow channel communicating with the sea in immense shoals. On the sea coast they spawn, and give rise to larvæ that shortly return to the lakes.

Prior to 1920 grey mullet were virtually absent from Lake Mariut, as it was impossible for them to achieve their life-cycle in relation to it. In that year Mr. G. Paget, then Director of Fisheries Research, noticing that hundreds of thousands of mullet fry could be seen stemming the pumps outflow in the Mex marine channel, organised a service to transport the fry to the lake. The fry breasting the current, concentrated near the walls of the channel and a long-handled net having a rectangular mouth and a mosquito netting-bag was dragged and pushed along the walls in the direction of the sea. The numbers of the fry so obtained were estimated by marks on the bag of the net. They were then carried over to the other side of the pumps in buckets, emptied into boats with perforated zinc removable sides, the boats towed out some way into the lake and the fry released.

This introduction made at a cost of a few hundreds of pounds resulted in a fishery for two species of grey mullet (*Mugil capito* and *M. cephalus*) running into tens of thousands each year.

A review of the amounts of fry introduced to this lake during the last ten years and compared with the annual yield, suggests that these amounts have been in excess of the needs of the lake for the yields obtained in the case of *M. capito*.

The mullet stock in Lake Edku is reinforced by the introduction of fry taken from an out-flowing drain near Aboukir.

Mullet fry are also introduced into Lake Menzala. Here, the fry congregate on the Nile side of the El Ratama channel in the winter and spring. At these seasons the Nile is low, and the water in the cut flows gently from the lake to the Nile.

About 200 kilometres south of Alexandria, there lies at the northern edge of the Fayum oasis a large crescentic lake, called Lake Quaroun. Fresh drain water from Fayum province runs into its southern and eastern edges. Nevertheless, the lake is much salter than the delta lakes, particularly towards its northern and eastern limits. It is also much deeper, giving a sounding of 8 to 9 metres in one part. The salinity is such that towards the northern shore live cockles may be found on the bottom. High yields of the " Bolty " (*Tilapia zillii* and *T. nilotica*) have been taken from this lake. These fish are chiefly confined to certain shallow parts, while in the deeper holes there is a small fishery for the predatory " Laffash " or Nile Perch (*Lates niloticus*). With the bulk of the fishery composed of two species, experience has shown its fluctuations to have been enormous.

In 1928, therefore, experiments were made to see if the fry of the larger of the two species of grey mullet (*M. cephalus*) would live in the water taken from the southern shore of this lake. The experiment was successful. Sent in insulated fish kettles by rail from Mex, 50,000 fry of this species were introduced into the lake during the autumn season of 1928.

The purpose of introducing this species was to attempt the damping out of the big fluctuations formerly shown by the lake, and also, by adding a specialised vegetarian feeder of good growth rate and different habits to the indigenous species, to attempt to increase the total yield. The experiment had another interesting purpose. The actual spawning-grounds of the grey mullet are not known, but when mature they leave the delta lakes for the immediate coastal belt, from which area their fry return to the lakes shortly afterwards. It has been said by certain fishermen that grey mullet could spawn in the lakes. Now, though where the fry were introduced near the outflowing drains, the water was rather fresh, in the rest of the lake and particularly towards the northern shore the salinity was considerably higher. It was thought possible, therefore, that when the fish became mature in two years' time, they might find salinity conditions within the lake similar to their marine breeding-grounds, and a successful spawning might result. As a female grey mullet gives rise to about one million eggs, a few successful spawners each year would ensure a continuous stocking of the lake achieved without the necessity of annual introductions, as is the case at Mex.

The mullet introduced in 1928 thrived and, as early as June 1929, fish of catchable size were seen leaping out of the water in their characteristic way. It was decided not to introduce any further fry until it would be obvious whether or no those first introduced had produced a brood of their own. This would have been shown by the occurrence of small fish of marketable size in the summer of 1931. Ripe male fish were caught in 1930, and females of that year showed well-filled almost ripe gonads, but no fresh generation appeared in 1931, and routine annual introductions were started in the autumn of that year.

During the intervening years experiments designed to solve the difficult problem of transporting the fry from the sea coast to this distant and rather inaccessible inland lake were carried on. It was found that only very small numbers could be carried in the insulated fish kettles, otherwise the mortality was heavy on the eighteen-hours railway journey. By kind permission of the Royal Air Force, H.Q. Middle East, one or two useful experiments were carried out using large troop-carrying aeroplanes. Various containers were tried, some with and some without oxygen. The mortality was much reduced by these quick transmissions, and the results were considered very satisfactory. The most successful experiment, however, was one in which 100,000 fry were carried in a railway tank wagon with a very low mortality, over 70 per cent arriving at the lake alive and strong.

While the aeroplane work showed that 50,000 live fry could be introduced to the lake at a cost of £E.60–90 per time, at least 70,000 fry could be conveyed by rail at from £E.15–20. The heavy land aeroplane was, therefore, hardly as efficient as the railway at one-third to one-sixth of the aeroplane's operating costs. This of course does not mean that a flying-boat or seaplane with a built-in tank may not yet prove to be the ideal means of transport.

The result of these trials was that when the routine introductions were begun rail transport was adopted.

The 50,000 fry introduced in 1928 gave the following yield to the fishery :

1929	181 kg.
1930	427 „
1931	52 „

FISH FARMS

The delta lakes that have been mentioned probably give a greater yield per unit of area than any other uncultivated fishery in the world. The following list gives some idea of how their fertility compares with certain other fisheries in yield per kilogramme per hectare. The Egyptian lake figures are based on a nine year mean :

Lake Monzala	64·0	North Sea	17·3
„ Brullos	60·3	Commachio Lagoon	19·0
„ Edku	279·3	Bardaweel	4·57
„ Mariut	141·1	Cultivated Carp ponds in Germany	65·5–165
„ Quaroun	95·8	Etang de Berre Marseilles	9·6

In spite of this great fertility, with the exception of Lakes Edku and Quaroun there is no area that it would not pay to reclaim in the interests of agriculture. In normal times a feddan of Egyptian delta land will yield about £E.10, when under cotton cultivation, and this figure is not likely to be approached save by Lakes Edku and Quaroun. Though the yield per unit area of Lake Quaroun is much less than Lake Edku, the stock of fish upon which it depends is supported by a relatively small shallow area along its southern shore.

There is, therefore, every prospect of the ultimate reclamation of those areas from which the bulk of Egypt's fish supply is drawn. This reclamation is indeed going on already. For this reason the question of mitigating an anticipated decrement, and if possible of maintaining the present total annual yield is one of vital importance for the country. One of the ways by which the question has been tackled, was by studying the possibility of lake fish culture. For this purpose two fish farms were established in 1923. One was a station of 17 ponds of varying sizes, dug over brackish springs on the north-west shore of Lake Mariut at Mex. Fresh water from the lake was pumped into the system by a small pump and it was intended to make experiments in ponds of different concentrations of stock and salinity. The other farm was a chain of nine ponds of small size linked by sluices with gauze windows and supplied with an inflow from the Darraga canal and an outflow to the Minufia canal at the Barrage, Cairo. Here, it was proposed to work on fresh-water fish.

Experiments were hardly begun at these farms when the Director of Fisheries Research, who had been responsible for their inception, retired, and no further work of fisheries development was undertaken until the present writer took up the direction of research at the end of 1927. By this time both places had become derelict. At Mex the pumping system had been removed and, at both farms, ponds and channels had become silted and choked. Schemes for their rehabilitation were put forward, but were only partially achieved and then only in the case of the Barrage station. In spite of this a certain amount of work that proved to be of unusual interest was accomplished.

In 1930 and 1931 at Mex, a certain number of mullet fry (mainly *M. cephalus*) were introduced and at the end of the year interesting censuses were made. These showed that though some of the *M. cephalus* individuals had made good, another species, *M. auratus*, which did not appear in the rest of the lake as a result of similar fry introductions, had appeared in considerable numbers in the very brackish pond into which they had been introduced. In this same pond it was found that another species of mullet, *M. capito*, generally failed to survive introduction by twenty-four hours.

A consideration of the communities of fish in the Egyptian lakes had a considerable influence on the planning of experiments at the Barrage farm. It appears that the lake areas richest in yield are those with the smallest numbers of important species. (See Report on the Fisheries of Egypt, 1930.)

The same thing seems to be the case in the sea trawling areas of North-West Europe. For these reasons populations containing different numbers of species were established in this farm. The ponds were drained in the winter of 1930–31 and a census was made.

This analysis showed that the greatest weight per unit area was obtained from the pond containing the largest proportion of one species, and that ponds decreased in yield as the population became more heterogeneous. In the case of ponds in which a predatory or carnivorous species like the Nile perch, was a considerable element, this generalisation did not hold and the yield was nothing like so great as where an omnivorous carp or a vegetarian mullet were in like proportion. The proportion of the species in some of the ponds are shown in Fig. 4. Above the plan are circles whose areas are directly proportional to the weight per unit area of the pond they represent and their segments are divided in proportion to the number of species present. The circles below the plan deal similarly with the numbers of individuals of the different species.

Some useful direct observations on growth rate and mortality were also obtained from these ponds. In one pond, in which 120 Nile carp of from 10 to 20 centimetres long had been introduced in May 1929 and from which individuals 30 centimetres long were taken at the end of the same year, 6 fish varying from 39 to 48 centimetres and averaging roughly 1 kilogramme in weight, were taken. A neighbouring pond had 41 of the same species from 10 to 20 centimetres long introduced in May 1930. They were marked through the caudal peduncle with a numbered disc at the time of introduction. At the end of the year 16 of them were recovered, their mean length being in the neighbourhood of 30 centimetres. Most of them had lost their marks, but bore evidence of them in a deformity of their tails. Only 2 carried the mark. One remained at 15 centimetres (its length at introduction) and the other had grown from 19 to 33 centimetres. The carp in this pond had given rise to a brood of 224 small ones, measuring from 8 to 15 centimetres long at the end of the year.

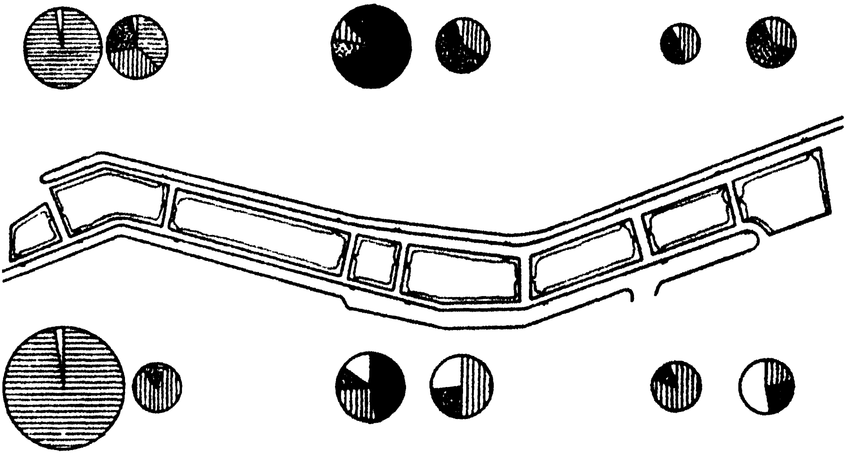


Fig. 4.—Plan of the Barrage fish farm, Cairo.

Above the plan are circles showing the proportion of the weights of the different species per unit area of the corresponding ponds, and below numbers are treated the same way as weights Horizontal lines indicate "*Labees*," vertical lines "*Bolti*," dashes "*Bouri*," dots "*Laffash*," and all black "*Tobar*."

Of these two stocks of carp, the natural mortality of the first was 95 per cent over eighteen months, and of the second 61 per cent for six months. In another small pond, 500 small grey mullet (*M. capito*) of about 2 centimetres in length were introduced towards the end of March 1930. These gave a population of 34 at the end of the year. The natural mortality in this case was 93·2 per cent. A point of especial interest about this introduction was that while in the delta lakes the introduced species seldom reached 20 centimetres in length as a result of its first year's growth and was, as a rule, 14 to 16 centimetres long, the lengths in the first winter in this pond near Cairo lay between 25 and 28 centimetres.

As far as it has gone this work shows the desirability of introducing *M. capito* into the fresh-water areas of the delta, rather than the brackish ones. The advantage of regulating community stocks by reducing the number of species present, and the possible undesirability of the Nile perch are other conclusions that seem to follow.

FLUCTUATIONS

The annual catch of the Egyptian fisheries has varied between twenty-seven and forty-nine thousand metric tons. The cause of these annual fluctuations, as is the case with most of the country's other activities, seems likely to be related to the volume and dispersion of the Nile flood. The annual flood of the Nile takes place in August, reaching its peak in September, but varying in intensity in different years. A study of the yield of Lake Menzala, from which we have figures going back a long period, and which come from an area that may be regarded as representative of the rest of the fisheries, may be compared profitably with the annual floods.

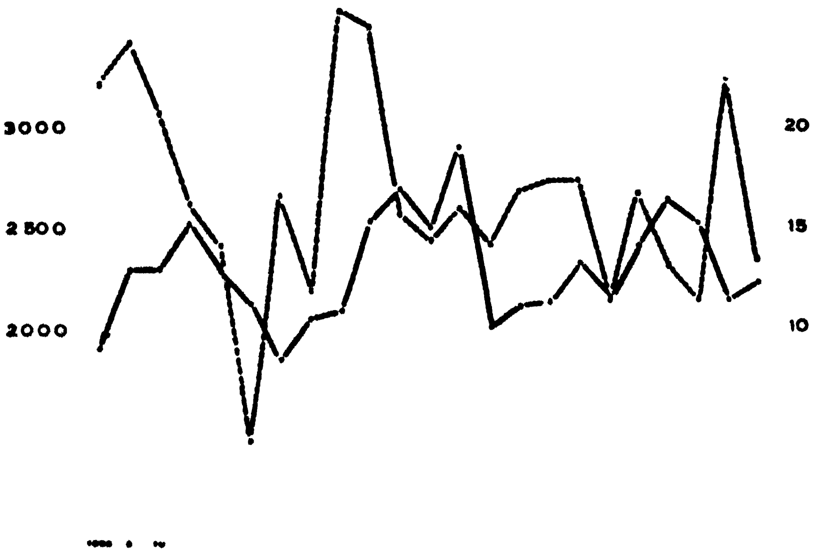


FIG. 5.—Graph showing the relation between the annual mean discharges in cubic metres per second through Aswan sluices, and the annual fishery yield of Lake Menzala for the years 1908–30 in thousands of metric tons.

The values of the Aswan discharges are joined by a broken line, and those of the Menzala yield by a continuous one.

The comparison shows that exceptionally high floods have been followed either in the next year or within two years by an increase in the yield. So obvious was this effect, that on the occurrence of an abnormally high flood in 1929 the Fishery Report of that year confidently anticipated a resulting increase in the catch. It is gratifying to note that this anticipation was borne out by the returns of the subsequent year.

The relation of the stimulus applied by the Nile flood to the various food fishes is, nevertheless, not an immediately clear and directly predictable thing. Much work is necessary upon the qualitative composition of the water, upon the contained plankton, algæ and vegetation and upon the rate of growth of the fishes before this relation can be made clear.

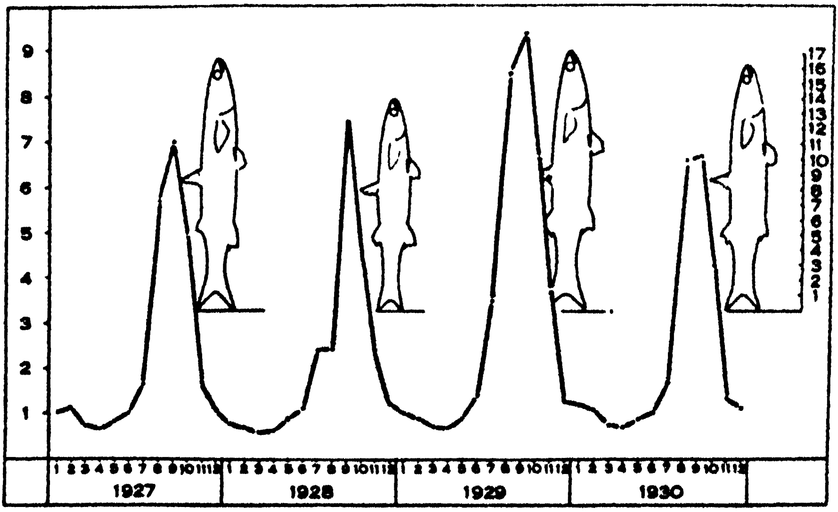


FIG. 6.—The monthly mean discharges of the Aswan sluices in thousands of cubic metres per second between 1927 and 1930, compared with the mean calculated lengths of "I" group mullet at the ends of these years.

Some attention has been paid to the rate of growth, weight caught, and number introduced, compared with the Nile flood and lake level, in respect of the small grey mullet of Lake Mariut. A comparison of the monthly mean flood discharges for the different years, and the mean size at one year old of the commonest age group is shown in the figure above.

It will be seen that the rate of growth has varied directly with the Nile flood. Nevertheless, yield has been falling during this time and for a matter of ten years there is little correspondence between the number of millions of fry introduced into the lake and the catch. The numbers surviving have evidently been thinned out in the last few years by a falling lake level, and, moreover, this thinning out must have been in the very early life of the fish, for the diminishing lake level has left no mark on the growth rate to one year of age. It seems probable that the reduction in numbers brought about by a lower lake level is due to the extinction of some littoral plankton community upon which the very young fish rely. However, to elucidate this matter a plankton study will be necessary.

The fluctuations in yield are of a different order in the different areas and fisheries. In the case of the lakes, there is a very clear inverse relation between the fluctuation in yield from the mean for the nine years prior to 1928 and the number of species figuring in the market returns. The following list shows the extent of this :

Lake.	Per cent Greatest Fluctuations	Number of Species in Returns
Quaroun	202	3
Edku	142	11
Mariut	128	9
Menzala	50	21
Brullos	46	21

It would appear from this that investigations of an ecological nature into the proportions and characters of the species making up the community of a fishery are likely to have wide economic implications. I am not aware that this aspect of fishery investigation has yet been referred to, though it is well known that in a stock of fish of one species, a stock consisting of a small number of age groups is liable to greater fluctuations than one with a larger number. Equally obvious with the fact that areas with a smaller number of species are liable to enormous and disconcerting fluctuations is the fact that these same areas are richer in yield per unit area over a period of years. This is well shown by comparing the table above with the yields given at the beginning of the section on " Fish Farms."

THE SEA FISHERY

Prior to 1928 the Sea Fisheries could have been put down in round figures as 5,500 metric tons, of which 1,500 were from the trawl, and 2,000 each from the sardine and shore fisheries. Now, however, it is increasing on account of the expansion of the trawl fishery.

Up to 1928 the trawl fishery had been made up of Italian sailing vessels of 50 to 60 feet in length and furnished with one mast and a triangular sail (" *Tartones* "), or with two masts and lugsails (" *Bragozzis* "). These vessels fished in pairs, each drawing a line connected to the wing of a single trawl or "Bull net." Fleets of about forty of them worked out of Alexandria and Port Said and in addition there would be a small number registered at Suez. The Suez fishery was, however, usually pursued by vessels from Port Said.

Between 1928 and 1930, however, the Tartones were fitted with heavy oil motors and fished a single otter trawl. Other sailing vessels were withdrawn, and motor schooners working an otter trawl substituted. In 1930 the result of this mechanisation was to give Alexandria 25 trawls operated by motor ships in addition to 5 used by sailing vessels. At Port Said 5 out of 22, and at Suez 1 out of 5 trawls were motor-operated. The trawling area is therefore being considerably extended, catches are rising and are expected to increase until the whole fleet is fitted with motors

The Egyptian trawl fishery shows an interesting but unexpectedly greater catch per net per day's fishing on the Gulf of Suez grounds compared with those of the Mediterranean. The mean figures in kilogrammes for a ten-year period at Alexandria and Suez, and for a nine-year one at Port Said are as follows:

Alexandria	115
Port Said	131
Suez	232

Comparable figures concerning long lining, available from an experimental motor ketch, point to a greater weight per unit time being taken in the Gulf of Suez than off Alexandria.

It has also been shown that there is more organic matter in the water of the southern part of the Suez Canal than in the northern. There is, therefore, little doubt that the Gulf of Suez area is the most fertile and productive Egyptian sea area, though this is not to say that its products are the most valuable and palatable.

The sea fisheries as a whole do not fluctuate to the extent that some of the lakes do. The most important contributor to fluctuations in this fishery is the "Sardine" (*Sardinella granigera* and *S. aurita*). This catch may vary from one to seven thousand metric tons, the greater part of which is usually fished by special boats from Port Said and Rosetta. In 1930, 360 boats engaged in this fishery were registered at Rosetta and 235 at Port Said. This was a year of especial success in the fishery, over six of the seven thousand tons catch being landed at Rosetta and the neighbouring ports. Particulars of the positions of the successful catches showed that they took place in an area with centres 10 miles N.N.E. to 15 miles E.N.E. This is a little on the "down-stream" side of the sea opposite the western mouth of the Nile at Rosetta (Fig. 7).

In some years the fishery occurs off the eastern mouth of the Nile near Damietta. On these occasions, however, it is noticed that the landings are less than when the fishery occurs at the west side of the delta, and it is thought that this is connected with the fact that a greater volume of water leaves the Nile through its western mouth. The connection between the Nile outflow and the sardine fishery is shown in another way. The flow of water through the Nile's two mouths is entirely stopped by dams erected in February at Edfina and Fariskur. When the Nile flood rises in August these barrages are cut, and the Nile water allowed to flow out to sea. It is not until the flood water reaches the sea that this fishery begins. In some years the eastern dam is broken before the western, or vice versa.

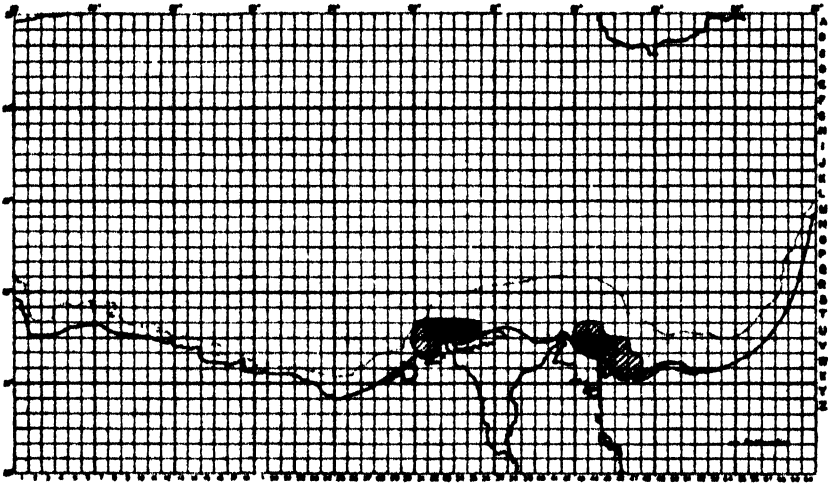


FIG. 7.—Map of the Egyptian coastal region divided into ten-minute squares.

The contoured area on the west side of the delta indicates the Rosetta Sardine fishery, and that on the east side the Port Said trawl fishery in 1930. In both cases more than a thousand trips per square is shown by the black area, more than 500 by the dotted, and more than 50 by the oblique-lined.

At these times it has been noticed that the branch of the river that commences to flow much before the other is the one off which the greater part of the catch will be taken. The practical incidence of these observations is that it would be desirable to cut the western dam a week or so before the eastern, in order to have the bigger western fishery.

In 1930 the sardine fishery was so good that, taking all the Egyptian fisheries catch, the category "Sardine" amounted to 30 per cent or more than the "Bolty" which is usually commonest.

Preliminary work with a small motor ketch suggests that a profitable long line fishery for this type of craft could be worked out of Alexandria. This fishery would probably take place over the rough bottom on the sponge beds west of Alexandria. Similar work carried out with the Danish seine net, a gear from which others working in tropical and semi-tropical seas had expected much, was extremely disappointing both in the Mediterranean and the Red Sea.

These remarks may be summarised by saying that there is every reason to expect that the application of the motor engine to the trawl, sardine and long line fisheries are likely to lead to an increase of the landings of sea fish, the limits of which it is difficult to foresee. But one thing can certainly be foreseen, and that is that unless this expansion of fishing power is intelligently controlled the increase will be followed by a decrease, particularly among the trawl fish. In the event of a big increase in the number of trawlers it will be in the highest degree necessary to watch the effect of using the present type of cod-end on the trawls. This part of the net in the Italian gear used at Alexandria is so fine that no fish, however small, can pass through it. It seems certain that in order to obtain a high and stable optimum yield a very much larger minimum size of mesh will have to be enforced by statute.

RESEARCH

The administration of what is probably the most closely controlled licensed fishery in the world and that of certain concessions are in the hands of the Egyptian Coastguards and Fisheries Service. In 1919 this service, conscious of the need of research and expert advice in its efforts to conserve the fisheries, and to suggest suitable laws, secured the services of a Director of Fisheries Research. The valuable work of fry introduction and the fish farms initiated by this official have already been referred to. In addition, by using the nation-wide organisation of the Coastguards and Fisheries Service, fishery statistics were collected for the first time, and an annual report was issued. In 1923 the Director of Fisheries Research retired and fisheries research was suspended on the grounds of economy. However, foundations of great value had been laid, and the annual report containing the statistics that had been initiated, was maintained.

At the end of 1927, the present writer was invited to take charge of fisheries research in Egypt, on a short contract. During this time the lines of a programme directed mainly to marine investigations were laid down. In pursuance with this policy a research steamer was built and completed in 1930. This vessel was built on the lines of a large trawler, was fitted with a big winch, together with other special gear for the fullest attack on fishery and oceanographic problems (Fig. 8). A small motor ketch was also

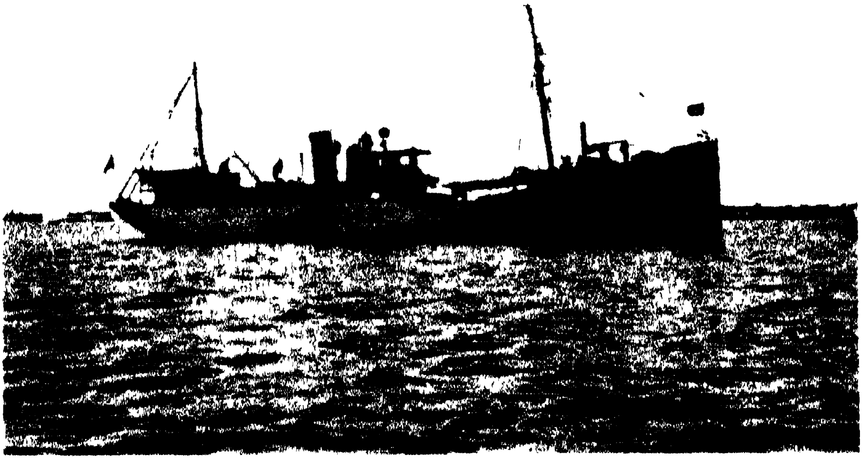


Fig. 8.—H.E.M. Research Ship *Mabahiss*.

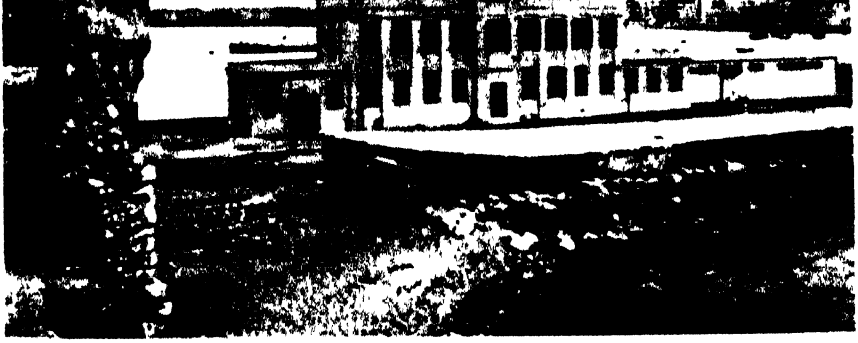


Fig. 9.—The Marine Fisheries Laboratory, Alexandria.

purchased and efficiency experiments on various types of fishing gear used in North-West Europe were initiated by her. Finally, on an arm of land forming the western border of Alexandria Eastern Harbour, a Marine Fisheries Laboratory was built (Fig. 9). The Marine Fisheries Laboratory is a rectangular three-storey building of ferro-concrete, embellished in the Neo-Pharoanic style. On the ground floor are an aquarium of several tanks, pump-room, laboratory attendant's room, chemical store, dark-room, bath-room, and four working cubicles for the investigators. The first floor contains the Director's room, a clerks' office and a large general laboratory. On the top floor are the library and museum. In the working cubicles and the general laboratory, sea water is laid on to the sinks, as well as tap water. The sea-water supply comes from a ten-ton tank in the roof and the waste from the tanks, etc. is designed to drain through a filter into either a 70- or 30-ton settling tank under the floor, according to which one is in use at the time. A small hatchery is also attached to the Laboratory.

During this period Egyptian students were undergoing suitable instruction in Europe, and Europeans were engaged temporarily in order to provide a staff of four workers, to carry out a planned marine investigation. Unfortunately, this programme was interrupted before the end of the preparatory stage by the financial crisis in 1931.

At present the Fisheries Research Directorate of the Coastguards and Fisheries Service Marine Laboratory consists of two Egyptians who have recently returned from training in England and France, and one who has had the additional experience of working on the *Mabahiss* during the John Murray Expedition to the Indian Ocean. To this latter official, Dr. Hussein Fawzi Effendi, L.-ès-Sc., Paris, has been confided the general direction of research work.

TASTE AND CHEMICAL CONSTITUTION

By A. J. MEE, M.A., B.Sc.

The chemistry of taste is a subject still in its infancy. Although many observations have been made on the tastes of substances, we are still very far from possessing a theory which will correlate these observations with the chemical constitution of the substance. It is clear that taste must be a chemical sense, and must be conditioned, in some way not yet known, by chemical constitution. It is the purpose of this article to give a summary of the theories that have been put forward up to the present.

Considered from the physiological point of view, the sense of taste has much less capacity for discrimination than the sense of sight. Whilst colours are more or less easily recognised by the majority of persons (with the exception of the colour-blind), there is frequently a considerable difference with regard to the discrimination of taste. What is a sweet substance to one, may be bitter, or insipid to another. The phenomenon of "taste-blindness" is now being more fully investigated, and some of the results that have been obtained will be briefly referred to later. Although it is sometimes difficult to discriminate between tastes, it is even more difficult to discriminate between degrees of sweetness and sourness. In taste there are only five general classes—sweet, bitter, salt, sour, and insipid—into which substances may be placed ; and it may be pointed out that these correspond rather to light and darkness in the case of sight, than to colour perception. Degrees of sweetness and sourness are only perceived with difficulty, probably because the sense of taste is due to the stimulation of a whole host of taste-buds, and may therefore be regarded as an "average" effect. There is the possibility that some may be more sensitive than others, as indeed has been recently shown. In the case of sight, the perceptive organ may be regarded as a single whole, and, consequently, light discrimination may be much more delicately accomplished. Of course, it may be remarked that even with the eye, it is very difficult to estimate degrees of brightness with accuracy.

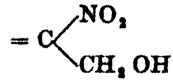
The fact that acids taste sour has long been known. Possibly one of the earliest of chemical processes was the fermentation of sugar, and the subsequent oxidation of the alcohol produced, to form vinegar. If an acid is completely dissociated into its ions in aqueous solution, which is true for strong acids, its taste must be due to that of the two ions. The limit at which hydrochloric acid still tastes sour was determined by Kahlenberg [1] in 1900 to be $N/800$. A solution of common salt of this concentration is tasteless. Hence, the chloride ion has ceased to taste at this concentration, and the taste of the acid must be due to the hydrogen ions alone. Weak acids, which, of course, provide a smaller concentration of hydrogen ions than strong acids, are less sour than strong acids at the same concentration, a fact first stated by Richards [2] in 1898. He was also able to show that the acid taste of hydrochloric acid was diminished by the addition of sodium acetate, which would, of course, lower the hydrogen ion concentration. He must have been possessed of a remarkable sense of taste, for he claimed to be able to tell the end-point in a titration of $N/10$ acid and alkali by this means. Such a method of volumetric analysis could not, however, be generally adopted, owing to the large individual differences in taste perception, to which reference has already been made, and also owing to the fact that taste, like the other senses, gets fatigued, and after a time discrimination is almost impossible. In fact, taste seems to be more easily fatigued than most of the other senses.

It would appear then, from the work already quoted, that the taste of an acid is largely due to the hydrogen ion concentration. All attempts to derive any quantitative relationship between hydrogen ion concentration and taste have, however, failed. Kahlenberg [1] investigated weak and strong acids, and found that acetic acid still possessed a distinctly acid taste in $N/200$ solutions, whilst for hydrochloric acid, the limit was $N/800$, although the hydrogen ion concentration in solutions of these concentrations was four times as great in the hydrochloric acid as in the acetic acid. With acid sodium and potassium salts, the limit was even less than this.

Solutions of sodium and potassium hydroxide, and also of sodium carbonate have characteristic tastes, which appear to be due to the hydroxyl ion; but there is little doubt that it is not the cation or anion alone that decides the taste of a substance, but the combination of the two. Thus, although an acid tastes sour, and there appears to be little or no difference between the taste of one acid and another, the statement made above that the hydroxyl group possesses a characteristic taste, indicates that the anion in some cases, if not in all, may also be a factor in deciding the taste. Höber and Kiesow [3] carried out experiments on the taste of sodium and potassium salts, and found that the anion as well as the cation was effective in deciding the taste. This indeed is only a common-sense conclusion, for otherwise all sodium salts would taste salt, since sodium chloride does.

The extent to which an anion or a cation can decide the taste of a compound depends a good deal on its nature. In the case of the chlorides of the alkali and alkaline earth metals, the cation seems to have very little influence, since the chlorides of sodium, potassium, ammonium, and calcium all taste salty. The difficulty which has yet to be solved is, To what extent is taste an additive property? The mixing of tastes is not yet understood. The most recent work on the tastes of salts has been done by Dietzel [4].

Turning now to organic compounds, we find that the field is much wider, although attempts have been made to solve the problem in much the same way as with inorganic compounds. A large amount of material on the taste of organic compounds has been collected; the difficulty is to reduce it to order. It is well known that many of the carbohydrates, such as glucose, sucrose, etc., are sweet. Nef first pointed out that these compounds contained, in common with other sweet substances, the group $(CHOH)_n$, which seemed to have the power of conferring sweetness on a substance. The work of Emil Fischer [5] on the amino-acids, showed that many of the α-amino-acids have a sweet taste. L. Henry [6] showed that bitterness was frequently associated with the group



$$=C\begin{cases} NO_2 \\ CH_2OH \end{cases}$$

and the nitro-group often seems to give bitterness to substances. In 1914, all the available material was collected by G. Cohn in his book *Die Organischen Geschmackstoffe.* He attempted to show that there was a connection between taste and chemical constitution, in so far as taste appeared to be dependent upon the existence of certain groups in the molecule, such as hydroxyl and amino groups. These taste-producing groups he called "sapophoric" groups. He pointed out that they occurred very frequently in pairs. A remarkable fact brought to light by Cohn was that sweet and bitter substances are often very closely related chemically. The lower members of an homologous series may be sweet, and the higher members sour, there being a continuous gradation of taste between the two. He also showed that the taste of a substance may frequently be complicated by the existence of an "after-taste." A sweet substance may leave behind it a bitter after-taste. Cohn was unable to arrive at any theory by means of which the taste of a substance could be predicted from its chemical formula.

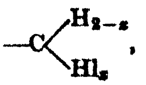
The first attempt to do this was carried out by Oertly and Myers [7], who dealt with the sweetness of aliphatic compounds. The comparative success of the theory of the relationship between colour and chemical constitution, developed by Witt and others [8], led them to work on a similar basis for taste and chemical constitution. It will be remembered that in Witt's theory, the dyeing properties of a substance are supposed to be due to the presence in the molecule of two groups, the first, a chromophore, which is characterised by the presence of a double bond, and the second an auxochrome. The substance containing the chromophoric group is called a chromogen, and this molecule itself is frequently coloured. It will not, however, dye, until one or more auxochromes are introduced into the molecule.

Similar relationships were sought by Oertly and Myers amongst substances possessing a taste. Cohn had already pointed out that his "sapophoric" groups often occurred in pairs, and Oertly and Myers found that two factors were necessary for the production of taste in an organic compound. By analogy with the terms used in Witt's colour theory, they called these two groups the *glucophore* and the *auxogluc*. Any glucophore will give a sweet compound with an auxogluc. To find out whether a substance will be sweet or not from an inspection of its chemical formula, it is necessary first to see whether it contains a glucophore; if it does not, it may be said at once that the compound is not sweet. If, however, it does contain a glucophore, it may or may not be sweet. To be sweet it must also contain an auxogluc. If it contains an acid radical, it will be sour, or possibly bitter.

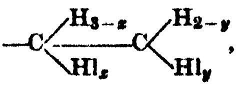
The chief glucophores were

$—CO—CHOH—(H)$,[1] $—CH_2OH.CHOH—$, $—COOH.CHNH_2—$,

$—CH_2O.NO_2$,



$—C\begin{smallmatrix} H_{2-x} \\ Hl_x \end{smallmatrix}$, and



$—C\begin{smallmatrix} H_{3-x} \\ Hl_x \end{smallmatrix}—C\begin{smallmatrix} H_{2-y} \\ Hl_y \end{smallmatrix}$,

where Hl stands for a halogen atom. The auxoglucs were H, the radicals of saturated hydrocarbons, C_nH_{2n+1}, where *n* could be 1, 2, or 3, the radicals $C_nH_{2n+1}O$, of monohydric alcohols, *n* being 1 or 2, and the radicals $C_nH_{2n-1}O_n$ of polyhydric alcohols.

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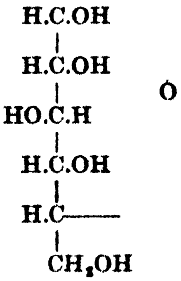
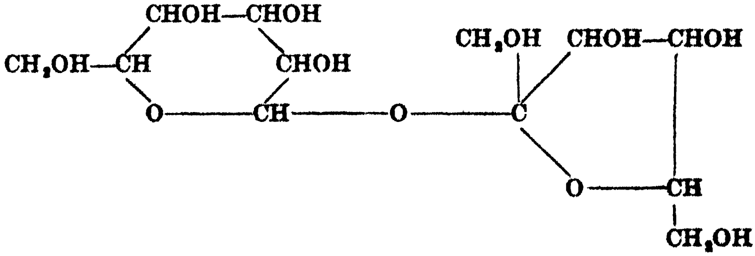
[1] The —CO—CHOH— grouping must be combined with at least one hydrogen atom to become a glucophore. This is indicated by putting the H atom in brackets.

It must be emphasised that the auxogluc must be present in addition to the glucophores. Thus, nearly every organic compound contains hydrogen, but, unless it contains hydrogen in addition to the glucophore (which may itself embrace all the hydrogen in the compound), it will not be sweet.

To take a few examples, we may consider the typically sweet compounds glycol, glycerol, glucose, sucrose, and saccharin. Glycol has the composition $CH_2OH.CH_2OH$. It contains the glucophore $CH_2OH.CHOH.$—, and is the most simple compound in which this glucophore occurs. This would not, of necessity, make it sweet, but it contains the auxogluc, hydrogen, in addition, and hence is sweet. Glycerol contains the same glucophore, for its structural formula is $CH_2OH.CHOH.CH_2OH$. It also contains the auxogluc CH_2OH, the radical of a monohydric alcohol already referred to, in which *n* is 1. Glucose has the structural formula



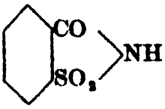
It therefore contains the group —CO—CHOH—, which is a glucophore, and also the group CH_2OH. To a certain extent the degree of sweetness of a compound seems to depend upon the number of glucophores or auxoglucs in it. Thus, sucrose, of which the constitution is



contains more CHOH groups than glucose, and is considerably sweeter. It is seen that it contains three of the necessary auxogluc groups, CH_2OH. It is a peculiar fact that the higher carbohydrates, such as starch, and cellulose, lose their sweetening power.

This may be due to stereoisomeric influences, about which a little will be said later, but no theory concerning this decrease in sweetness of the polysaccharides has yet been put forward.

Saccharin, which is well known as a substitute for sugar, being 550 times as sweet as the same quantity of cane sugar, is an aromatic body, and as such did not fall under Oertly and Myers' consideration. It is the imide of *o*-sulphobenzoic acid, and has the constitution



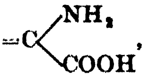
The ammonium salt is said to be even sweeter than the imide itself. As an imide, saccharin may be regarded as the anhydride of a sulphamido-acid, containing the group $C.SO_2NH_2$, but this was not among the list of glucophores or auxoglucs. No reason for the sweetness of this compound has yet been derived.

Returning to aliphatic compounds, we may consider the amino-acids, which were shown by Fischer to be sweet. α-aminobutyric acid, $CH_3.CH_2.CH(NH_2)COOH$, is sweet. It contains the glucophore $COOH.CHNH_2$, and also the auxogluc, C_2H_5. Aspartic acid, $COOH.CH_2.CH(NH_2)COOH$, contains the same glucophore, but has no auxogluc; it is instead attached to an acid radical, and therefore tastes sour.

The theory was a step forward; it certainly did explain the taste of many aliphatic organic compounds on the basis of their constitution, but it was not comprehensive enough, and had to meet numerous objections. One of these is the effect of stereoisomerism on the taste of a compound, which is not even yet explained. Many compounds are sweet in one optically active form, and insipid, or even bitter, in the other. It would therefore appear that taste is not dependent simply upon the number of groups in the molecule, and their nature, but also upon their arrangement in space. *d*-Valin, $(CH_3)_2CH.CH(NH_2)COOH$ is made up of the glucophore $—CH(NH_2)COOH$, and the auxogluc $(CH_3)_2CH—$, and is sweet, as required by the theory; but *l*-valin, containing the same groups, but differently arranged in space, is insipid. The racemic mixture is sweet. Stereoisomeric influences are also shown by the fact that α-anisaldoxime is sweet, whilst β-anisaldoxime is bitter.

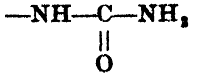
The effect of the introduction of the phenyl group, C_6H_5, is also important. Although their work did not cover the aromatic compounds, Oertly and Myers pointed out that when a phenyl group was introduced into a sweet compound, it very often reduced the sweetness, and even sometimes made it bitter. Thus, glycol is sweet; styrol, $C_6H_5.CH(OH)CH_2OH$, is slightly bitter. The bitterness of certain glucosides containing aromatic residues may perhaps be accounted for in a similar way.

No theory of bitterness seems yet to have been put forward. It might be possible to work out such a theory on similar lines to those of Oertly and Myers. A theory resembling in some degree that of these investigators was put forward later by Heiduschka and Komm [9], in an attempt to explain the taste of the α-amino-acids. They distinguished between amarogeneous and dulcigenous groups The group



found in the α-amino-acids, is a dulcigenous group.

The effect of replacement of certain elements or groups in a compound by others, on its taste, has been investigated by Fox [10]. Dulcin, *p*-ethoxyphenylcarbamide, which contains the group



$$—NH—\underset{\underset{O}{\|}}{C}—NH_2$$

has, for most people, a definitely sweet taste. If the oxygen of this group is replaced by sulphur, the substance becomes bitter. The replacement of the hydrogen of the amino-group, or the addition of other groups has no effect. This may be due to the fact that unsymmetrical compounds are formed. They have for the majority a bitter taste, and in this compound, the CO and CS groups appear to be the only ones affecting the taste. Fox showed that the bitter taste of many derivatives of phenylthiocarbamide, *s*- and *as*-diphenylthiocarbamide, and certain other derivatives of thiocarbamide is perceived only by certain individuals. Others find these compounds tasteless. This "taste-blindness" is not connected with sex, age, or race. The failure to taste these compounds may be due to their insolubility in the saliva of some persons, for it seems to be a rule that for a substance to be tasted, it must be soluble in the saliva.

It is of interest to study this question also from the physiological side. Blakeslee [11] has determined the threshold concentrations of aqueous solutions of phenylthiocarbamide at which the bitter taste is distinguishable for different individuals. It varies considerably from case to case, and sometimes is so high that a concentrated aqueous solution, and even the dry powder placed on the tongue, does not call forth the taste. The experiments were carried out with 103 families, and seem to indicate definitely that the capacity for tasting certain substances is innate, and is inherited by the offspring according to Mendel's laws. This result was also arrived at by Fox [12], from work on the taste of *p*-ethoxyphenylcarbamide already referred to, and has been verified by Snyder [13]. The deficiency is due to a single recessive gene. Taste-blindness has also been studied by Levene and Anderson [14], who have shown that the percentage of individuals who fail to taste *p*-ethoxyphenylcarbamide is much less among the American Indians than among the white population. Of course, it is necessary to be wary of making a sweeping generalisation from such few data, but it certainly seems from the recent work that taste-blindness is inherited.

The difficulties in the way of any satisfactory theory of the relationship between taste and chemical constitution have been made apparent in this review. When we have a rational standard upon which to base a scale of taste, and the personal element can be eliminated, we still have to discover the relationship between the arrangement of the groups in the molecule of the compound and its taste, before any comprehensive theory can be drawn up.

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MICROBES IN ASSOCIATION

By HUGH NICOL, Ph.D., A.I.C.

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MEDICAL and veterinary bacteriologists have in most cases supposed, when micro-organisms have been found associated in a lesion or in the production of a syndrome, that all but one of these species were more or less accidental concomitants or followers of the true, single, pathogen.

Until recently, plant pathologists have made similar assumptions to those made by animal pathologists. Erwin Smith, who may almost be considered as the founder of plant pathology, so notable have been his contributions to that science, was particularly strict in applying Koch's postulates to the study of micro-organisms pathogenic for plants.

The fact that pathogens usually occur as single species in internal lesions of animals and in many plant diseases is probably accounted for by the unadaptability of tissues of higher organisms ; these, while alive, have a positive resistance towards invasion by microbial agents. Non-living matter is subject to the most diverse forms of microbial attack, so that the bacteriology of decomposition is necessarily a study, not of the reactions of a bacterium and of a many-celled higher organism, but of a complex of micro-organisms in some sort of equilibrium amongst themselves. M. Stephenson's *Bacterial metabolism* (London, 1930) considers very few cases of bacteria grown in association. In soil bacteriology, the study of societies of two or more species of bacteria has not made much progress. In this respect, indeed, soil bacteriology is not absolutely worse off than other branches of bacteriology, but relatively it is so, since its need for the study of bacterial and fungal associations is probably greater than in medical or in industrial bacteriology.

Protozoologists unselfconsciously use the term "pure mixed culture" to denote a strain of protozoon, itself pure, grown in the society of a pure strain of bacteria, upon which the protozoa feed. A pure strain of protozoon is often grown with a mixed flora of bacteria, because it is seldom easy, or necessary for the protozoologist's purpose, to reduce the number of species of bacteria to one. In bacteriological work the need for some such term as "pure mixed culture" is beginning to be felt, but no new and satisfactory term has yet been evolved, so far as the author is aware. The awkward phrase "pure mixed culture" seems likely to continue in use among bacteriologists, who do not, of course, include protozoa in their sense and use of that term.

Medical bacteriologists have made numerous but sporadic observations on effects of bacteria in association upon and in culture media. Many of these observations relate to acid and gas production from carbohydrates, frequently by fæcal bacteria or by the commoner saprophytes. Results are for the most part merely qualitative and disparate. A good summary of such work is given in the opening pages of the third volume of Buchanan and Fulmer's *Physiology and Biochemistry of Bacteria* (London, 1930), in which numerous other associations are mentioned. To the author it does not appear that any clear result emerges from a perusal of this summary, unless it be a conviction that the various kinds and degrees of association require closer defining than that which has hitherto been granted to them. To give a physical analogy, the study of bacterial associations is somewhat in the state in which wave-physics would be if waves of all kinds were considered indiscriminately.

There are many possible types of association, from a reciprocal symbiosis to antagonism. Terms have been coined to express some of these types: commensalism, synergy, metabiosis, antibiosis, are examples, but the states they imply are loosely defined. Much work will be necessary before such terms can be used with precision, and additional nomenclature will no doubt be required in proportion to the degree of clarification attained. The degree of dependence of the members of an association may vary from that of a symbiosis to a simple commingling or mixture, though there is often metabolic exaltation due to mere association. On the other hand a depression of activity of one or both members may be observed, as in parasitism. There is no sharp line of division: symbiosis may pass into parasitism, or both states may coexist. Dr. H. G. Thornton's published (in part with Dr. Winifred Brenchley) and unpublished work performed at Rothamsted has shown that in the association of leguminous host plant and nodule bacteria, the reciprocally helpful state may pass into parasitism upon certain changes occurring in external conditions; also, symbiosis and parasitism normally coexist in each lucerne nodule that has attained a sufficient age. In the United States it has been shown that some strains of nodule bacteria are essentially parasitic. An able and philosophical review of some types of association has been made by W. L. Holman in Jordan and Falk's *Newer Knowledge of Bacteriology and Immunity* (Chicago, 1929), but that review is restricted to phenomena in medical bacteriology. Some additional associations will now be mentioned.

NATURAL AND SEMI-NATURAL ASSOCIATIONS.

Swine influenza has recently been shown to be due to the joint action of a bacterium and a virus, neither of which alone is able to reproduce the typical picture of the disease, which is interesting because it somewhat resembles human influenza. A good account of the investigations on swine influenza has been given by F. C. Minett in the *Journal of the Royal Agricultural Society* for 1933 (**94**, 232). In plants several strains of one fungus, *e.g. Botrytis cinerea*, may be found to occur simultaneously, but this simultaneous invasion is by no means a matter of course, nor, when it occurs, is it clear how the various strains act or react. A single leguminous plant is liable to be infected with at least two strains of its appropriate nodule bacteria. (D. H. Dunham and I. L. Baldwin, *Soil Sci.*, **32**, 1931, 235). Such multiple infection becomes of practical importance whenever there is antagonism between strains, and when it is desired to compete by a process of seed "inoculation" with an ineffective strain already present in the soil. This problem is being studied at Rothamsted.

In the brewing industry rigorous exclusion of infection is seldom aimed at, but ordinary working conditions are adjusted to keep the yeast free from gross contamination by unwanted organisms. British brewers do not employ pure lines of yeast, though their inoculum must be substantially a pure culture in the ordinary sense. Occasionally the yeast becomes infected with "wild" yeasts or with undesirable bacteria or both, and it then has to be rejected. A less obvious source of deterioration of yeast causes the yeast to become "weak." A "weak" yeast is often benefited by a "change of air"—most probably a change in conditions of growth. Exchanges of yeast between breweries are frequently made, and a yeast which perhaps is showing signs of weakening, often recovers its full power of fermentation when transferred to another brewery. The cause of weakness in yeast is obscure, but the condition is possibly ultimately dependent on the relative proportion of two or more strains.

In German breweries the use of pure lines of yeast which have been originated from a single cell is widespread. It has been suggested that the lack of distinctive character of similar kinds of beer brewed by different German breweries is due to the use of pure lines, and a proposal to use mixed pure lines of yeast has been made. It was hoped in this way to establish an individuality for each beer that was lost when pure lines replaced the former mixed strains native to each brewery.

Fermented milks, such as kefir, are made by the joint action of yeasts and bacteria ; the former supply the alcohol and the latter the acid. It is to the resultant mixture of flavours of alcohol and sharpness, together with esters and carbon dioxide, that the characters of kefir are due. According to S J. Thomas (*Journ. Ind. Eng. Chem.*, 8, 1916, 821) matzoon, an Armenian fermented milk, contains three active organisms : a bacillus, a streptococcus and a yeast. The lactic acid of matzoon is optically inactive, but resolvable.

Yoghurt is milk soured after partial evaporation. The joint action of several bacteria is necessary to secure the correct condition of the curd with a desirable flavour, so that a wide range of associations is possible to meet the taste of the consumer. M. Beijerinck's view of the condition of associations of bacteria responsible for the production of commercial preparations of soured milk may be expressed by saying that these associations are in unstable equilibrium, owing to their varying biotic requirements—especially as to temperature and oxygen supply. On account of this instability it is not possible to continue inoculation of fresh milk from a properly soured sample indefinitely ; after a very few transfers the balance has been so much disturbed that an undesirable product results from fermentation. Commercial quantities of yoghurt are therefore made from continually renewed mixtures of the single species, which are maintained in pure cultures in milk.

Biological filters offer interesting examples of complex natural micro-organic associations spontaneously formed on an artificial support. Sand beds have long been used as biological filters for the purification of water, while a small sectional biological filter, consisting of a vertically disposed series of cups of porous stone, was illustrated in Bradley's *New Improvements in Gardening* as long ago as 1724.

A film of bacteria and protozoa, supported on gravel or clinker, has been used to effect the oxidation of the dilute impure sucrose solutions discharged from beet sugar factories. Such an effluent, after passage through the filter, contains notable quantities of nitrate. According to Winogradsky's classical work, the nitrite- and nitrate-producing bacteria should be depressed by the presence of sugar and of the other organic matter. Whereas Winogradsky was able to obtain colony formation of the nitrifying bacteria only after exclusion of organic matter, biological production of nitrite and nitrate actively proceeds in the filter in presence of living and dead organic substance. The biological filter as applied to the purification of effluents thus affords an instructive example of the effects of a miscellaneous association and of the caution that is necessary in arguing from the results of a study of pure cultures to a consideration of natural conditions.

Oxygenation in rice fields occurs under what is apparently stagnant water, flooding of the *padi* area being requisite for growth. In view of the commercial importance of the crop, it is surprising that the biology of the rice-field has not been more fully investigated. It appears that a film forms upon the surface of the soil under a shallow sheet of water, and oxygenation proceeds in this film, which is largely composed of algæ. The water slowly percolates downwards, thus ensuring sufficient aeration of the film and of the plant roots. It is noteworthy that rice that has been transplanted makes more vigorous growth than rice allowed to pass its whole life on the spot where the seed falls. No adequate explanation of these facts is known to the author, but it may be suggested that transplanting disturbs the surface soil enough to allow the film of associating micro-organisms to develop a greater area in the immediate neighbourhood of the roots. It is possible that a further association exists between the micro-organisms and the plant roots. (Cf. C. Thom and H. Humfeld, "Notes on the Association of Micro-organisms and Roots," *Soil Sci.*, **34**, 1932, 29.)

CULTURAL ASSOCIATIONS IN THE LABORATORY

Higher micro-organisms frequently exhibit curious phenomena of interaction in the zone of contact where two species or strains are grown on one nutrient plate after inoculation at a little distance apart. As examples may be mentioned the observations on myxomycetes given by A. Quehl, referred to and extended by C. Vahle (*Centrbl. f. Bakt.*, Abt. II, **25**, 1910, 178). M. O. Reinhardt observed that when mycelia of the same fungal species (*Sclerotinia*) were paired in a culture, they grew towards each other and formed hyphal fusions quite smoothly; but when mycelia of different species of *Sclerotinia* were paired, they interfered with each other's growth to a marked extent. "Action at a distance" has been invoked in such connections. Other examples and illustrations are given by A. R. R. Buller in his book *Researches on Fungi*, Vol. V (Longmans Green, 1933). Repressive effects of association with soil saprophytes were noted by G. B. Sanford and W. C. Broadfoot

(*Sci. Agric.*, **11**, 1930–1, 512) for the "Take-All" fungus (*Ophiobolus graminis*) of wheat. Filtrates of bacteria and of fungi were sometimes nearly as potent as living cultures in suppressing the pathogenicity of *O. graminis* in soil culture. Continuing this line of work, A. W. Henry found (*Canadian Journ. of Research*, **7**, 1932, 198) that the "Take-All" fungus, inoculated into sterile soil, induced high mortality in wheat seedlings, but the incidence of the attack of the fungus upon seedlings was greatly minimised when both were grown in unsterilised soil at 27° C. There was, however, a marked temperature effect.

Probably the largest variety of groupings and of quantitative observations on associated growth of bacteria have been made in mixtures containing as one component, *Azotobacter*, the free-living, aerobic, nitrogen-fixing organism. The groupings may be considered seriatim.

(*a*) *Azotobacter* with protozoa. S. M. Nasir (*Ann. Applied Biol.*, **10**, 1923, 122) and D. W. Cutler and D. V. Bal (*Ann. Applied Biol.*, **13**, 1926, 516) have shown that in presence of protozoa, *Azotobacter* fixed more nitrogen than when grown in pure culture. The nitrogen-fixing organism in presence of protozoa remained in the colourless form and did not show the browning due to senility.

(*b*) *Azotobacter* with algæ. C. B. Lipman and L. J. H. Teakle (*Journ. Physiol.*, **7**, 1925, 509) noted a stimulation of *Azotobacter* in the presence of *Chlorella*. In 1894, P. Kossowitsch had already shown that a mixture of the alga *Nostoc* with a crude culture of soil bacteria was able to fix nitrogen. The species of the bacteria responsible for the nitrogen fixation was not determined.

(*c*) *Azotobacter* with *Azotobacter*. J. Hanzawa (*Centrbl. f. Bakt.*, Abt. II, **41**, 1914, 573) showed that two species of *Azotobacter* when grown together fixed more nitrogen than did either grown separately, but Mahmoud Selim (*Proc. Second Int. Congr. Soil Sci.*, **3**, 83, Moscow, 1932) found that a mixture of two strains fixed no more nitrogen than did an ordinary pure culture.

(*d*) *Azotobacter* with other bacteria. M. Beijerinck, who first recognized the nitrogen-fixing ability of *Azotobacter*, began by assuming that the co-operation of another bacterial species was essential for fixation to occur. Although this was shown by J. C. Lipman to be untrue, it led to numerous observations upon mixed cultures of *Azotobacter* and other bacteria. Of these may be cited J. Stoklasa (with *Radiobacter*, *Centrbl. f. Bakt.*, Abt. II, **21**, 1908, 484 and 620), D. M. Novogrudsky (with *B. Stutzeri* and *B. denitrofluorescens*, *Trans. Inst. Fertilizers*, No. 76, p. 69, Moscow, 1930), and several mixtures studied by Mahmoud Selim (reference above), An interesting case is that of activity in soil of *Clostridium pastorianum*, the anaerobic nitrogen-fixing organism. V. S. Omeliansky and M. Solunskov thought that aerobic soil organisms, including *Azotobacter*, removed oxygen and thus facilitated the existence and activity of the *Clostridium* ; at the same time they neutralised the butyric acid excreted by that anærobe. Similar explanations may, of course, apply in other cases where one member of a mixed growth uses up a constituent or by-product detrimental to another member. (An abstract of Russian papers by Omeliansky and Solunskov can be found in *Centrbl. f. Bakt.*, Abt. II, **49,** 1919, 473)

Further examples and a review of some work on *Azotobacter* are given by E. J. Russell, *Soil Conditions and Plant Growth*, London, 1932, pp. 335 *seqq.*

Effects of protozoa upon the efficiency and activity of bacteria in the production of ammonia and carbon dioxide have been investigated by D. W. Cutler and L. M. Crump (*Ann. Applied Biol.*, **16,** 1929, 472), by Jane Meiklejohn (*Ann. Applied Biol*, **17,** 1930, 614, and **19,** 1932, 584) and by L. de Telegdy-Kováts (*Ann. Applied Biol.*, **19,** 1932, 65). The activity of the bacteria was heightened by the presence of protozoa, which appeared to maintain the bacterial culture in a condition of " physiological youth."

According to C. E Marshall (*Centrbl. f. Bakt*, Abt. II, **15,** 1906, 400) who worked with associations of lactic acid bacteria the products of associated bacteria " are known " to exert the same influence as the living bacteria themselves. This idea has been followed up by S. Knudsen and A. Sørensen (*Den kongelige Veter.- og Landboh. Aarsskrift*, 1929, p. 64 ; Copenhagen ; Danish with English summary). These authors noticed that some of their strains of *Streptococcus cremoris* grew poorly in aseptic and in sterilised milk, and displayed abnormal cell forms therein. After the addition of autolysed yeast, of milk digested by moulds, and of several other unrelated substances, the abnormal strains grew normally. In ordinary dairying, foreign bacteria introduced by dirt prepare the way for the growth of the starter streptococci difficultly cultivable in sterile milk. A variability in the natural bactericidal power of milk from different cows makes itself felt upon the effects of the starter, not directly, but through and upon the growth of associated foreign micro-organisms.

The starters themselves are not, however, pure, since they contain variable numbers of the aroma-producing Betacocci. Knudsen and Sørensen found that Betacocci throve best " in symbiosis with " those streptococci which formed acid slowly, as the Betacocci could tolerate only a certain amount of acid. This case is analogous to that of *Clostridium pastoranium* in soil, mentioned above, but it is doubtful whether the term " symbiosis " should be used for either.

C. S. Pederson, W. H. Peterson, and E. B. Fred (*Journ. Biol. Chem.*, **68**, 1926, 151) stated that in pure culture bacteria may produce dextro- or lævo-lactic acid almost exclusively. If those same pure strains are grown in mixture with an organism that does not produce lactic acid, an enantiomorph that is produced may have a sign opposite to that of the enantiomorph formed in pure cultures. E. L. Tatum, W. H Peterson and E B. Fred (*Biochem Journ.*, **26,** 1932, 846) found that *Clostridium acetobutylicum* affected both dextro- and lævo-lactic acid-producing bacteria by causing them to form inactive lactic acid. Intimate contact between the lactic acid bacteria and the *Clostridium* seemed to be necessary, no effect being obtained when the species were separated by a viscose membrane. Production of *i*-lactic acid was specifically due to the presence of *Clostridium acetobutylicum* This type of association is evidently a closer one than that which obtains in the starters, or in the matzoon studied by Thomas, whose work these authors do not mention.

ACKNOWLEDGMENTS

The author has pleasure in thanking Mr. C. B. Taylor and Mr. F. E. Day, for kindly supplying information in reference to yoghurt, and brewery yeasts, respectively.

POLYMERS

By A. S. C. LAWRENCE, Ph.D.

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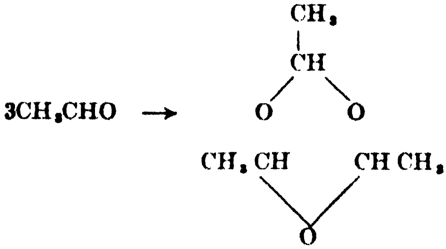
The practical importance of polymers is made obvious by the widespread use of synthetic resins. Their theoretical importance is also great since they provide new fields for the chemist, the physicist and the physical chemist. The old-fashioned idea of polymers as aggregates of two or three molecules introduced no new field since these polymers were no more than large molecules —smaller often than many single molecules. What I may call the " new polymers " are giants. The polymer-molecule may contain a hundred or more polymerised units. The manner in which these are built up, their size and shape, the manner in which they are packed in the solid state and their disposition in solution ; these are some of the problems which comprise the new field. We are building with bricks enormously larger than the chemist's ordinary bricks although the active chemical " bricks " are still the ordinary chemical groups.

I call these " new " polymers, not because they are all new, but because their recognition as such is new. It is certain that many of the polymers being examined to-day have been obtained in the past, but the workers who made them did not call them polymers. They called them something quite different and threw them down the sink. Reluctance to recognise extreme polymerisation as a regular chemical process is understandable. We know nothing yet of the peculiar reaction kinetics which can build up 100 formaldehyde molecules into a giant polymer-molecule chain of polyoxymethylene. We need not consider seriously the possibility of 100 molecules uniting simultaneously—a 100th order reaction ; but a step-by-step aggregation of molecules does not explain the ready and complete conversion of monomers to very high polymers which takes place so frequently.

The usual text-book definition of a polymer is that n molecules of a substance M form a polymer $(M)_n$, so that the percentage composition of the polymer is identical with that of the monomer. This is not only inadequate but in many cases misleading, because it is implied also that the chemical behaviour of the polymer is similar to that of the monomer. Typical examples are the polymerisation of acetylene to benzene and allylene to mesitylene. To regard benzene as a polymer of acetylene is pointless and, if the second criterion is applied, grotesque. The wider view of polymers is well illustrated by viewing a standard text-book example in a new light. Aldehydes polymerise readily. Acetaldehyde forms paraldehyde thus :



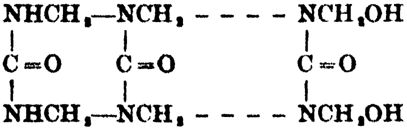
The monomer also undergoes the aldol condensation .

$$2CH_3CHO \rightarrow CH_3CH(OH)CH_2CHO.$$

But the aldol condensation need not stop here. It can go on and on :

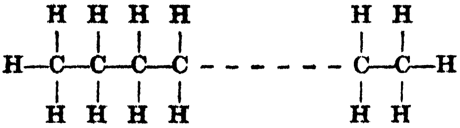
$$CH_3{\cdot}CH(OH)\,|\,CH_2CH(OH)\,|\,CH_2CH(OH)\,|\,CH_2CH(OH)\,|\,----\;CH_2CHO.$$

Now this is a real polymer in the sense that *it contains a unit repeated over and over again* with the chemically active groups at each end. The old-type polymer could never yield the huge field opened by these giant polymer-molecules, the number of which from any single monomer is very large and only limited by increasing sluggishness in reacting when they become so enormous. Further, this definition does not require the polymer to be built up from a single molecule. Condensation of two different molecules can form a polymer in which there is a regularly repeated unit of structure as in the urea-formaldehyde resins :



$$\begin{array}{lll} NHCH_2\text{—}NCH_2 & ---- & NCH_2OH \\ | \qquad\quad | & & | \\ C{=}O \quad C{=}O & & C{=}O \\ | \qquad\quad | & & | \\ NHCH_2\text{—}NCH_2 & ---- & NCH_2OH \end{array}$$

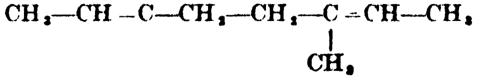
The hydrocarbons may be regarded as the simplest possible polymers.



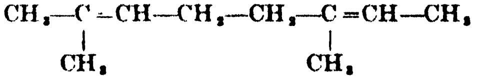
$$\begin{array}{c} H\;\;H\;\;H\;\;H \qquad\qquad H\;\;H \\ H\text{—}\overset{|}{\underset{|}{C}}\text{—}\overset{|}{\underset{|}{C}}\text{—}\overset{|}{\underset{|}{C}}\text{—}\overset{|}{\underset{|}{C}}\;------\;\overset{|}{\underset{|}{C}}\text{—}\overset{|}{\underset{|}{C}}\text{—}H \\ H\;\;H\;\;H\;\;H \qquad\qquad H\;\;H \end{array}$$

They are thus polymethylenes. Their chemical behaviour is that of the terminal groups, in this case the two hydrogens. This is not a very characteristic example of a polymer owing to lack of polarity of these terminal groups, but natural petroleum is properly regarded as a complex mixture of various-sized polymers. A process such as "cracking" is a degradation of larger polymers to smaller.

Formerly emphasis was laid on the fact that the monomer could be regenerated easily from the polymer, but this is not a general rule. Poly-oxy-ethylenes form a complex mixture of substances when heated to 300° C. The separation of a substance from a naturally-occurring polymer of large size is not necessarily the best starting-point for preparation of the polymer. Isoprene is the recurring unit in rubber, but there is no reason for saying that rubber is built up from it. Actually isoprene can polymerise in three distinct ways:

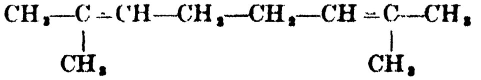
(1)


$$CH_2{-}CH{-}C{-}CH_2{-}CH_2{-}\underset{\underset{\displaystyle CH_3}{|}}{C}{=}CH{-}CH_2$$

(2)


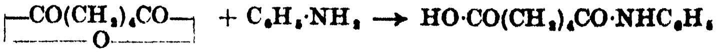
$$CH_2{-}\underset{\underset{\displaystyle CH_3}{|}}{C}{=}CH{-}CH_2{-}CH_2{-}\underset{\underset{\displaystyle CH_3}{|}}{C}{=}CH{-}CH_2$$

This is the structural unit in stretched rubber.

(3)


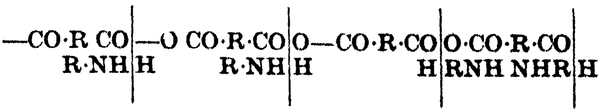
$$CH_2{-}\underset{\underset{\displaystyle CH_3}{|}}{C}{=}CH{-}CH_2{-}CH_2{-}CH{=}\underset{\underset{\displaystyle CH_3}{|}}{C}{-}CH_2$$

Chemically, the polymer can often be considered as the two polar end-groups joined together by a long, more or less flexible bar. An end-group can be used to determine the molecular weight of the polymer. For example, in the polymers from the ω-hydroxy acids, each polymer-molecule contains one free carboxyl group which can be titrated, thereby giving a rather accurate estimate of the extent of polymerisation. In other cases, peculiarities occur in the polymer. A very pretty case is that of adipic anhydride, whose monomer forms an anilide in the usual manner:



$$\underset{\text{O}}{\underline{{-}CO(CH_2)_4CO{-}}} + C_6H_5{\cdot}NH_2 \rightarrow HO{\cdot}CO(CH_2)_4CO{\cdot}NHC_6H_5$$

But in the polymer, addition can occur in the following ways:

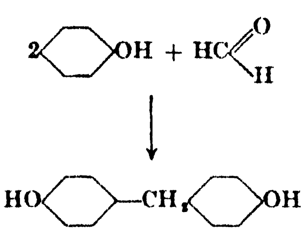


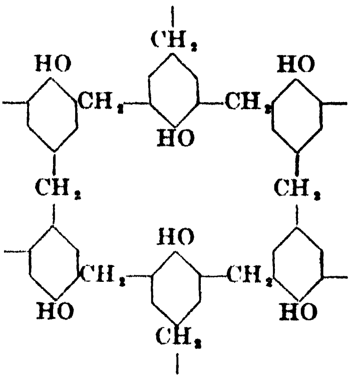
$$\begin{array}{r|l|l|l|l} {-}CO{\cdot}R\ CO & {-}O\ CO{\cdot}R{\cdot}CO & O{-}CO{\cdot}R{\cdot}CO & O{\cdot}CO{\cdot}R{\cdot}CO & \\ R{\cdot}NH & H \quad R{\cdot}NH & H \qquad\quad H & RNH\ NHR & H \end{array}$$

For infinitely long chains, the average proportion of products will be 50 per cent of monoanilide, and 25 per cent each of dianilide and free acid. Such a distribution has been found.

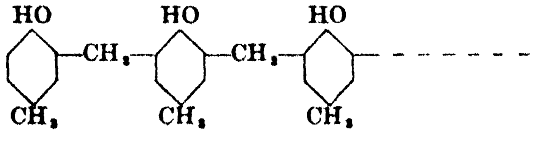
THE SHAPE OF POLYMERS

For simplicity, only linear polymers have been mentioned so far, but the most important technically are three-dimensional polymers. The phenol-formaldehyde type of resin is built up on some such plan as:





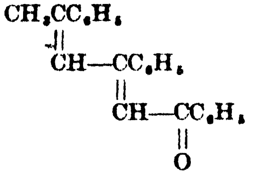
This is somewhat idealised and only ortho and para coupling is allowed for. It is clear that the polymer molecule can reach enormous dimensions. A lump of resin might be a single giant polymer molecule. This is a good type of polymerisation since the rings existing in it are starting-points for other rings linked to them. If the number of positions of coupling is reduced by using *p*-cresol, coupling can only occur in the ortho position (to the OH) and linear polymerisation results.



Two types of ring formation must be considered. Ordinary ring closure of single molecules which is antagonistic to polymerisation; and ring closure of polymers. The latter is not always easy to detect. It requires, of course, that there should be no active terminal groups, but the activity of these groups in the linear polymer is decreased when the length is great. ε-caproic acid, $NH_2(CH_2)_5COOH$, polymerises linearly, but the polymers showed no evidence of free acid or amino groups. Chains may be formed which consist of two long chains parallel and joined at the two ends. Sometimes the shape of the polymer can be identified by

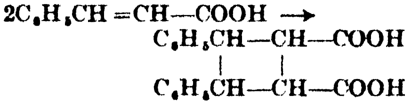
X-ray examination. The Würtz reaction on decamethylene dibromide yielded 2, 3, 4, 5, 6, and 7 polymers which were recognised as the normal hydrocarbons containing 20, 30, 40, 50, 60, and 70 carbon atoms respectively by the X-ray long spacings.

A substance such as acetophenone can polymerise only linearly :



$$\begin{array}{lll} CH_2CC_6H_5 & & \\ \downarrow\| & & \\ CH{-}CC_6H_5 & & \\ & \| & \\ & CH{-}CC_6H_5 & \\ & \;\;\| & \\ & \;\;O & \end{array}$$

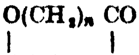
Ring formation is fairly common to form cyclo-butane derivatives such as the addition of two molecules of cinnamic acid to form truxillic acid :



$$2C_6H_5CH{=}CH{-}COOH \longrightarrow \begin{array}{l} C_6H_5CH{-}CH{-}COOH \\ \qquad\;\;|\qquad\;| \\ C_6H_5CH{-}CH{-}COOH \end{array}$$

If the active groups are separated by 5 or 6 carbon atoms, ring closure becomes more probable. There are a number of substances which can only occur as rings or as polymers ; for example, the anhydrides of the polybasic acids.

The substance ε-caproic acid already mentioned in which the reactive groups are separated by 5 carbon atoms forms the ring lactam, as well as the polymer in the proportion of about 20 per cent of lactam to 80 per cent of polymer, at 210° C. The hydroxy acids can react in three different ways, all of which have been observed. The monomer can form a lactone

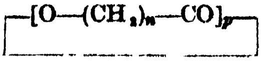


$$\begin{array}{l} O(CH_2)_n\;CO \\ |\underline{\qquad\quad}| \end{array}$$

Linear polyesters are formed by intermolecular esterification :

$$H[O{-}(OH_2)_n{-}CO]_pOH$$

Finally, cyclic polyesters can be formed :



$$\boxed{[O{-}(CH_2)_n{-}CO]_p}$$

The Mechanism of Polymerisation

The possibility of a linear polymer forming a closed chain and so stopping further growth is not great since, while the ends of the chain are swinging round, there is much greater chance of meeting active groups on the ends of other molecules—single or polymer—before they come together. The ease with which very long polymer chains are formed has not been explained. Little is known of the kinetics of the process except for a mass of information on accelerators and inhibitors. Numbers of patents have been taken out for catalysts for making resins and synthetic rubber. From this mass of empiric facts, there emerge two salient ones. First, that the more rapid the polymerisation, the smaller the size of the polymer-molecules. In some cases apparent limits to the extent of the polymerisation appear to exist, but they may be due in these cases to some accidental experimental conditions. An interesting case is that of the action of sodium on *p*-dibrombenzene. In ether, $Br{\cdot}C_6H_4(C_6H_4)_6C_6H_4{\cdot}Br$ was formed, while in boiling toluene, the polymer $Br{\cdot}C_6H_4(C_6H_4)_{12}C_6H_4{\cdot}Br$ twice as long was formed. According to the rule stated, we should have expected the shorter polymer in the boiling toluene.

The second point is that oxygen is a very efficient catalyst for many polymerisations, although it has been found to act as an inhibitor when present in excess In cases where it has acted as catalyst, organic peroxides have been isolated, but this cannot be taken to mean that they are the accelerators. That they are isolated may mean that they represent the inhibited polymerisation, the action in this case being oxidation of polar groups so that polymerisation cannot occur.

In substances such as the saturated hydrocarbons, the unsaturated ethylenic compounds and the benzene ring it is easy to picture the action of the oxygen as loosening the terminal hydrogen atoms which are eliminated or by loosening the double bond of the ethylene linkage. Further, one can picture a chain mechanism by which a single oxygen atom builds up an objective polymer chain of great length. The rate of polymerisation of vinyl acetate is pseudo-unimolecular, due to formation of an activated molecule for each one which reacts.[1]

It is unfortunate that we do not know anything of the energy changes in the polymerisation processes. In condensation of paraffins or of benzene rings, a hydrogen is eliminated from each condensing molecule, but we do not know how the critical energy for this loosening varies as the polymerisation increases. Is this energy the same for methane, pentane and a hydrocarbon infinitely long ? Or is it the same for benzene, diphenyl and di-diphenyl ? The internal energy corresponding to a given temperature increases with the number of linkages in the molecule, but we do not know to what extent this energy contributes to the breaking of any one particular link. This is a general problem, but its bearing on polymerisation is clear. In some cases polymerisation seems to proceed more rapidly when it has gone a short way. Freshly distilled styrene when heated polymerises with difficulty. If it stands for a day, it goes very much more readily at the same temperature. This, however, may be a spurious effect due to absorption of oxygen while standing which then-acts as a catalyst. It was found that the trimeric *iso*butylene when free from monomer and dimer would not polymerise further, whereas in their presence higher polymers were formed readily.[1]

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[1] H. W. Starkweather and G. B. Taylor, *J.A.C.S.*, 1930, 52, 4708.

THE PHYSICAL PROPERTIES OF POLYMER MOLECULES

When a substance or mixture has been polymerised by heating or other suitable treatment, a mass is obtained which consists of polymers of various sizes and, possibly, some free molecules. Some workers state that the resins are gels and that they are two-phase systems. Obviously, at sufficiently high temperatures there will be a suspension of large particles in a liquid dispersion medium, but under those conditions the mixture is not a resin. The word " phase " in its ordinary scientific sense is meaningless in this connection. The resins have " melting-points " which are, in the higher polymers, the transition from opaque to clear plastic mass. They are not fluid owing to the structure of the larger polymer-molecules present. The mechanical properties of a resin depend upon the shape of the polymer molecules. It is desirable that polymerisation should be of the three-dimensional type since the linear polymers tend to be fibrous. They are similar to rubber except that they are harder at room temperatures. Carothers has described how linear polymers of oxy-decoic acids can be drawn out into threads when hot provided the molecular weight is above 7,000 and still higher polymers can be cold drawn to threads which have a silky lustre, very large tensile strength and which are optically homogeneous and give an X-ray fibre picture.[2] The method of X-ray examination does not give information of the size of the polymer-molecules but of the regularly repeated atomic groupings within it. For example, cellulose gives a spacing of 10·3 Å.U., confirming Haworth's structure proposed on purely chemical evidence. The micelles in cellulose may have micellar weights as high as 100,000. That is, the micelle is a polymer-molecule some 700 glucose units long.

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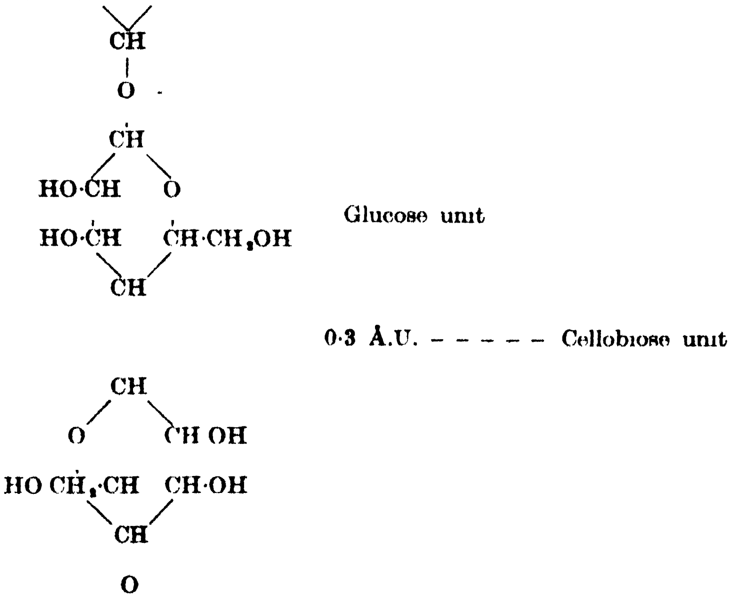
[1] S. V. Lebedev and G. G. Koblyanki, *Ber.*, 1930, **63**, 1432.

[2] W. H. Carothers and F. J. van Natta, *J.A.C.S.*, 1933, 4714.

Cellulose



Optical examination of solutions of polymers may give evidence of fibrous form by streaming double refraction. It is, of course, necessary that there should be sufficient difference of refractive indices of polymer and dispersion medium. Then, if the form is linear, streaming will orient them and the flowing liquid will appear birefringent. Polystyrene shows the effect markedly. The polymer is :

$$- - - \mathrm{-CH-CH_2-CH-CH_2-CH-} - - - - - - -$$

The cresol-formaldehyde polymer referred to already shows no such double refraction which suggests that the picture of linear polymerisation by ortho coupling is too simple. Ring closure of the polymer chains must occur or else sufficient meta coupling to form three-dimensional polymer. The amount of this meta coupling need obviously be quite small to destroy all linearity.

The most complete examination of a physical property of polymers has been made by Staudinger who used viscosity as his tool. High polymers form solutions which are colloidal. The micelle is the polymermolecule. Since this is held together by valency forces, the temperature coefficient of viscosity of such sols is that of a true solution and not the anomalously high one of sols such as the soaps in which raising the temperature increases the dispersion as well as the normal temperature effect on the liquid. Since the micelle is a single polymermolecule, the solutions are all true molecular solutions, but some are colloidal and the lower polymers are not. There is no sharp dividing line. The sols of intermediate polymers are colloidal or not according to the definition or test employed. In all such solutions, linear polymers cause increase of viscosity above that of the solvent according to their length. From measurements, therefore, of viscosity, estimates of the size of polymers of any series can be obtained. Staudinger has also pointed out that there will be a critical concentration of any linear polymer above which the viscosity will increase more rapidly owing to increased interference of the micelles. This concentration is calculated by assuming that the polymers occupy effectively a volume equal to $\pi\left(\frac{l}{2}\right)^2\theta$ where l is their length and θ thickness. When this effective volume multiplied by the number of micelles in a given volume of solvent is equal to that volume, the critical concentration is reached and further increase of solute will cause greater interference of micelles with correspondingly greater viscosity. When the concentration gets higher still, flow will become anomalous. Poiseuille's law is no longer obeyed and, if the polymers are large enough, gelation may occur.

A synthetic resin is not a gel since there is no liquid dispersion medium present. The commercial products are treated specially to remove free molecules and to weld the mass into the most coherent form possible. I have not mentioned the types of polymer used for commercial purposes since the common ones are well known and the total number of ingredients which can form polymer resins is so great that the mere list is suggestive of the worst type of preparative chemistry. I have tried to show that we have in polymers a new field for investigation, in which there is scope for every type of scientific inquiry and room for everyone who is interested in the objective reality of the molecule.

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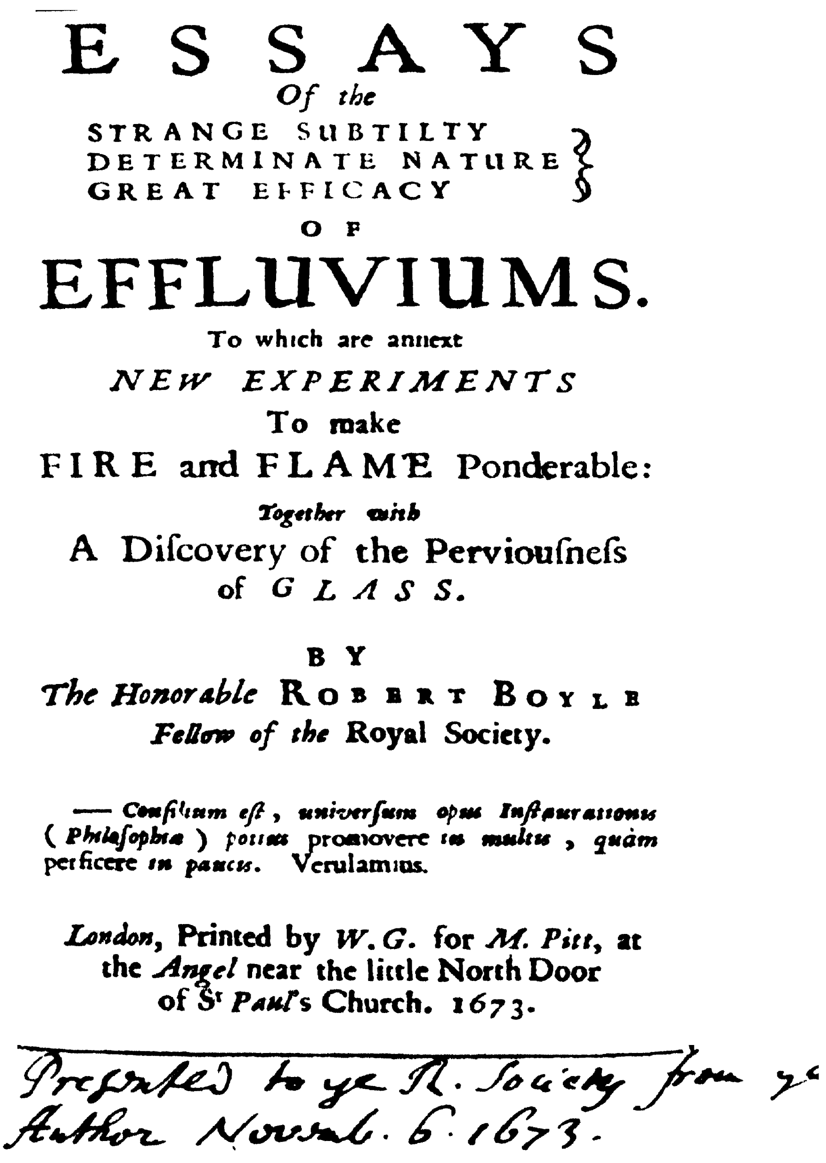

ESSAYS

Of the

STRANGE SUBTILTY
DETERMINATE NATURE
GREAT EFFICACY

OF

EFFLUVIUMS.

To which are annext

NEW EXPERIMENTS

To make

FIRE and FLAME Ponderable:

Together with

A Diſcovery of the Perviouſneſs
of *GLASS.*

BY

The Honorable ROBERT BOYLE
Fellow of the Royal Society.

— *Conſilium eſt, univerſum opus Inſtaurationis (Philoſophiæ) potius* promovere *in multis, quàm* perficere *in paucis.* Verulamius.

London, Printed by *W. G.* for *M. Pitt*, at
the *Angel* near the little North Door
of S^{t} *Paul's* Church. 1673.

Presented to ye R. Society from ye
Author Novemb. 6. 1673.

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THE HON. ROBERT BOYLE'S *ESSAYS OF EFFLUVIUMS* (1673).

By DOUGLAS McKIE, Ph.D., B.Sc.

Department of History and Methods of Science, University College, London

Before the work of Lavoisier the most important attempt to explain the increase of weight observed in the calcination of metals was made by the Hon. Robert Boyle in his *New Experiments to make the Parts of Fire and Flame Stable & Ponderable* (London, 1673). With occasional modification Boyle's explanation that this gain in weight was due to the fixation of fire-particles in the metals was generally accepted by chemists for the next hundred years, although during this period chemical thought was completely dominated by the phlogiston theory. It has been claimed that, had Boyle's conclusions been a little bolder—and Boyle was never a bold theorist—" he would have forestalled the phlogiston theory " (J. F. Fulton, *A Bibliography of the Honourable Robert Boyle*, Oxford, 1932, p. 73). On the other hand, some writers have held that Boyle's ideas on this problem formed the basis of that theory, but it has been pointed out that his explanation that a metal gains in weight on calcination by the addition of igneous corpuscles is the very opposite of the phlogistic explanation that calces are produced from metals by the loss of phlogiston (J. H. White, *The History of the Phlogiston Theory*, London, 1932, p. 41).

Boyle's tract, however, appears to have been studied previously in isolation, whereas it should certainly have been considered in relation to his *Essays of Effluviums* (London, 1673), in which it forms one of five tracts. The historical interest and importance of these *Essays* have been much neglected. Fulton refers to this book as " one of the important but little-known scientific works of Boyle, ranking in some respects with the *Sceptical Chymist* " (op. cit., p. 73). Its full title reads *Essays of the Strange Subtilty, Determinate Nature, Great Efficacy of Effluviums. To which are annext New Experiments to make Fire and Flame Ponderable : Together with A Discovery of the Perviousness of Glass.* For the purposes of this paper we have been privileged to use the copy [1] presented by Boyle to the Royal

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[1] No. 106 in Fulton's *Bibliography* (p. 75).

Society on November 6, 1673, the title-page bearing his inscription being reproduced here by kind permission of the Society.

The joint appearance of these five tracts in one volume is explained in an introductory *Advertisement to the Reader*, in which it is pointed out that the author had previously shown that "the corpuscular Philosophy"[1] gave more satisfactory and comprehensible explanations of the properties of bodies than did the Aristotelian theory of the four elements and the Schoolmen's doctrine of substantial forms; that he had since made many further notes and experiments, more especially relating to "occult" properties, that is, properties that were not visible or manifest, and that these studies had necessarily led him to the consideration of effluviums, the pores of bodies, the figures of corpuscles and local motions; that this work together with the material of his *Notes &c. about the Atmospheres of Consistent Bodies*, appended to *A Continuation of New Experiments Physico-Mechanical, Touching the Spring and Weight of the Air, and their Effects. The I Part* (Oxford, 1669), now appears in these *Essays*, unrevised because he had found "that in what he had design'd about Occult Qualities, he had cut himself out more work than probably he should during many years have opportunity to set upon in earnest, and complete"; that, since the notes on the pores of bodies, the figures of corpuscles and local motions were either missing or so misused that they could not be easily got ready for this volume, the author added the tract on Flame because of its close connection with the doctrine of effluviums; and that he had intended to include another tract in which the ponderability of the matter of sunbeams[2] was to be discussed, but the "cold and wet Summer" was "unfriendly to the Tryals to be made with Burning-glasses."

The *Essays* are written in the style and with the scientific approach that characterises all Boyle's work: the philosopher ambles leisurely along the road, continually diverging to wander up parenthetical bypaths but always returning to the highway of his purpose with another fact or illustration carefully gathered for his argument. Exact quantitative experiments are noted down alongside bare observations of everyday life together with travellers' and occasionally old wives' tales. But everywhere the *Essays* show Boyle's keen perception of the common facts of nature and of their value in supporting or refuting hypotheses: and throughout they are written in that glorious seventeenth-century prose of which the Sceptical Chymist was so notable a master.

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[1] Boyle was the restorer of mechanical philosophy in England: he sought to explain all natural phenomena in terms of corpuscles that differed in their sizes, shapes, motions and order of arrangement.

[2] Whence he had hoped to obtain some evidence bearing on the question of whether light was corporeal or a mere quality.

THE STRANGE SUBTILTY OF EFFLUVIUMS

The tract *Of the Strange Subtilty of Effluviums* opens with the statement that, although the view that the effluviums of bodies consist of extremely minute particles is not inconsistent either with the theories of the atomists, who suppose that all bodies are made up of insensible, but indivisible, corpuscles, or with those of the Cartesians, who hold that matter is indefinitely, if not infinitely, divisible, its correctness will be demonstrated, not by deduction from principles, but by induction from experiments and observations. Boyle then proceeds to collect evidence to show that some bodies are capable of great rarefaction without their parts becoming intangible; that a small portion of matter can produce a "multitude of Visible Corpuscles"; that the pores that are penetrable by the effluvia of certain bodies are extremely small; that a body loses only a minute part of its weight on parting with "great store of Effluvia"; and that a small amount of matter, when rarefied, may sensibly fill a very large space.

From the extensive mass of evidence discursively presented by Boyle it will suffice here to quote some typical instances in the order indicated in the preceding paragraph. Gold, silver and gilt wires and silk could be drawn to an exceedingly great fineness: the measurement of the area of six weighed gold-leaves showed that a grain of gold was divisible into two million visible parts: and an ounce of gold, used to gild silver wire, was extended to a length of over 155 miles, while this superficial cylinder of gold was clearly further divisible into strips along its length. An ounce of water converted into steam by an æolipile maintained a conical cloud, 1 inch in diameter and 20 inches long, of "manifestly visible Corpuscles" for about 20 minutes: the oil of a lamp wasted imperceptibly, although its particles continually furnished "a considerable stream of shining matter": the smoke from a half-grain of gunpowder filled a space more than 500,000 times larger than that originally occupied by the powder: the parts "that make up the Alimental Liquors" of the embryos in the eggs of cheese-mites must be "extremely and unimaginably minute": and one grain of copper dissolved in "Spirit of *Sal Armoniack*" perceptibly coloured 513,620 times its bulk of water. Cantharides applied outwardly affected the secretion of urine: the poisons of some animals acted through the clothes and without direct contact:

and the effluvia of loadstones penetrated glass,[1] "generally reputed to be as close a Body as any is." Weighed amounts of ambergris, asafœtida, nutmegs and cloves suspended in the air for days lost either nothing or only a minute fraction of their weight, though they continually emitted odours[2] Dogs were able to track down animals and men hours after their quarry had passed, though the effluvia of the latter were exposed to the air and had meanwhile impregnated an incomparably greater atmosphere · plague was preserved in clothes and beds for long periods · and gloves had retained their scent for 30 years.

THE DETERMINATE NATURE OF EFFLUVIUMS

In the second tract, *Of the Determinate Nature of Effluviums*, Boyle complained that Aristotle and the Schoolmen had classified effluviums into "hot and dry" and "hot and moist," into fumes and vapours, a system that was as useful as the division of animals "into those that are *Horned*, and those that have *Two Feet*," whereas it could be shown that effluviums retained the properties of the bodies from which they were emitted. The water-vapour in the air condensed into drops on cold surfaces or fell as dew and rain. Gilders had died from the fumes of mercury, the condensed liquid, alleged Boyle, "having been several times found in the Heads and other parts of such People." Flowers of sulphur, obtained by sublimation, could be melted into sulphur identical with that originally subjected to sublimation: and camphor sublimed unchanged. Chemical distillations provided further evidence. The fumes of spirit of nitre or spirit of salt had the properties of the fluids that evolved them. The essential oils of vegetables, such as oil of cinnamon and oil of wormwood, although mere effluvia and highly rarefied, had the genuine taste of the bodies from which they were distilled. A bloodhound would track down a particular man over many miles and through crowds of other men. Magnetic effluvia did not lose their properties by passing through glass. Apothecaries had been cast into deep sleeps when distilling opiates: and drugs left openly in rooms had brought on stupefaction until they were removed, while the odours of certain medicines produced the same effects on some persons as if these medicines had been taken orally.[1] Writing in invisible ink was made plainly legible by the effluvia from a suitable liquid both at a distance and when the writing was enclosed in several sheets of paper.

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[1] Boyle proved this by studying the effects of a loadstone on needles hermetically sealed in glass tubes floating on water, and by magnetising a thin cylinder of iron sealed in a glass tube and set perpendicularly to allow of penetration by "the Magnetical *Effluvia* of the Earth."

[2] Boyle was not sure whether loadstones lost weight: he distrusted his balances and he thought that the effluvial losses might be continually repaired, partly by particles given out at one pole returning to the other and partly by particles from the earth.

" Other Examples," said Boyle, " may be given of the setled difference of Effluviums directly perceivable by Humane Organs of Sense, as dull as they are ; which last expression I add, because I scarce doubt, but that, if our Sensories were sufficiently subtile and tender, they might immediately perceive in the size, shape, motion, and perhaps colour too of some now invisible Effluviums, as distinguishable differences, as our naked Eyes in their present constitution see, between the differing sorts of Birds, by their appearances, and their manner of flying in the Air, as Hawks, and Partridges, and Sparrows, and Swallows."

THE GREAT EFFICACY OF EFFLUVIUMS

The third tract, *Of the Great Efficacy of Effluviums*, asserts that too much trust has been placed in the negative information of the senses with consequent great error in philosophy and medicine. Minute corpuscles, although invisible, can, if they are numerous, fitly shaped and in rapid motion, act on bodies in ways in which larger and visible masses cannot act. Aqueous vapours, regarded as the faintest and least active of all effluviums, would in rainy weather dissolve salts, cause bodies to putrefy and oak doors to swell, and formed drops of rain which in sufficient number led to great floods and inundations. A rope was so shortened in damp weather that it sensibly raised a lead weight of 100 pounds The effluvia of *aqua fortis* reduced corks " into a yellow pap." Vessels containing certain substances had to be opened warily to avoid the effects of the issuing " steams." " Spirit of *Sal Armoniack*," without heating, gave effluvia that would open the nasal passages when clogged with " a great cold." The aqueous exhalations that fell as dew in the torrid zone would rust knives in their sheaths and swords in their scabbards. The power of corpuscles in rapid motion was obvious from the effects of gunpowder, flames, winds and whirlwinds. " Vigorous Loadstones " could demagnetise a needle or reverse its " verticity," and carry suspended eighty times their own weight. Odours often produced headaches, faintness, swoons and worse evils : and the " steams " of " Spirit of *Sal Armoniack* " would sometimes restore those so struck down. Notable medicinal effects were produced by the outward application to the body of gems, stones and other remedies.

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[1] Effects usually ascribed in Boyle's time to occult qualities, *i.e.*, properties.

And, adds Boyle, it must be remembered that effluvia are not emitted from a body all at once "as Hail-shot out of a Gun," but as "Waters out of a Spring-head in continued Streams, wherein fresh parts still succeed one another."

THE PONDERABILITY OF FIRE AND FLAME

Although the fourth tract, *New Experiments to make the Parts of Fire and Flame Stable and Ponderable*, is almost entirely experimental, its connection with the preceding tracts is clear. Boyle had already indicated in several places that he regarded flame as rarefied fuel "Fewel when 'tis turn'd into Flame" he compared with "the rarefaction of Water when 'tis turn'd into Vapors" (*Strange Subtilty, etc.*, p 20): inflammable bodies suffered a "prodigious Expansion" on conversion into flame (*ibid.*): and flame was a solution of the parts of fuel in an "Igneous *Menstruum*," which was comparable in its action on fuels with *aqua fortis* and *aqua regia* in their action on metals (*ibid.*, p. 54). To Boyle therefore flame was a solution in an igneous menstruum of the rarefied corpuscles or effluvia of a combustible. He recognised the novelty and improbability of such a theory, and accordingly sought to prove it "by a good number of Tryals." These experiments, summarised below, are historically the first extensive study of the increase of weight observed in the calcination of metals.

Experiment I.—A piece of copper, weighing 2 drachms 25 grains,[1] was fixed in a crucible, which was inverted and placed in the upper part of a tall crucible so as to be at a distance from the kindled sulphur which the latter contained. A hole in the base of the small crucible provided a vent for the flame. The sulphur burned away in 2 hours without any appearance of flowers of sulphur in the upper crucible. The copper had thickened, "which appear'd to be done by a real accession of substance." Boyle wiped the copper clean and, although this admittedly removed some particles of the metal, it was found that there was an increase of weight amounting nearly to 32 grains.

Exp. II.—A repetition of *I.* Silver, weighing 1 drachm less 2 grains, increased in weight to 1 drachm 5¾ grains. To save needless repetition, Boyle here pointed out that, while some might imagine that it was an extravagant hypothesis to suppose that such a light and rarefied body as flame could produce an increase in the weight of such heavy bodies as minerals and metals, flame, if it was a menstruum, would like other menstruums "have some of its own Particles united with those of the Bodies expos'd to its action."

[1] 20 grains = 1 scruple; 3 scruples = 1 drachm; 8 drachms = 1 ounce.

And these particles being of a piercing and terrestrial nature, " 'tis no wonder, that being wedg'd into the Pores, or being brought to adhere very fast to the little Parts of the Bodies expos'd to their action, the accession of so many little Bodies, that want not gravity, should, because of their multitude, be considerable upon a Ballance, whereon one or two, or but few of these Corpuscles would have no visible Effect."

Exp. III.—1 ounce of copper, heated in a shallow crucible in a cupelling furnace for 2 hours, increased its weight to 1 ounce 30 grains

Exp. IV.—1 ounce of copper filings was similarly heated " under a Muffler " for about 3 hours. It gained in weight about 49 grains. Boyle ascribed the larger increase (in comparison with that of *Exp. III*) to the longer time of exposure

Exp. V.—1 ounce of calcined hartshorn in small lumps was heated in a crucible in a cupelling furnace for 2 hours. It lost 6 or 7 grains, probably through loss of moisture, observed Boyle, since it gained slightly in weight on exposure to the air. 1 ounce of well-heated brick, in a similar test, maintained constant weight, but gained a little on subsequent exposure to the air.

Exp. VI.—1 ounce of tin heated on a cupel under a muffle for 2 hours was thoroughly calcined and gained 1 drachm in weight

Exp. VII.—1 ounce of lead, heated as in *Exp. VI*, was converted into litharge and, although there was a loss of some of the cupel through a breakage, an increase of weight of 7 grains was observed.

Exp. VIII.—A blank test with a cupel. The cupel weighed 2 ounces 2 grains when hot, but presently this had increased by 21 grains and later on it had taken up a further 3 grains. Boyle ascribed these additions to atmospheric moisture, to which initial losses and subsequent increases in the weight of cupels were evidently due.

Exp. IX.—4 drachms of steel filings, heated as in *Exp. VI*, gained 1 drachm 6½ grains.

Exp. X.—A piece of silver, weighing 3 drachms 32½ grains, heated for 1½ hours as in *Exp. VI*, increased in weight to 3 drachms 34½ grains. But Boyle suspected that it contained some copper and repeated the experiment with ½ ounce of " well refin'd " silver filings, heated for 3 hours. This now weighed 4 drachms 6 grains.

Exp. XI.—1 drachm of zinc or spelter filings, heated as in *Exp. VI* for about 3 hours, appeared to be calcined and increased in weight by 6 grains.

Exp. XII.—2 drachms of filings of tutenag (a zinc alloy from the East Indies), heated as in *Exp. VI* for about 2 hours, gained 28½ grains. On repetition with an equal weight, the same increase "abating less than one grain" was detected.

Exp. XIII.—Weighed amounts of lead and filings of copper were heated together in a cupel for about 2 hours, and the cupel appeared to have nothing on its surface "worth weighing distinctly." But the increased weight of the cupel almost accounted for the weight of the metals that had disappeared, which contradicted the common opinion that in cupellation lead and the baser metals removed from silver or gold "fly away in Smoak." Evidently they "enter'd into the Substance of the Cupel."

A change in procedure was now introduced. Boyle, "having shewn that either Flame or the Analogous Effluxions of the Fire will be, what Chymists would call, Corporified with Metals and Minerals exposed *naked* to its action," decided to try similar experiments "in close Vessels"; and, as it was difficult to expose bodies to fire in glass vessels without breaking or melting such containers, he first used crucibles "carefully luted together, that nothing might visibly get in or out." He therefore proceeded thus:

Exp. XIV.—1 ounce of freshly prepared steel filings, included between luted crucibles and heated strongly for 2 hours, gained 5 grains. In a repetition, 1 ounce gained 6 grains after being heated for 1½ hours.

Exp. XV—2 ounces of copper, 2 ounces of tin and 2 ounces of lead were separately and simultaneously heated in a similar way for about 1½ hours. The copper gained 8 grains and the tin 6 grains. The crucible containing the lead broke.

Exp. XVI.—Suspecting that thinner copper plates than those previously used might show a greater increase, especially if they were heated for a longer time, Boyle repeated *Exp. XV*, heating 1 ounce of thin pieces of copper for about 3 hours. The metal and the scales formed on its surface weighed 21 grains more than it did originally.

Exp. XVII.—A piece of block tin, weighing ½ pound, was heated in a glass retort in a sand-furnace which kept the metal in fusion without breaking the glass. The weight increased by about 2 grains. The retort was evidently sealed (pp. 27–9 and 37), but the nature of the seal is not stated.

Exp. XVIII.—Various weights of mercury were similarly heated in glass vessels, some hermetically sealed and others closed with corks. The sealed vessels broke and the process was so slow in the others that Boyle "was by a remove oblig'd to leave the tryal imperfect."

Boyle then tested whether a second heating would produce a further increase in the weight of bodies which had already been increased in weight by a previous heating.

Exp. XIX.—1 ounce of calx of tin heated for 2 hours as in *Exp. VI* gained 1 drachm 35 grains and appeared much whiter than it was before.

Exp XX.—1 ounce of steel filings, already increased in weight by a previous exposure to the fire, was subjected to a second exposure at the same time as the calx of tin in *Exp. XIX.* The weight increased by 2 drachms 22 grains.

Exp. XXI.—1 ounce 4 drachms of the copper from *Exp. XV* was included between two crucibles and heated in a strong fire for 2 hours It gained 10 grains and was covered with easily detachable dark flakes

These were the chief experiments that Boyle was able to carry out successfully, and he had used up all his " Cupels and commodious Glasses " He did not know whether by other means he might have been able " to make any discovery of the Nature of the Substance that made the Increment of Weight," but he was satisfied that he had shown that such experiments, especially if they involved cupellation, must be carried out " more warily," that his results would justify what he had to say elsewhere about " the Substance, what-ever it be, that we are speaking of," and that the discovery of this substance would excite the curiosity of philosophers since it was " a Fluid, far more subtile than visible Liquors, and able to pierce into the Compact and Solid Bodies of Metals . . . add something to them, that has no despicable Weight upon the Ballance, and is able for a considerable time to continue fixt in the Fire."

The tract concludes with a section entitled ***Additional Experiments, about Arresting and Weighing of Igneous Corpuscles,*** from which it is clear that in reverting to experiments in closed vessels (cf. *Exps. XVII* and *XVIII*) Boyle had satisfied himself as to the nature of the substance mentioned above. The new series ran as follows :

Exp. I.—8 ounces (troy) of block tin, cut into pieces, was put in a glass vessel with a long neck. The tin was melted by holding the vessel over " quick Coals " without touching the latter, and the vessel was shaken all the time. The orifice " was cover'd with a Cap of Paper (which sometimes fell off by moving the Glass) to keep the Air and Steams of the Coals from getting into the neck." This was continued for almost an hour. The tin was then removed from the vessel. It had gained 18 grains.

Exp. II.—The residue from *Exp. I,* metal and calx, was transferred with a loss of 3 grains to another similar vessel with " a stopple of Paper put to it to keep out Smoak and Air." The tin was melted as before and the vessel was then carefully laid on the coals and kept there for 2 hours. The tin now weighed 8 ounces 24 grains, and had thus gained only 9 grains in 2 hours, which Boyle ascribed to the fact that in this case the vessel was not shaken as in *Exp. I.*

Exp. III.—Boyle then tried heating a weighed amount of tin in a sealed retort to prevent all access of smoke or other particles. Shortly after this was placed on the fire, "it suddenly broke in a great multitude of pieces, and with a noise like the Report of a Gun; but (thanks be to God) it did no harm neither to me nor others that were very near it."

Exp. IV.—8 ounces of tin in small pieces were put in a glass vessel with a neck 20 inches long. This was hermetically sealed after it had been kept some time over the fire to avoid subsequent fracture through expansion of the included air. It was heated for 1¼ hours, and then opened. The tin had increased in weight more than 23 grains and the calx alone "amounted not to four Scruples or eighty Grains."

Exp. V.—A repetition of *Exp. IV* with 2 ounces of tin filings heated for 2 hours. Fumes were seen to escape from the orifice before it was sealed. On removal the tin weighed 2 ounces 12 grains, and the increment "appear'd to have been gain'd by the Operation of the Fire on the Metal."

Exp. VI—1 ounce of zinc filings was heated as in *Exp. V* for 4 hours. "Smoak" escaped at the orifice before it was sealed. There was a loss of over 5 grains, which Boyle ascribed to the exhalations that had escaped.

Exp. VII.—A repetition of the preceding with 4 ounces of lead in small pieces. Kept on the fire 2 hours before sealing and 2 hours after, the lead gained over 13 grains.

Exp. VIII.—A repetition with 2 drachms of coral, hermetically sealed in a glass bulb. In an unstated time the coral gained about 3½ grains.

Exp. IX.—2 drachms of fresh quicklime, heated strongly in a cupel for 2 hours, gained 29 grains.[1] Thus, argued Boyle, a body that had been exposed to very violent fire for days could still take up and retain fresh corpuscles; and it was possible that in the preceding experiments the increase of weight was greater than the balance indicated, since some of the less fixed particles of the bodies exposed to the fire might have been driven off.

This last experiment and those carried out in sealed retorts showed " that what is afforded by Fire may in a Corporeal way invade, adhere and add Weight to even fixt and ponderous Bodies."

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[1] From the carbon dioxide produced in the furnace.

THE PERVIOUSNESS OF GLASS

The last tract, *A Discovery of the Perviousness of Glass to Ponderable Parts of Flame*, is described as an appendix to the experiments " about Arresting and Weighing of Igneous Corpuscles." Here Boyle describes further experiments carried out with the flames of burning sulphur and spirit of wine to avoid the criticism " by suspitious Wits " that the vessels used in his previous experiments were exposed, not to the action of " mere Flame," but " held upon Charcoals," which were regarded by some as containing " but a Grosser kind of Fire."

Exp. I.—2 ounces of tin filings were heated in a glass retort over the flame of burning sulphur, the orifice being sealed when it was thought " convenient." The tin melted in 2 hours and was heated a further 1½ hours. It gained 4½ grains. But Boyle, like other chemists since his time, lost the paper on which his results had been written down. Repeating the experiment therefore, he found that 2 ounces of tin filings gained over 11½ grains.

Here, to meet the objection that the increases in weight observed in his experiments might be due to particles of the glass loosened by the violence of the heat and driven into the metals, Boyle carried out a blank experiment with two glass vessels. These were weighed, heated for " a considerable time " and re-weighed. One of them appeared to have *gained* over ½ grain, and the result showed that increments of weight of the order found in his experiments could not have proceeded from the glass.

Exp. II.—A repetition of *Exp. I* with spirit of wine, a homogeneous liquid affording a flame without soot or residual phlegm, whereas sulphur was known to contain oily or inflammable parts and acid parts. 1 ounce of filings of block tin was heated in a retort as before for nearly 2 hours and gained 5 grains, " so that of so fine and pure a flame as of this totally ardent Spirit, enough to amount to five grains was arrested, and in good measure fixt by its operation on the Tin it had wrought upon."

Exp. III.—A repetition of *Exp. II* with highly rectified spirit of wine and 1 ounce of lead, heated for over 2 hours. When the seal was broken, " the external Air rush'd in with a noise," which Boyle regarded as proof that the seal was tight. The gain in weight amounted nearly to 6 grains. Boyle here noted that, although " the *absolute* Gravity " of the metal was thus increased by the addition of the particles of flame, " yet this Aggregate of Lead and extinguish'd Flame had lost much of its *specifick* Gravity," so that the particles were not really transmuted into metal, but had made "a Coalition with those of the Lead."

These were not, added Boyle, the only experiments he had made on this question, but they were sufficient to make good the title of his tract. "For," he wrote, "whence can this increase of *absolute* weight (for I speak not of *specifick* Gravity), observ'd by us in the Metals expos'd to the mere flame, be deduc'd, but from some ponderable parts of that Flame? And how could those parts invade those of the Metal inclos'd in a Glass, otherwise than by passing through the pores of that Glass?" Boyle was careful to limit the penetrability of glass to some of the ponderable parts of flame Comparison of the increments in weight of bodies exposed to the naked fire and of those enclosed in vessels suggested that the difference in these increments might be due to the differences in size and motion of the corpuscles of which fire and flame consisted, the glass being porous only to "the minutest and most active," and this possibly only when its texture was opened by the violence of the flame. He did not agree with the opinion that glass was penetrable by "Chymical Liquors" or by quicksilver; and his own classic experiments had shown him that it was not penetrable by air.

Boyle then concluded with four corollaries that might "more warily be deduc'd" from his experiments to prevent any "unsafe Consequences" being drawn from them. Firstly, it appeared that flame as a menstruum could act on bodies, not only by comminution and dissipation of their parts, but also "by a Coalition of its own particles with those of the fretted Body, and thereby *permanently* adding Substance and Weight to them." And it was to be noted that a menstruum was not to be regarded as such only when it dissolved bodies, since some menstruums corroded bodies without dissolving them to any considerable extent, *e.g.* the action of *aqua fortis* on tin and of oil of vitriol on mercury.

The second corollary pointed out that the discovery of the perviousness of glass to flame called into question the truth of the opinion generally held by chemists and others about calcination, namely, that in this process the greater part of the body was driven away and only its earth and fixed salt left behind. "These notions," said Boyle, "are not well fram'd, and do not universally hold," since they were inapplicable to the metals used in his experiments. "For," he added, "it *does not* appear by our Tryals, that any proportion, worth regarding, of moist and fugitive parts was expell'd in the Calcination; but it *does* appear very plainly, that by this Operation the Metals gain'd more weight than they lost." Moreover, he had frequently obtained lead by merely heating its calx, and therefore calcination had certainly not deprived it "of the suppos'd radical moisture requisite to a Metal."

Thirdly, since the experiments showed that igneous particles penetrated glass and permanently adhered to the included bodies in sufficient numbers to be manifestly ponderable, they refuted the notions of the Epicurean atomists, who thought that, although the particles penetrated glass, they merely pervaded the included bodies in their passage upwards, and also the ideas of the Cartesians, who denied the penetrability of glass and argued that the textures of the included bodies were altered by the violent agitation originating in the fire and propagated to them by the small parts of the glass.

Finally, the experiments showed that very spirituous, fugitive and minute bodies, associated with congruous particles of another nature, might so change their nature "as to be arrested by a solid and ponderous Body" so strongly as not to be driven from it by a heat intense enough to melt and calcine metals This unsuspected result led Boyle to utter a veiled regret about the abandonment of the proposed experiments with burning-glasses, because it suggested "a main Inference to be drawn from the Operations of the Sunbeams on appropriated subjects, supposing it to prove like that of flame on Tin and Lead," the inference at which he here hints being clearly the corpuscular nature of light.

In conclusion, it may be pointed out that some writers have thought that Boyle was misled by the results of the experiments on the calcination of metals in sealed vessels, because the vessels were sealed only after some of the air had been driven out in the earlier stages of the heating, and he was thus led to ignore the real significance of the inrush of air when the seals were broken. But consideration of his general approach to this problem, as revealed in the *Essays of Effluviums*, suggests that he had already mentally connected the increase of weight of metals on calcination with the absorption of effluvia. Moreover, he nowhere appears to have suspected the part played by air in this process, although his classic experiments with the air-pump had made him already thoroughly familiar with the necessity of air for combustion. These studies were evidently directed by a belief in the corpuscular nature of flame, and in more fortunate circumstances the results here presented would doubtless have been accompanied by others tending to prove that light too was corpuscular.

GRAPHIC STATICS—A NEW APPROACH

By T. W. HALL, A.R.C.Sc I., B.Sc.
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Structural Engineering design can scarcely be said to be a virgin field for original ideas, and the definition of an engineer—" one who can do for one guinea what any fool can do for two "—comes more nearly true in this branch than for any other one of the profession. The designer's work, then, consists very largely, not in devising new forms, but in determining the sizes of members of standard forms which he may employ. To do this, the engineer has two tools ready for his use, calculation and graphics.

The controversy on the merits of the two has often waxed furious. A story is told of the late Professor P. G. Tait who held that calculation is always superior to graphical methods. At that time the greatest achievement of the Structural Engineer had been the design of the Forth Bridge, each member of which had been designed by the aid of graphics. Professor Tait undertook to calculate the force in each member of the bridge, but—he never finished the job. It may be concluded that graphical methods are an essential part of the equipment of every structural engineer.

The educationist has always looked askance on graphical methods as the student could learn to use them without realising the principles involved and the method readily degenerated into rule of thumb. A method is now available, however, which does away with this objection as it is parallel with the methods used in calculation and offers a complete substitute for the methods at present in use.

The method is the graphical equivalent of the well-known principle of moments, the first-discovered and still one of the most important generalisations of mechanics. The fundamental construction gives a graphical method of calculating the moment of a force about any point. If o is the point and F is the force (Fig. 1), set down a line ab to represent F in magnitude and direction. Join a and b to o and produce the line of action of the force to cut oa in a' and ob in b'. Then the intercept $a'b'$ represents to a certain scale the moment of F about the point o. Those familiar with the subject will at once see that the product of *a'b'* and *h* (*i.e.* the perpendicular from *o* on *ab*) gives the moment required.

For several parallel forces the construction is as shown in Fig. 2.

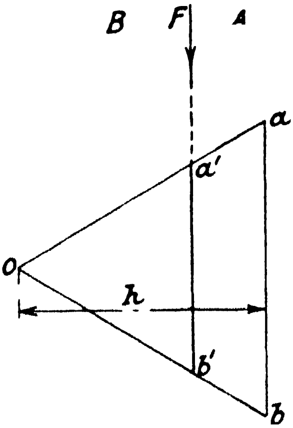

FIG. 1.

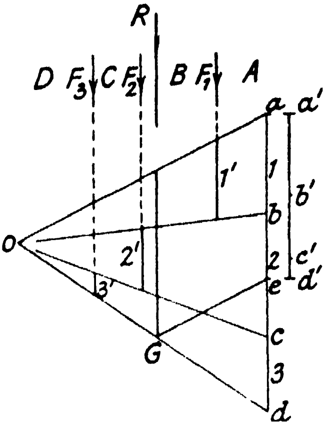

FIG 2.

The line of loads is set out as before and the appropriate intercepts are added to give the sum of the moments. If the sum of the moments is set out along the line of loads and a line drawn parallel to *oa* to cut *od* in G, then the resultant of the forces passes through G. This follows from the fact that a force equal to the sum of the forces, if it acts through G, will give a moment about *o* equal to the sum of the moments.

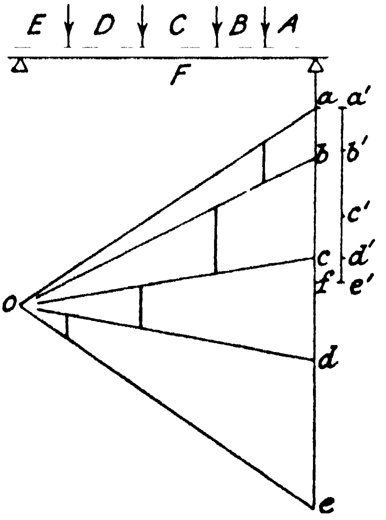

FIG. 3.

The construction is particularly useful for finding the reactions at the supports of a beam. The line of loads is set down from one support, in Fig. 3 the right-hand one. The moment about the other support is found for each force and their sum is equal to the moment of the right-hand reaction about the same point. Hence the sum of the intercepts represents the right-hand reaction. Also, if it is set down from *a* on the load line giving the point *f*, then *ef* represents the left-hand reaction. (For clearness the several reactions are set out to the right of the load line in Figs. 2 and 3.)

To find the reactions at the supports of any structure we can imagine it replaced by a beam with the same supports, P and Q. Produce the forces to PQ and consider the forces resolved along PQ

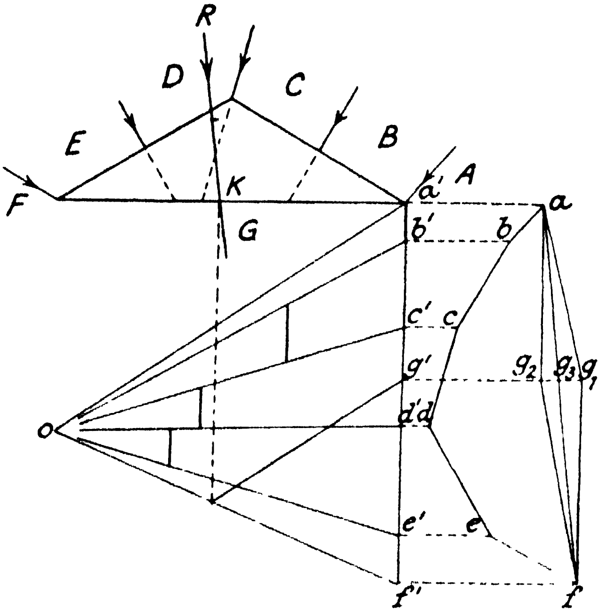

FIG. 4

and perpendicular to it. The horizontal components have no moment about a support so when the force figure is drawn as in Fig. 4, the vertical components are projected on to the vertical

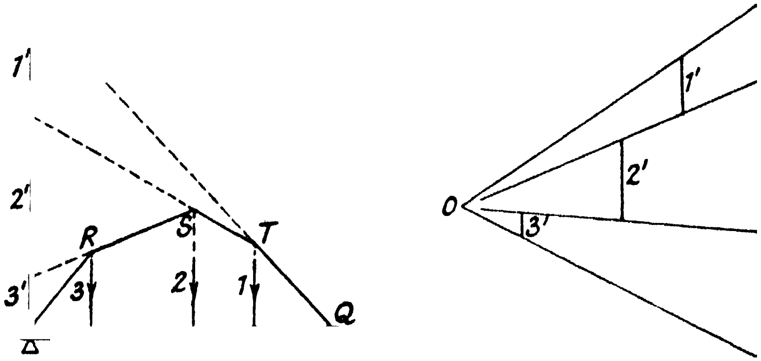

FIG. 5.

through a support. Using these components as vertical loads we can find the vertical components of the reactions. The actual reactions will depend upon the fixing and *g* will be in one of the positions g_1, g_2 or g_3, according as the right-hand or the left-hand reaction is vertical or the two are parallel. Finding the position of the resultant of the vertical components of the forces as before and denoting by K the point where it cuts the line PQ, we see that the resultant of the forces passes through K and is parallel to *af*. Thus

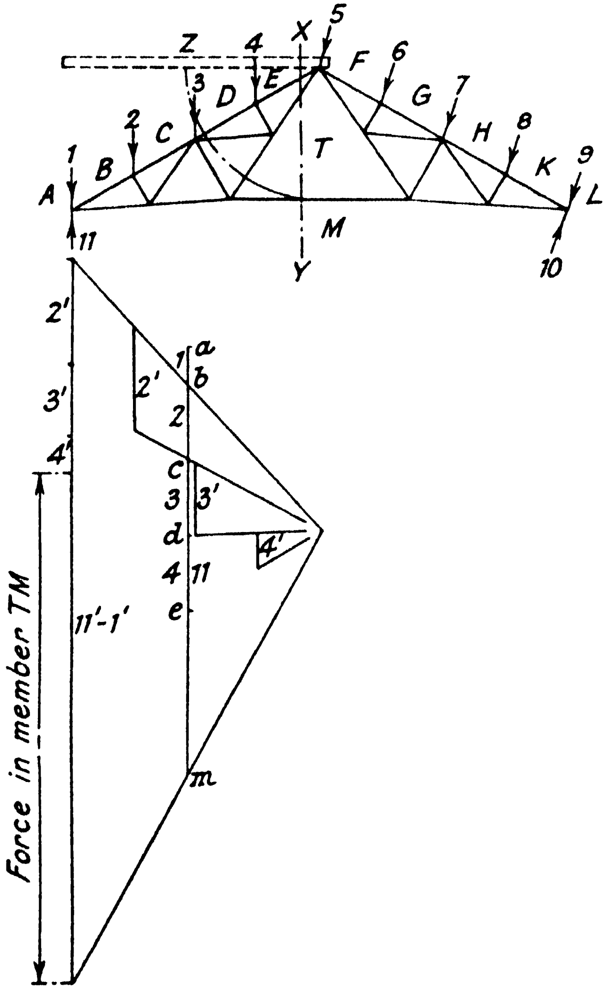

FIG. 6.

we can find the position of the resultant of any number of forces by imagining them to act on a beam.

The bending moment diagram for a beam is easily constructed from the moments about a support. If, in Fig. 5, 1′, 2′ and 3′ represent the moments of the forces 1, 2 and 3 about the left-hand support P, from P set up 3′, 2′ and 1′ in the order given and join as shown.

The figure PRSTQ represents the bending moment diagram for the beam. In the figure the distances set up are doubled to make the curve less "flat."

The very general "method of sections" depends upon the taking of moments about a point, so the construction applies. In Fig. 6, it is often necessary to find the force in the member TM by taking a section along XY. Drop a perpendicular from the vertex on TM and with the vertex as centre and this perpendicular as radius describe a circle cutting a horizontal through the apex at Z. Draw a vertical tangent to this circle at Z and along it set out the forces acting to the left of the section, including the left-hand reaction (supposed vertical) The force in the member TM is the same as the left-hand reaction on an imaginary beam supported at the vertex and at the point Z in Fig. 6 and loaded with the loads to the left of XY.

It has been proposed to call the method the "projection method" and it is expected that it will replace the use of the funicular polygon. A fuller account appears in the *Mathematical Gazette* for October 1933.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., Royal Observatory, Edinburgh.

Meteors.—Interest in meteors has been greatly stimulated by the spectacular shower of October 9, 1933, undoubtedly the finest observed since November 1885, when the famous Comet Biela, which had split into two separate portions in 1846, gave the second of its two remarkable meteoric displays. The shower of 1933, with radiant point near the star β Draconis, was not unexpected, but its magnificence came as a complete surprise. In *Monthly Notices, R.A.S.*, **80**, No. 8, Dr. M. Davidson made an examination of the comets which approach sufficiently near to the orbit of the earth for their associated meteors, if present, to enter our atmosphere in appreciable numbers, and he showed that, among others, Comet 1900 Giacobini-Zinner satisfied this condition and might be expected to give rise to a shower at its descending node, reached by the earth about October 10. The probable radiant point was given as R.A. 251·5° Dec. + 55·9°.

The period of this comet is 6·58 years, and as it was due at perihelion on December 11, 1926, special watch was maintained at various stations on the 9th of October preceding. It was soon realised that an exceptional meteor stream was in evidence. The observations were summarised by W. F. Denning in *Monthly Notices, R.A.S.*, **87**, No. 1, and little doubt remained concerning the connection of the comet with the meteoric display. Though all the meteors were slow-moving, they were remarkable for their long persistent trails, and a certain number were brilliant, but the total hourly count fell short of 20. On October 9, 1927, the meteors were again in evidence but very few in number, while in succeeding years up to 1932 none were visible. This negative result was not unexpected while the comet itself was in a remote part of its orbit, its return in 1933 being awaited with considerable interest. Its perihelion passage took place on July 15, when its distance from the earth was 114,000,000 miles, and a good display of meteors on the following 9th October was anticipated. The most sanguine expectations were, however, greatly exceeded. Starting about 7 p.m. the shower rapidly increased in intensity; the sky became thick with meteors almost as with flakes in a snowstorm, the number certainly approaching 500 per minute at the maximum. As in 1926, the meteors moved slowly leaving persistent trails, and they were mostly of the third or fourth magnitude, though there were several brilliant exceptions. During the whole display absolute silence prevailed. Unfortunately in many parts of England the weather was cloudy, but in certain areas, in Scotland, and in Northern Ireland, the storm was seen at its finest. Observations were also made at stations ranging from Central Asia to Spain, Portugal and the Gold Coast. Detailed descriptions are given in *Astronomische Nachrichten*, No. 5984; *L'Astronomie*, November 1933; and the *Journal of the British Astronomical Association*, **44**, No. 3. The results are summarised as follows by A. King in the last-mentioned publication:

1. The meteors were most frequent in the British Isles, more particularly in Northern Ireland.
2. The time of maximum was about 8.0 p m.
3. The meteors were mostly faint, and the effective duration of the shower was 4 to 4½ hours.
4. The meteor stream is undoubtedly connected with Comet Giacobini-Zinner.

It is not surprising that bright meteors formed such a small proportion of the number observed, for their velocity with reference to the earth was only 23 km. per sec., this being the lowest velocity known for any important shower.

The luminosity function of the Giacobinids is investigated by Fletcher Watson, jun., in *Harvard Bulletin*, **895.** He makes use of the naked eye observations of Felix de Roy in Belgium (*Gazette Astronomique*, **20,** 170) and of the telescopic observations of Sandig and Richter at Leipzig (*Ast. Nach.*, **250,** 170). The former observed 534 meteors in two intervals, one of 30 minutes when the shower was increasing in intensity, and the other of 10 minutes when it was at its maximum. The German observers used a 70 m.m. telescope with an eyepiece of power 8, the field being round the pole, about 35° from the radiant point. Certain factors determined by Dr. Öpik were applied to the actual counts of meteors in order to obtain approximate estimates of the numbers visible over the whole sky for magnitudes 0 to 8, the values for mags. 6, 7 and 8 being of course derived entirely from the telescopic observations. The curve of the luminosity function was then obtained by plotting the logarithm of the number of meteors of each magnitude against that magnitude. Luminosity functions were also obtained in a similar way for the

Perseid and Leonid showers, and the three curves were compared. The two latter show an unexpected similarity, with a shift between them of two magnitudes, which is probably due to the much greater geocentric velocity of the Leonids. Both curves have abrupt changes in slope at certain magnitudes, caused probably by a coma which forms round the larger particles of the shower but is non-existent when the particles fall below a certain size. A similar result, however, would be produced by a real lack of small bodies in the meteor swarms.

The magnitude shift between the Giacobinid and Perseid curves to be expected from the known difference in their geocentric velocities is three magnitudes, but the Giacobinid luminosity function shows other and distinct variations. These are probably caused by a real difference in the distribution of masses in the showers. For the Giacobinids a linear relation appears to exist from mag. 3 to mag. 7 between the apparent magnitude and the logarithm of the number of meteors.

In *Comptes Rendus*, **197**, No. 16, E. Esclangon calls attention to a few extremely brilliant members of the stream, some of which exceeded Sirius and even Venus in brightness, and left behind them long, luminous and persistent trains. There is evidently a great variety as regards size and mass in the components of this swarm. For the mean radiant, derived from observations at several stations Esclangon gives the value

R A. 266·1° Dec. + 55·0°.

In 1926 Dr. A. C. Crommelin calculated the radiant corresponding to the elements of the orbit of the comet, and obtained the value R.A. 265° Dec. + 54°, a result in remarkable agreement with the meteor observations of 1933. The orbital elements of the stream are compared with those of the comet by K. D. Pokrowsky in *Poulkovo Observatory Circular*, No. 10, and show close agreement. He also gives estimates of the number of meteors visible at different times, based on counts organised at the observatory.

In 1926 the shower preceded the comet's passage through perihelion by two months, while in 1933 the comet reached perihelion on July 15 and the shower was seen three months later. If, therefore, the meteors are the results of the comet's disintegration, they have evidently spread themselves out both in advance and in the rear of the parent body. It is possible that the great number which were visible in 1933 were due to a period of more rapid disintegration of the comet due to its recent passage through perihelion and consequent exposure to the intense influence of the solar rays.

This unexpectedly brilliant display was some compensation for a further disappointment caused by the Leonids in the following month. As it was exactly one hundred years since the memorable shower of 1833 which inaugurated the serious study of meteoric astronomy, the main swarm of meteors should again have been in the neighbourhood of the earth in accordance with the accepted period of rather over 33 years. In Great Britain cloudy skies conspired to prevent anything being seen, but in America conditions were more favourable. Observations carried out at the David Dunlop Observatory on the nights of November 14–15, 15–16, and 16–17 are described in the *Journal of the Royal Astron. Soc. of Canada*, March 1934. Out of a total number of 678 meteors observed on the three nights only 231 belonged to the Leonid shower, the mean magnitude being 2·40. This is markedly less than the number observed at Harvard in 1932 when bright moonlight tended to interfere, and when the non-Leonids were only one-third as numerous as in 1933.

An innovation in meteor observation was introduced at Prague by Hubert Slouka of Karlovy University (*Nature*, 1934, Jan. 20). By arrangement with the Czechoslovak Government and with the Public Airlines Co. a 3-engined Fokker plane carrying the pilot, three observers, one assistant and a radio operator ascended from Prague at 1.30 a.m. on November 17 in order to get above the blanket of cloud which prevented any observation of the Leonids. The plane reached a height of 3,000 metres, but, although the sky there was clear, only a few meteors were seen. The observers found that their view was limited by the enormous wings of the aeroplane, nevertheless, granted certain modification in design to obviate this difficulty, the usefulness of this method for meteor observation, as well as for that of solar eclipses, was clearly demonstrated.

A convenient and simple method of computing the orbit of a meteor stream is described by Dr. M. Davidson in the *Journal of the British Astron. Assoc.*, **44**, Nos. 3 and 4. Formulæ are developed for obtaining the elements of the orbit, distinguishing between the two cases when the latter is assumed to be elliptic or parabolic. Fully worked illustrative examples are added, and should prove very helpful to anyone unaccustomed to such computations. In the case of a meteor shower associated with a comet, it is shown how the radiant point and the time of appearance of the meteors may be determined from the known elements of the comet's orbit. The author points out that exercise in meteor computation would provide the beginner with an excellent introduction to dynamical astronomy of a more difficult nature.

It is disappointing to learn from the *Annual Report of the Harvard Observatory*, 1934, that the financial situation has made it necessary to close down the work of the Arizona Meteor Expedition after nearly two years of successful operation. This was the first prolonged scientific effort to obtain systematic data on the heights and speeds of these bodies, and it is some consolation to know that the abundant material already accumulated is likely to extend our knowledge regarding the cosmical origin of meteors very considerably, while their study is still to retain an important place in the programme of the Observatory.

In this connection a general survey has been made of the whole Harvard collection of plates in a search for meteor trails photographed fortuitously. This search and its results are described in *Harvard Bulletin*, **852.** The scrutiny has included the photographs taken at all the Harvard stations except Jamaica ; in all, no less than 71,454 plates have been examined and 213 meteor trails found. An extensive series of plates taken more recently with a new Ross Wide Angle Xpres lens have also been searched by Dr. Willard J. Fisher, and the combined results have been tabulated as a Catalogue by Dr. Fisher and Miss Olmsted in *Harvard Annals*, **87**, Part 3. This Catalogue includes records of 386 meteor trails, and tabulates the time and duration of exposure, the sun's longitude, the coordinates of the initial and final points of the trail, its length and position angle, together with descriptive notes. In *Harvard Annals*, **87**, Part 2 B, Miss Olmsted describes a method of obtaining the geocentric path of a meteor from two photographs taken simultaneously at different stations, and, as an example, calculates the heights of three meteors at different points of their paths from three pairs of trails found on photographs taken at stations 1·8 miles apart.

In *Harvard Circular*, **385**, Willard J. Fisher deals with the Mass and Velocity of Meteorites, and the air density along their luminous paths. His theory is founded on the assumption that the rate of loss of mass of the meteorite at any moment is proportional to the rate of loss of kinetic energy due to diminution in its velocity. Only bodies which actually reach the earth in solid form are considered, and evidence in support of the theory is provided by facts of observation, both visual and photographic, and by the study of meteorites actually found on the ground, illustrations of which are given in the paper. Reasons are given for assuming the following attributes of a meteorite : a rectilinear path ; a velocity exceeding that of sound ; no fracturing ; a shape not sensibly altered by ablation, so that the figures assumed by the object remain similar solids ; a permanent orientation ; an air resistance proportional to its density, to the square of the velocity of the meteorite, to its cross-section normal to its path, and to a constant depending on its shape. A formal solution relating velocities, masses and air densities is given in the form of an equation connecting two integrals, one of which is exponential, but the presence of three constants whose values are as yet uncertain makes the result of theoretical interest rather than of practical value.

New Instruments —The construction of two large telescopes has lately been entrusted to the firm of Sir Howard Grubb, Parsons & Co., of Newcastle-on-Tyne. One, a reflector of 36-inch aperture, has already been completed and installed at the Royal Observatory, Greenwich, the formal ceremony of "opening" the telescope being performed by the First Lord of the Admiralty on Visitation Day, June 2. The telescope, complete with building and dome, is the gift of Mr W. Johnston Yapp, and is intended to commemorate the work of Sir Frank Dyson during his 25 years as Astronomer Royal. It possesses greater light-gathering power than any hitherto located at Greenwich, and is exactly equal in size to the large reflector of the Royal Observatory, Edinburgh, which it resembles in many ways though there are important differences. The new instrument is described by C. Davidson in the *Observatory*, May 1934. The mounting is of the modified English form with a long polar axis supported by piers at the north and south ends, and the telescope is attached to the middle. The large mirror, 36 inches in diameter and 15 feet focal length, is 6 inches thick and weighs 550 lb., and two Cassegrain convex mirrors of fused quartz, 11 and 7 inches in diameter, are provided for use with a slit and a slitless spectrograph respectively. Electric motors have been provided for quick and slow motions in both Right Ascension and Declination. It is intended to employ the instrument in continuing the work on colour temperature of stars which has been carried on very successfully for some years with a less convenient and less powerful equipment.

The moderate size of this telescope is in striking contrast with the new 74-inch reflector of the David Dunlop Observatory near Toronto, described by R. K. Young in the *Journal of the Royal Ast. Soc. of Canada*, 1934, March. This instrument is not yet in commission, the large mirror being still at Newcastle, but the building and dome have been erected and the mounting is now in place. The mirror is made of pyrex glass, is 13 inches thick and weighs 5,000 lb. The mounting is similar to that of the Greenwich instrument, but, of course, on a very much larger scale. When completed the telescope will be the most powerful in the British Empire.

The advantages of aluminium-coated reflectors are emphasised by S. L. Boothroyd in the *Astrophysical Journal*, 1934, July. He lately organised a special expedition from Cornell University, equipped with a 15-inch aluminised mirror and quartz spectrograph, and obtained at Lowell Observatory and its mountain station 174 spectra of 97 typical stars in a total observing time of about 300 hours. For the most part the spectra are said to extend to the ultra-violet limit of atmospheric transmission. For permanence and average reflectivity aluminium is very superior to silver for coating mirrors, and its use is gradually being extended. The largest mirror yet aluminised is the 36-inch of the Lick Observatory.

PHYSICS. By L. F. Bates, Ph.D., D.Sc., F Inst P , University College, London.

New Conceptions in Magnetism.—It is convenient at this time to take stock of some new conceptions concerning magnetisation processes in ferromagnetic materials which have been developed during the last few years. The historical aspects of the development will not be considered here. It has long been the custom to regard a ferromagnetic body as consisting of a very large number of elementary regions, particles, Bezirke, or just elements, which are magnetised to saturation. It is imagined that, in the virgin, initial, or unmagnetised state of the substance, these elements are so arranged that the body possesses no resultant magnetic moment. If now the body is exposed to an external magnetic field, what happens inside the body ? In answering this question the idiosyncrasies of the material, e g. iron or nickel, will for the moment be disregarded.

At least two distinct processes occur. These are respectively termed Umklappprozesse and Drehprozesse in the German literature ; here they will be termed reversal and rotation processes. (In the writer's opinion, considerable confusion is caused by what appears to him to be the indiscriminate use of the first term, particularly by one German writer of repute.) Thus, if attention is fixed upon one particular magnetic element, when it is subject to the first process its magnetic vector simply turns completely through 180°, while if the second process is effective, the magnetic vector is orientated or rotated through an angle which may be 180°. The two processes may be regarded as independent of each other, and the writer personally pictures the first process as a sudden reversal of the spins of a number of spinning electrons, and the second process as a rotation of the element such as might be imagined to occur when the body containing it is deformed.

Now, what experimental evidence is there that these two processes exist? In the first place it has long been known that it is possible by suitable mechanical and thermal treatment to produce a nickel wire which will give a magnetic hysteresis curve which is more or less rectangular. The explanation for this special shape is that the magnetic elements have all been arranged with their magnetic vectors parallel to the axis of the wire, some of course being positively directed and others negatively directed parallel to the axis to produce an initially unmagnetised specimen. Consequently, Gerlach suggests that when an external field is applied only reversals may occur, and these can only give the sudden changes in the intensity of magnetisation shown by the rectangular curve.

Further evidence is provided by work on the changes in the electrical resistance of a ferromagnetic wire in a magnetic field. Taking first the case of a longitudinal field, it has been shown that only rotations, and not reversals, produce the observed changes in resistance. This was excellently illustrated by an experiment by Englert on a strongly stretched wire, composed of 92 per cent iron and 8 per cent nickel, when placed in a longitudinal field. The effect of heavy stretching was to arrange all the magnetic elements with their axes parallel to that of the wire. When the magnetic hysteresis curve was rectangular no changes in resistance with applied field were recorded. Again, with a wire in which the axes of the magnetic elements were parallel to the axis of the wire, a transverse magnetic field could only produce rotations through 90°, and consequently large changes in resistance. This prediction was also verified with the iron-nickel alloy wire mentioned above.

To avoid confusion, it is necessary at once to add that when a pure nickel wire is heavily stretched, the magnetic elements set with their axes perpendicular to that of the wire. Indeed, one of the most striking of modern experiments on magnetic hysteresis may be carried out with a pure nickel wire in a longitudinal field. Suppose that the descending branch of the hysteresis cycle is first taken with the wire under light tension; the well-known curve is obtained. As the tension is increased the shape of the curve profoundly changes, the maximum magnetisation decreases and the curve flattens, until, finally, under very heavy tension, the curve becomes a straight line passing through the origin. This result, which was predicted on theoretical grounds by Becker, is a most striking example of the change of direction of magnetic vectors when a substance is strained. Nickel is the only ferromagnetic substance known to behave in this particular way, *i.e.* with the magnetic axes setting perpendicular to the axis of the wire.

It is obvious from the last experiment that under certain conditions rotations may be reversible in the thermodynamic sense, for only rotations can account for longitudinal magnetisation when the wire is under heavy tension, and the hysteresis curve is a straight line These rotations have been specially investigated by Becker and Kornezki (*Zeit. für Phys.*, **88**, 634, 1934) in a paper on magneto-elastic torsion experiments. They point out that in substances which form cubic crystals the magnetic vectors either lie parallel to the crystal edges, as in iron, or parallel to the cube diagonals, as in nickel. Thus, in the case of iron the six directions parallel to the edges of the cube are equally probable, so that in an unmagnetised and undistorted crystal the magnetic elements are on the average equally distributed in these six directions. This distribution is, however, upset if the substance is exposed to elastic deformations. Certain directions then become more favourable, according to the type of magnetostriction which the substance exhibits.

It is therefore clear that an elastic deformation may have definite directional effects upon the magnetisation, even if the former is too small markedly to force the latter out of alinement with the sides or diagonals of the cube. The effects undoubtedly consist in the turning of magnetic vectors through 90° from one direction to an adjacent one, and these rotations may be reversible displacements of the confines of the element or irreversible displacements with which the Barkhausen effect is associated. Now, these effects cannot be directly investigated, for, although a given direction may become more favourable, the new elements which set along it will do so in such a way that there is no resultant magnetic moment. On the other hand, these 90° rotations must produce magnetostriction and magneto-resistance changes. The latter have been investigated by McKeehan, Gerlach and others, and it is found that it is always possible to obtain the same change of resistance either by tension or by a magnetic field as long as the magnitude and direction of the field is so chosen that it produces the same directional effect on the elements as the extension produces.

The new experiments of Becker and Kornezki deal with changes in the purely mechanical properties which are occasioned by directional effects produced by strain. For example, they deal with the residual deformations which follow the torsion of annealed wires of pure iron and of nickel, and they show that they can be destroyed by applying a longitudinal magnetic field or by demagnetising the wires by the passage of an alternating current. Again, a remanent deformation is practically the same as the twist which is set up by a magnetic field of screw form, which may be produced by sending a current through the wire so that the field it produces at the surface of the wire is equal in intensity to the longitudinal field in which the wire is placed.

On plotting a graph of angle of torsion against applied torque with increasing and decreasing values, a curve which encloses an area is in general obtained. This curve may be closed, *i.e.* the area becomes zero, when a suitable magnetic field is applied to the wire. The field merely annuls the directional effects produced by the torsion. Perhaps the most striking of these experiments is the effect of a longitudinal magnetic field on the damping of the torsional oscillations of a wire When a pure iron wire performs torsional oscillations in the absence of a field they are heavily damped, because the curve just discussed encloses an area. When a suitable longitudinal field is applied, the damping can be reduced by a factor of ten, simply because of the reduction in the energy dissipated by the irreversible magnetic processes. We thus see that the new conceptions provide adequate explanations for some striking, even if only qualitative, experiments.

PLEOCHROIC HALOES.—A pleochroic halo consists of a minute discoloration occurring in minerals like mica, and, as Joly first showed in 1907, forms one of the most interesting manifestations of radioactivity It is caused by the presence of a tiny particle of radioactive matter, originally uranium, in the middle of a discoloured sphere, so that a section through a halo consists of a series of concentric dark rings whose radii are determined by ranges of the α-particles released from the radioactive centre during the course of millions of years.

It has just been stated that the centre was originally a speck of uranium, and this view is generally accepted, but in the course of time the uranium has decayed and we now have an equilibrium system of radioactive elements present in the centre, and the different elements of the radioactive family give rise to their several rings. Thus, in addition to providing an extraordinarily delicate test for the presence of radioactive bodies, pleochroic haloes may be used to obtain information concerning the age of the minerals in which they are found, and, it is sometimes even hoped that they may provide evidence of the existence of elements now vanished from the face of the earth.

Consequently, it is of interest that a fresh quantitative study of these haloes has been undertaken by G. H. Henderson and his collaborators S. Bateson and L. G. Turnbull (*Proc. Roy. Soc.*, **145**, 563, 1934), who have used a special halo photometer. The halo section to be examined is mounted on a slide upon the stage of a microscope, and by means of a motor head-lamp and condenser lens it is arranged that an image of about 1·4 cm. diameter, *i.e.* magnified some 200 times, is projected on to a slit about 1·8 × 0·05 mm. Light from a small section of the image is thus allowed to pass through the slit into a lens which focuses it on a vacuum cæsium photoelectric cell. The resulting photoelectric current is amplified and supplied to a self-recording d'Arsonval galvanometer. The slide is slowly and regularly moved across the microscope stage, and a complete photographic record or photometer curve is obtained in ten minutes.

Henderson has put forward a fairly definite picture of halo formation and has confirmed it quantitatively with the new instrument, the ranges of all the α-particles, including those from U I, fitting the scheme very satisfactorily. The photometer curves were compared with integrated ionisation curves obtained from the well-known Bragg ionisation curves for simple radioactive substances emitting α-particles. Experiments were made on the artificial blackening of micas by exposing them to radium emanation and taking photometer measurements at intervals during the course of the exposure. In this way curves of the intensity of discoloration with duration of exposure were obtained and were used as standards for comparison with the halo photometer curves, and, consequently, to obtain relative measurements of the intensity of α-ray action suffered by various portions of the halo.

It is thus found that the halo discoloration can be fully accounted for by an ionisation process in the mica, or at any rate by a process akin to ionisation, of which it is not at present possible to describe the exact nature. No rings corresponding to thorium α-rays or to α-rays of exceptionally long range were found in the haloes in specimens of two biotites, one from Mount Apatite, Maine, and the other from Murray Bay, but evidence of α-particles of unusually short range was found in the latter specimen. In the halo nucleus the ring corresponding to the 5·50 cm. α-rays from Ac C was recorded for the first time ; it was quite definite and had a radius of 27·1 μ.

In the case of small flakes of mica from granites, which were split with sharp tungsten needles, it was difficult to get clear central sections free of flaws. However, occasional haloes were obtained in a light brown biotite from Egansville, Renfrew Co., Ontario, which showed traces of thorium rings. In addition, haloes were found with ring radii corresponding to α-rays of range 1·74 and 1·05 cm., the latter measurement agreeing well with a ring previously noted by Joly and by Iimori and Yoshimura. It is hinted that this ring may correspond to α-rays of 1·13 ± 0·04 cm. range found by von Hevesy to arise from samarium, but further work on these small rings is necessary before their origin can be stated.

The new work on pleochroic haloes provides the possibility of a new method for the determination of the age of the earth. The method so far most frequently used for an estimation of the age of a mineral is the radioactive one based on a measurement of the uranium-lead ratio in the mineral. Obviously, this method is open to error from the introduction of lead from other sources. Henderson now suggests that as actinium is found in the haloes the ratio Ac C – Ra C′ may be found, and, since this is equal to the ratio Ac D – Ra C_1, the age of the earth can be calculated from the appropriate formula. The Ac C – Ra C′ ratio can be found from the calculation of the changes in ionisation effect along a radius of a halo produced by the addition of given percentages of actinium to the centre, and by comparing the results with the photometer record of an actual halo containing actinium. This has been done for the three biotites whose properties have just been described, and the ages of the Renfrew, Murray Bay and Mount Apatite biotites were thus found to be 800, 750 and 400 million years respectively, these values being in agreement with the geological evidence. Since, however, the above calculations are based on assumptions concerning the period of Ac U and concerning the branching ratio Ac U to U I atoms, following the accepted theory of the origin of the actinium series from the parent actinouranium, Ac U, an isotope of uranium, the new method has not yet provided its final results.

The Constitution of Water.—The application of the Raman spectrum technique to the study of the constitution of water has been attempted by several workers, and a particularly wide investigation is described by I. R. Rao (*Proc. Roy. Soc.*, **145**, 489, 1934) who has measured the Raman bands for ice and water at 0°, 4°, 38° and 98° C., and their intensity distribution with respect to wavelength.

In the measurements on ice, long rectangular blocks were placed in a double-walled wooden box, open at the top, with sawdust between the walls. Light from a mercury arc was condensed on to the block by a cylindrical tube of pyrex, 5 cm. in diameter, containing water. The scattered light was collected and focused on the slit of a spectrometer in the usual manner, and exposures of 10 hours were sufficient to give a photographic record just good enough for microphotometry.

In the case of liquid water the usual horn-shaped Wood's tube made of quartz was used, and the liquid was directly illuminated by a mercury arc placed above the quartz tube. Suitable cooling attachments were provided for the lower temperature measurements, and for the high temperature measurements heating was arranged by a nichrome wire wound round the quartz tube. The Raman spectrum of water vapour was not studied as the results of Daure and Kastler (*Comp. Rend.*, **192,** 1721, 1931) were available and are incorporated in the discussion.

The Raman band for ice consists of two components, the one on the short-wave side being the sharper and more intense, while water at 0° also gives two components which, however, are not so clearly resolved as in ice, the long-wave component now being the sharper, and both are shifted slightly towards the region of long wavelength. There is no noticeable difference between the spectra for water at 0° C. and water at 4° C., except a slight decrease in the intensity of the short-wave component, a decrease which becomes very pronounced at 38° C. The appearance of the band for water at 98° C. is markedly different, there now being a single pronounced maximum whose position is shifted towards the long-wave side.

The bands may be expressed in terms of infra-red wavelengths as follows—ice, 3·01 and 3·13 μ ; water at 0°, 2·86 and 3·01 μ ; water at 38°, 2·86 and 3·02 μ ; water at 98°, 2·89 μ ; water vapour, 2·74 μ. The relative intensities were found by a method previously described (*Proc. Roy. Acad. Sci. Amsterdam*, **33,** 638, 1930).

Rao explains the above changes in frequency and intensity on the assumption that three types of molecules arising from the association of single molecules of water are present. These are H_2O, $(H_2O)_2$ and $(H_2O)_3$, and are supposed respectively to give rise to the Raman lines 3582, 3435 and 3216 cm^{-1}. Changes in their relative proportions with rise in temperature and change of state are assumed to determine the observed changes in the Raman spectra. On this basis Rao calculates the proportions in which the three molecular types occur in the different forms of water. His conception may be contrasted with the early picture given by Sutherland (*Phil. Mag.*, **50,** 460, 1900) and recently amplified (*Phys. Rev.* **41,** 310, 1932) ; Sutherland assumes that there are no single molecules present in water.

Rao's analysis leads to the view that the number of single or so-called "vapour" molecules progressively increases with rise in temperature, while the number of double or "water" molecules remains practically constant except in the change of state at 0° C. where there is a sudden increase ; this number appears to be a maximum at 4° C. The triple or "ice" molecules show a sudden decrease in the transition from ice to water at 0°, after which they gradually decrease in number with rise in temperature. In Rao's view the explanation of the maximum density of water at 4° C. lies in the superposition of two effects, *viz.*, the thermal expansion with temperature and the change in the relative proportions of the three types of molecules. The latter change really reduces to a change from higher to lower triple molecule content, *i.e.* to a closer packing of the molecules and, therefore, to a higher density. The two effects work in opposite directions and presumably 4° C. is the temperature at which they give conditions for densest packing.

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

ONE of the most important advances in meteorology made during the last two or three decades has been in our knowledge of the physical and chemical states of the upper atmosphere from 10 to 200 km. above sea level Of that region a small portion, extending to about 20 km , has been sounded frequently with the aid of self-registering instruments carried by small free unmanned balloons. Pressure, temperature, and—except at very low temperatures—humidity, have generally been the three quantities measured. The pioneer work was done on the Continent, but in this country Mr. W. H Dines soon followed with an apparatus of his own design of such light weight that a smaller balloon could be used, with the important result that the expenses of a sounding were made very small, and exploration could be carried on in many parts of the world, with only a small outlay.

The exploration of this lower layer has revealed many curious features of the atmosphere. Foremost of these is the existence of a region of continuous, or almost continuous, decrease of temperature with height, which has been called the troposphere. The old-established rule that temperature falls about 3° F. for every thousand feet in the vertical applies roughly to this region in many parts of the world when averages over a long period are considered. Most of the clouds commonly observed, even the high feathery cloud known as Cirrus, float within the troposphere, and the precise rate at which temperature decreases with height is intimately connected with the type of cloud that can form : a rapid rate favours development of cumulus clouds which may grow into the shower cloud (cumulonimbus) and even into the thunder-cloud when enough moisture is present. A check in the rate of decrease, or its reversal giving what is known as an inversion of temperature, is often associated with stratified clouds (stratus or strato-cumulus), and when an inversion is present on the ground or on the sea, fog is often present.

The troposphere is bounded at the top by a horizontal or only slightly inclined surface known as the tropo-pause. Above that surface temperature changes little with height ; there the region known as the stratosphere begins, and extends upwards for several kilometres. The thickness of the troposphere is greatest in low latitudes ; it is generally greater above regions of high barometric pressure than over low pressure ; it is on an average greater in summer than in winter in all places where the average temperature differs greatly in the two seasons. A consequence of the greater thickness of the troposphere over the equator than near the poles (about 16 as compared with perhaps about 8 km. on an average), is that if you go far enough up, the high temperature of the earth's surface near the equator is more than off-set by the greater fall of temperature there , for at the equator the temperature will normally be falling continuously between about 8 and 16 km. while near the pole the whole of that region will normally lie in the stratosphere so that it will be no colder there at 16 than at 8 km. One of the lowest temperatures ever recorded in the earth's atmosphere is 182° A. (about 164 degrees of frost on the Fahrenheit scale), at a height of 16½ km. above Batavia ; in tropical India comparable readings have been obtained. Polar regions have produced no such cold. Readings up to 16 km. have now been obtained at a sufficient number of places to make it possible to sketch roughly the average distribution of temperature up to that height on a diagram representing the earth's atmosphere in a section extending along a meridian from pole to pole ; a diagram of that kind is included in Sir Napier Shaw's recently completed *Manual of Meteorology*.

Leaving the region up to 20 km. and proceeding to still greater heights, we come to a layer extending from about 20 to 30 km. above sea level where certain iridescent clouds are occasionally observed. These have been specially studied in recent years by Störmer. Iridescence, according to present theory, is due to diffraction by water drops ; since it is difficult to believe that the super-cooling of water can often, if ever, be carried to the extremely low temperatures to be found in the stratosphere, the existence of these clouds suggests that the temperature is probably higher in the region where they occur than it is lower down in the stratosphere. That this is almost certainly the case higher up is strongly suggested by researches along two entirely separate lines. One of these is the study of the audibility of sounds on the earth's surface, particularly the audibility of loud explosions such as that at Silvertown on January 19, 1917. It is believed that the fall of temperature with height in the troposphere causes the sound waves to be bent upwards, that they straighten on entering the stratosphere, and that only the presence of much warmer air higher up accounts for the fact that they do eventually return to earth, the result being the frequently observed phenomenon of a region of audibility for many miles around an explosion, then a zone in which the explosion is not heard, and still farther away—perhaps a hundred miles from the explosion—another zone of audibility. The observations indicate that the sound waves reach heights of 40 km. or more, and this suggests that the 20–30 km. layer may be one in which temperature increases considerably with height The second indication of a warm layer somewhere above the 20-km. level comes from an interpretation of the observed behaviour of meteors The meteor, or shooting star, is in the first instance a solid mass that enters the highest regions of the atmosphere from outer space with a speed generally to be measured in kilometres per second ; it soon becomes incandescent, and may leave a luminous trail that persists for several minutes. The position of such a trail can be observed among the constellations, and if this position is carefully noted by observers many miles apart the height of the trail can be calculated. Most meteors become luminous at a height between 100 and 150 km. above sea level and generally disappear before falling below 80 km., but many can be followed below 50 km. ; some of the largest even reach the ground. According to the theory worked out by Lindemann and Dobson the behaviour of meteors, in particular their rate of travel, their luminosity, and their length of path, furnish information about the density and temperature of the air through which they pass during the period that they can be followed. The calculations confirm the presence of a warm layer at great heights. In a recent paper (*Q. J. Met. Soc.*, July 1934, **60**, No. 256, 278) Dobson and Meetham refer to this layer, which can be regarded as two layers if we take into account the probability that the lower part (about 30–50 km.) owes its high temperature to the absorption of very short wavelengths in the solar spectrum by ozone (O_3) and the part above 50 km. by absorption of even shorter wavelengths by oxygen (O_2), the waves in the latter case being shorter than 0·26 μ, or 2600 Ångström units. In that paper it is stated that calculations based on the distribution of ozone in the upper atmosphere made by Gowan will need to be repeated in view of the more exact information about the ozone obtained in recent years by Dobson and others, who have used spectroscopic methods believed to give reliable figures. There is of course some uncertainty as to the average distribution of temperature between, say, 25 and 200 km., but there is some ground for the belief that 300° A. is reached at 40 km., and that it is as warm or even warmer throughout most of the higher levels up to the height of 200 km., where many meteor trails first become visible.

The presence of this warm layer raises the question as to the amount of water-vapour that may be present at heights of 20 or more km. Not many years ago, when all that was known was that the temperature was very low in the stratosphere, and that it changes very little vertically in the stratosphere, it seemed safe to assume that the amount of water vapour above the tropopause is negligible. That assumption can no longer be made. Here is one of the most interesting problems awaiting solution, but like many others relating to regions so high that atmospheric pressure is extremely small, its solution may possibly prove of little help in understanding ordinary weather changes, which last appear to be conditioned largely by events taking place in the comparatively well-explored region up to 20 km.

BIOCHEMISTRY.

By W. O. Kermack, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

Transport of Carbon Dioxide. Carbonic Anhydrase.—One of the more important functions of the blood is the transport of carbon dioxide from the tissues, where it is formed, to the lungs so that it may there be eliminated in the expired air. At first sight the problem would appear to be much simpler than the analogous one of the transfer of oxygen by the blood, for carbon dioxide is not only more soluble in water than oxygen but also is capable of forming highly soluble and easily decomposed compounds, namely the inorganic carbonates, which would appear to be an ideally suitable form in which the transport of carbon dioxide could take place, and indeed it was early assumed that the transport of carbon dioxide in the blood takes place in the form of sodium bicarbonate. But an alternative hypothesis was also put forward, namely, that the carbon dioxide yielded with the blood proteins, and more particularly with hæmoglobin, a readily formed and easily dissociated compound behaving in respect of carbon dioxide in much the same way as oxyhæmoglobin in respect of oxygen. The extensive work of Van Slyke about ten years ago on bicarbonate-carbonate-carbon dioxide equilibrium in blood appeared to show that the carbonate-bicarbonate system could account for all the carbon dioxide taken up and that there was no need to bring in a carbon dioxide-protein compound of the type postulated. But the work carried on up to this time had been under equilibrium conditions of the system, and when Henriques investigated the dynamics of the reaction, and, more specifically, the rate of liberation of carbon dioxide when the *p*H was reduced, he was led to a very different conclusion. The dynamics of the purely inorganic system, bicarbonate-carbonic acid-carbon dioxide had already been investigated by Faurholt in 1924, and Henriques showed conclusively that in the case of blood, liberation of carbon dioxide could take place with a speed far greater than that which was observed in simple aqueous solution. Henriques recognised that there were two possible explanations of this result; either blood contained an enzyme which catalysed the reaction $H_2CO_3 \rightarrow CO_2 + H_2O$ (I), or else a large part of the carbon dioxide was taken up by the blood to form a loosely bound and rapidly dissociable protein compound. He himself was strongly in favour of this latter explanation and he named the hypothetical protein compound carbhæmoglobin. His experimental results were fully confirmed by Van Slyke and Hawkins (*J. Biol. Chem.*, 1930, **87,** 265), but these latter came to the entirely opposite conclusion as to the explanation of the observations. They considered that all the experimental results were consistent with the view that blood cells contained an enzyme which catalysed the liberation of carbon dioxide from carbonate, and showed that the addition to an inorganic system of red blood cells, in an amount equal to one-twentieth of that present in blood, was sufficient to increase the rate of liberation of carbon dioxide to such an extent as to be practically identical with that in blood itself. Henriques (*J. Biol. Chem.*, 1931, **92,** 1), however, did not consider these results conclusive, and believed that the importance of carbhæmoglobin in carbon dioxide transport had not been disproved.

The whole subject has been studied recently by Meldrum and Roughton who have now published two important papers (*J. Physiol.*, 1933, **80,** 113 and 143) which very materially advance our knowledge of the subject. In a short preliminary communication (*J. Physiol.*, 1932, **75,** 3P, 15P), these authors have shown that they could isolate from the blood cells an enzyme which had this specific effect of increasing the rate of reaction (I) as well as the reverse reaction, $H_2O + CO_2 \rightarrow H_2CO_3$ (II), and proposed as a name for this enzyme, carbonic anhydrase. The same enzyme was discovered independently by Stadie and O'Brien (*J. Biol. Chem.*, 1933, **103,** 521), who separated it from red blood cells sufficiently completely to enable them to conclude that the substance in question was neither hæmoglobin nor globin but a distinct enzyme. They also observed its presence in muscle. The first of the two papers now published by Meldrum and Roughton gives a full account of the purification and properties of this interesting new enzyme.

After a rough purification by removal of the hæmoglobin by recognised methods, further purification was effected by means of dialysis and adsorption with suitable reagents. The final solutions were colourless and removal of the water at a low temperature yielded a glassy solid which contained the enzyme in very concentrated form. A solution containing one part of this solid in 7,000,000 parts of water doubled the speed of reaction (I). This very high activity is comparable with those shown by the most carefully purified samples of enzymes so far prepared, and therefore, although the possibilities of the adsorption method have not been fully exhausted, it is perhaps doubtful whether any further substantial increase in activity will be effected. Detailed chemical examination of the enzyme has, however, been deferred until these attempts to effect further purification and possible crystallisation have been completed, but the evidence at present available suggests that the substance is protein in nature and probably a protein of relatively low molecular weight It has all the ordinary properties of an enzyme. It is rapidly inactivated by heat and its action is inhibited by recognised enzyme poisons, such as cyanide, carbon monoxide, urethane and a variety of other compounds. Aqueous solutions slowly lose their activity on standing ; for example, a solution lost 20 per cent of its activity in five weeks—but the solid material is more stable.

One of the most interesting properties of the enzyme is that it catalyses equally well both reactions (I) and (II). Theoretically, all enzymes ought to catalyse the reverse reaction , lipase, for example, should catalyse the synthesis of fats from glycerol and fatty acids as well as their hydrolytic fission, but in practice the reverse reaction is often very difficult to demonstrate. In the present case, however, the theoretical expectation is realised without difficulty, and it has been shown that the hydration of carbon dioxide is accelerated as well as the dehydration of carbonic acid. This satisfactory result is probably dependent on the fact that reaction (I) is really a true reversible reaction with a real point of equilibrium. Another interesting observation of Meldrum and Roughton is that a number of chemical processes which have as one link the chemical reaction (I), (II) or $HCO'_3 \rightleftharpoons CO_2 + OH'$ are also catalysed by the enzyme. For example, the solution of calcium carbonate by weak acids, or the uptake of carbon dioxide by ammonia, are catalysed by the enzyme.

The work just described appears to place beyond doubt the existence in the red blood cells of an enzyme such as that postulated by Van Slyke and Hawkins, but it is realised by Meldrum and

Roughton that they do not exclude the possibility that carbon dioxide-protein compounds may also be formed in the blood, in accordance with the view tenaciously held by Henriques. They discuss the question fully in the second paper and bring forward a considerable body of evidence which appears to demonstrate, beyond reasonable doubt, that such compounds are formed. The proof depends on the observation mentioned above that carbonic anhydrase is poisoned by cyanide. They find that, in blood treated with cyanide, the loss of carbon dioxide occurs in two phases There is a first phase lasting a few seconds in which the evolution is very rapid, and this is followed by a second phase of slow evolution corresponding to the ordinary evolution of carbon dioxide from carbonate. A similar bi-phasic reaction had been previously observed by Henriques working with a solution of hæmoglobin, and it was indeed upon this fact that he argued for the existence of carbhæmoglobin ; but prior to the work of Meldrum and Roughton it was not altogether clear to what extent Henriques's results could be explained away on the assumption of the existence of an enzyme. Now that the enzyme has been isolated and its properties ascertained, it has been possible to obtain clear evidence that the bi-phasic reaction appears even when the action of the enzyme has been eliminated. It is probable that the hæmoglobin solutions used by Henriques happened to have been prepared in such a way as to contain little or no enzyme.

Henriques had suggested that the protein-carbon dioxide compounds were probably of the nature of carbamic acids, which are formed by the reaction of carbon dioxide with an amino group according to the equation $PNH_2 + CO_2 = PNHCOOH$. Meldrum and Roughton share this view and bring a considerable amount of evidence to support it. They show, for instance, that amino acids and polypeptides form carbamic acid compounds with properties similar to those of the carbon dioxide-protein complexes in blood, and also obtained direct chemical evidence for the existence of part of the carbon dioxide in a form yielding a soluble barium salt. It would seem that, of the blood proteins, it is only the hæmoglobin which under physiological conditions can combine with carbon dioxide to any appreciable extent. Furthermore, reduced hæmoglobin has a considerably greater combining capacity than oxyhæmoglobin. At 15° C. and 65 mm. pressure, 9·5 vols per cent. CO_2 combine with reduced hæmoglobin while 4·9 vols per cent are combined with oxyhæmoglobin. At 37°, the corresponding figures would be about 5 vols per cent for reduced hæmoglobin and about 2 vols per cent for oxyhæmoglobin.

The latter figures are small compared to the total carbon dioxide content of the blood which is of the order of 50 vols per cent, and so it would appear that, in the warm-blooded animal, the transport of carbon dioxide is only to a small extent dependent on the formation of compounds of the type of carbhæmoglobin. It is possible, however, that these compounds are much more important in the physiology of cold-blooded animals.

A few sentences may be added on the distribution of carbonic anhydrase in nature. In mammals there seemed at first to be some connection between the enzyme and hæmoglobin, for it was found associated with the latter both in the red blood cells and in muscle tissue. However, it was shown by Brinkman Margeria, Meldrum and Roughton (*J. Physiol.*, 1932, **75**, 3P) that the ratio of the enzyme to hæmoglobin in the erythrocytes was far from constant. An extreme case of this is pointed out by Meldrum and Roughton in reference to the blood of fœtal goats. The hæmoglobin of the embryonic animal is almost devoid of enzyme activity until a short time before the end of term, when the content of carbonic anhydrase begins to increase. A survey of the distribution of carbonic anhydrase in a considerable number of lower marine animals has been made by Brinkman (*J. Physiol.*, 1933, **80**, 171). He finds there is absolutely no correlation between the presence of hæmoglobin or hæmoglobin-like compounds and of the enzyme, neither is the latter associated with catalase or peroxidase activity. The enzyme, as isolated from certain of these marine organisms, has similar properties to that obtained from mammalian erythrocytes. In view of the lack of any essential connection between the enzyme and hæmoglobin, it is perhaps very surprising that, in the blood of the higher animals, the enzyme should be found in the red blood cells and not in the plasma. The greater part of the carbonate in the blood is located in the plasma, and it seems curious that the enzyme is entirely confined to the cells.

SYMPLEX COMPOUNDS.—Willstatter and Rohdewald (*Z Physiol. Chem.*, 1934, **225**, 103) have recently published a paper which deals primarily with the constitution of glycogen in liver and other animal tissues, but in which some remarks are made which may prove to be very important. The glycogen of liver is shown to consist of two fractions, the first soluble and readily extracted by water and the second much less easily extractable and tenaciously held by protein. The first type is called lyo-glycogen and the second desmo-glycogen. The richer the liver is in glycogen the larger is the proportion of the former to the latter. In both types the glycogen appears to exist in association with protein, and this fact leads Willstatter and

Rohdewald to point out that such associations of two or more high-molecular compounds in nature appear to be a very common occurrence. Willstatter has for some years considered that enzymes consist of an active group, in which the characteristic activity of the enzyme is localised, associated with a carrier, conceived of as a colloidal particle, on which the active centre is borne and which is essential for its stability and effective action. Similar combinations of two or more substances are frequently encountered in the living organism, and, in the opinion of the authors, are sufficiently well marked out from other compounds as to form a distinct group of substances. Willstatter proposes to call them "symplex" compounds This name is to denote, "not only those compounds formed by the combination of a prosthetic group with a high-molecular (hochmolekulare) component, but also those formed from two or more high-molecular components." As examples of the latter may be mentioned the combination of antigen and antibody and toxin and anti-toxin so frequently encountered in the realm of immunology. Many antibodies themselves appear to consist of a non-colloidal haptene (*e.g.* the specific soluble carbohydrate of Avery and Heidelberger) combined with an antigenic protein and would presumably belong to the group. Willstatter and Rohdewald mention six characteristic features of "symplex" compounds by means of which they may be characterised ; (i) increase of specific reactivity of a component, (ii) alteration of solubility of a component, (iii) alteration of optical properties, (iv) alteration in stability, (v) alteration in toxicity and (vi) alteration of reactions, *e.g* colour reactions. The combination of lignin and cellulose which occurs in wood seems also to belong to the group.

Two of the most interesting examples, however, are those related to the lyochrome and lipochrome pigments which recently have been arousing much interest. The yellow oxidation ferment of Warburg and Christian is fairly widely distributed in nature and appears to consist, like many enzymes, of a specific part associated with a colloidal carrier. The specific part appears to be closely related to, or identical with, the yellow pigments which have been found to exist in egg-white (ovoflavin), milk whey (lactoflavin), and certain animal organs and plants. Kuhn (*J. Chem. Ind.*, 1933, **52**, 981) considers that these flavin pigments are identical with vitamin B_2, and so it will be seen that these lyochrome pigments constitute a very interesting and important group of compounds. As an example of lipochromes (cf. Kuhn, Lederer and Deutsch, *Z. Physiol. Chem.*, 1933, **220**, 229) may be cited the blue pigment of the lobster which changes to the proverbial red on boiling. The essential pigment here is a carotenoid-like substance called astacin, $C_{40}H_{48}O_4$ mp. 241°, which is red in colour but which exists in the lobster (probably in the form of a simple ester) in association with a protein as a " symplex " compound. This compound is no longer red but happens to be blue. When the lobster is boiled the protein is denatured, its properties are altered and the astacin, freed from combination with protein, exhibits its original native colour and so the boiled lobster is red.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Sedimentation and Sedimentary Rocks.—While the subject of erosion does not strictly fall under sedimentation it is perhaps pertinent here to notice recent challenges to the almost universally accepted ideas of the cycle of erosion, and of insolation as a cause of rock weathering. C. H. Crickmay, in discussing " The Later Stages of the Cycle of Erosion " (*Geol Mag.*, LXX, 1933, 337–47), questions the reality of the late maturity and peneplain stages His criticism centres around the relative efficiency of general wasting of the surface, and of lateral corrasion, in producing graded plains. Crickmay contends that lateral corrasion is by far the more effective agent, and is responsible for the development of peneplains or *pan-plains*, as he prefers to call them These are developed by the confluence of flood-plains during the later stages of the cycle of erosion.

Similarly E. Blackwelder challenges the insolation hypothesis of rock weathering (*Amer. Journ. Sci.*, XXVI, 1933, 97–113), in which it is held that under favourable climatic conditions rocks which have been subjected to alternating stresses brought about by daily changes of temperature, are eventually broken down by splitting, exfoliation, and crumbling. Blackwelder reviews the question in some detail and thinks that the evidence in favour of the hypothesis is much less impressive, both in quantity and in weight, than the evidence against it.

The title of F. Zeuner's memoir, " Die Schotteranalyse. Ein Verfahren zur Untersuchung der Genese von Flussschottern " (*Geol. Rundsch.*, XXIV, 1933, 65–104) sufficiently indicates the nature of the work. The modes of sedimentation of subaerial deposits, especially sand and loess, are discussed by H. Klähn (*Zeit. d. Deutsch Geol. Ges.*, **85**, 1933, 14–45).

G. R. MacCarthy has studied the distribution of rounding in beach sands with somewhat anomalous results (*Amer. Journ. Sci.*, XXV, 1933, 205–24). A sorting action by waves and currents seems to take place whereby the smaller, less dense and more angular grains are concentrated at the lower levels of the beaches, whilst the larger, denser and more rounded grains lag behind, and are concentrated at higher levels.

An olivine-sand from Duntulm, Skye, has been described by F. Walker (*Trans. Geol. Soc. Edin.*, XII, 1932, 321); and an olivine-bearing conglomerate from Raasay, Skye, has been investigated by C. F. Davidson (*Geol. Mag.*, LXX, 1933, 272–4).

By means of a statistical study of clay and bauxite minerals on two triangular diagrams, and constructing frequency curves for the cardinal ratios, S. I. Tomkeieff (*Min. Mag.*, XXIII, 1933, 463–82) concludes that only pyrophyllite, kaolinite, diaspore and gibbsite, are definite mineral species. Termierite, anauxite, allophane, etc. are probably mineral species also, but the statistical evidence is not clear enough to establish them. All other clay minerals are mixtures or colloids of variable composition.

J. de Lapparent (*C.R. Acad Sci., Paris*, **198**, 1934, 669–71) has shown by investigation of the clays interbedded with the emery of Samos that the clay mineral leverriérite, which Ross and Kerr had declared to be identical with kaolinite, is really a mixture of muscovite and kaolinite. The same author, in a note on " Classification des argiles sédimentaires " (*C.R. Acad. Sci., Paris*, **195**, 1932, 257) distinguishes between halloysite-clays, montmorillonite-clays, and kaolinite-clays.

A statistical study of the " Textural and Lithological Variations in Glacial Till," by W. C Krumbein (*Journ. Geol.*, XLI, 1933, 382–408) based on material collected from the region south of Lake Michigan, shows that till is not so heterogeneous as formerly supposed, and that a given ice sheet tends to produce material having a fairly well-defined frequency distribution. Hence, under favourable conditions, mechanical composition may be used as a criterion for distinguishing between different boulder clays.

In his paper on " The Glaciated Surfaces of Nooitgedacht near Kimberley, and the Upper Dwyka Boulder Shales of the Eastern Part of Griqualand West " (*Trans. Roy. Soc. S. Africa*, XX, Pt. 4, 1932, 301–25), G. Slater shows that this ancient glaciated area contributes much to knowledge of glacial erosion and sedimentation. Nooitgedacht shows fine crag-and-tail structures, while a section at De Kalk exhibits the typical sedimentation of glacial material in water, and tillite bands representing englacial material deposited from floating ice. Intraformational folding is well displayed in these deposits.

In a valuable paper on " The Properties of Coals as Determined by their Mode of Origin," Professor G. Hickling (*Journ. Inst. Fuel*, 1933, 11 pp. (reprint)) seeks to establish the conclusion that each coal bed at the time of its formation acquired certain chemical characters as the result of the nature of the original plant materials, and the degree of their disintegration and bacterial decay. These characters establish the " type " of coal formed, and have persisted through the subsequent changes of composition leading to advance in " rank," which latter is attributed to the action of pressure and moderate heating. Hickling considers that Hilt's law is amply confirmed by the evidence of the South Wales coalfield.

In three recent papers Professor G. Hickling and C. E. Marshall have studied the microstructures of coal formed from perfectly definite and uncontaminated parts of trees. They show (" The Preservation of Plant Tissues in Coal," *Proc. Univ. Durham Phil. Soc.*, IX, Pt. 2, 1933, 105–16) that vitrain results from the preservation of plant tissues in a fluid medium which allowed the cell walls to be compressed and contorted without fracture. Fusain, however, is essentially brittle, and the cell walls fracture as the cells are not filled with organic matter, the tissues being preserved in the dry state.

In a second paper (*Trans. Inst Min. Eng* , LXXXIV, Pt. 2, 1932, 13–23) the microstructures of the vitrain formed from fallen stumps and isolated branches of fossil Gymnospermous trees, are investigated. The vitrain was found to have been derived entirely from the wood of the stems. In the third paper (*ibid.*, LXXXVI, Pt. 2, 1933, 56–75) the investigation is extended to coals formed from fossil tree barks derived mainly from species of *Sigillaria* and *Bothrodendron.* This leads to a valuable discussion of the nature and origin of vitrain, and its relation to other forms of coal, and to an analysis of the use and significance of the term vitrain.

In a paper on " The Lithology and Palæobotany of Certain British Coals," R. Crookall (*Fuel in Science and Practice,* **12,** 1933, 276–9) comes to the conclusion that fragile coals (such as those of Kent) owe their fragility to the scarcity or absence of durain, and the high proportion of clarain and fusain, but in some instances an exceptional development of cleat is the dominating factor. Abundance of fusain lenticles indicates that masses of plant debris were exposed to subaerial decay owing to fall in level of swamp waters. Physiographic conditions prevailing over the area of deposition affected floral development and thus the lithological nature of the resulting coals.

H. R. J. Conacher records a number of native hydrocarbons such as chrismatite, zietrisikite, and albertite, with two other forms which resemble ixolite, from the oil-shales of the Lothians (*Trans. Edin. Geol. Soc.*, XIII, Pt. 1, 1934, 89–97).

In an important paper, "The Precipitation of Calcium Carbonate on the Great Bahama Bank," M. Black (*Geol. Mag.*, LXX, 1933, 455–66) shows that this great shoal region acts as a huge evaporating pan, drawing in water of normal salinity from the surrounding ocean, and concentrating it in the central parts of the bank. As evaporation is greatest in this central area the supply of water bringing lime salts is constant, and whatever the mode of precipitation, a permanent precipitate of calcium carbonate is to be expected. Other topics discussed are precipitation by plants and invertebrates, and biochemical precipitation from calcium sulphate in mangrove swamps

In a monumental work entitled "Die Rezenten Kalksteine" J. Pia (*Min. u. Petr. Mitt., Erg.-Bd.*, 1933, 420 pp.) classifies the recent limestones as (i) Biogenic, and (ii) Abiogenic Under the former heading he distinguishes between (*a*) Physiological Limestones, due to the vital activities of organisms, and (*b*) Organic Limestones, due to the accumulation of calcareous skeletal parts, shells, etc There follows a general description and discussion of present-day world-wide limestone formation, including a full account of the origin of coral reefs

The fifth part of A. Hadding's great work on the Pre-Quaternary Sedimentary Rocks of Sweden is entitled, "On the Organic Remains of the Limestones. A Short Review of the Limestone-forming Organisms" (*Lunds Univ Årsskrift N.F. Avd.* 2, **29**, Nr. 4, 1933, 93 pp), and deals with plants and animals as limestone formers preliminary to the full description of Swedish limestones which is promised in a further memoir. In this work the groups of plants and animals which contribute to the limestones are treated in relation to this function. It is also sought to determine in what degree inorganic precipitation, organic formation, and the deposition of detritus, have contributed to the origin of calcareous sediments.

Another large memoir on limestones which has reached us belatedly is P. H Derville's doctoral thesis on "Les Marbres du Calcaire Carbonifère en Bas-Boulonnais" (*Univ. Strasbourg*, O. Boehm, 1931, 322 pp., 24 pls.). These rocks are of Viséan age, and are mostly algal limestones, although foraminifera and cephalopods are occasionally important constituents. Other topics discussed are the recrystallisation of limestones, the formation of ooliths, and the origin of magnesian sediments.

In discussing "The Geologic Importance of the Lime-secreting Algæ" M. A. Howe (*U.S. Geol. Surv., Prof. Paper*, 170—*E*, 1932, 57–65) describes a new travertine-forming organism, *Lithomyxa* *calcigena*, which forms laminated calcareous pebbles and travertine crusts on the rocks in Furnace Creek, West Virginia.

In his paper on the " Origin of the Concretionary Structures of the Magnesian Limestone at Sunderland, England," W. A. Tarr (*Journ. Geol.*, XLI, 1933, 268–87) shows that after consolidation of the carbonate muds had occurred, the rocks were fractured and jointed, permitting the ready circulation of ground water. This resulted in recrystallisation of calcite adjacent to the divisional openings, and thus developed the columnar forms. The globular concretions of the dolomitic marls originated from similar growths within powdery dolomite.

In a discussion of the " Origin of the ' Beef ' in the Lias Shales of the Dorset Coast," the same author (*Geol. Mag.*, LXX, 1933, 289–94) advances the theory that the " beef " (fibrous calcite) is due to the leaching of calcium carbonate from fine porous marl by a ground water solution, and its subsequent deposition. The thin paper-shales which intervene between the layers of " beef " may represent concentrations of the original clay of the marls.

Discussing the origin of the " Spherulitic Ironstones of West Yorkshire," T. Deans (*Geol. Mag.*, LXXI, 1934, 49–65) suggests that the iron was precipitated in a colloidal condition during the final silting-up phase of the sedimentary rhythm of the Upper Carboniferous. He also offers a speculative explanation of spherulitic growth in the siderite.

A. Hadding discusses the origin and paragenesis of the ironstone-bearing rocks of south-eastern Scania, and institutes comparisons between the oolitic ores of the Jurassic in Lorraine, England, and Scania (*Sver. Geol. Undersök*, Ser. C, No. 376, Arsbok 27, 1933, No. 1, 31 pp.)

It is conjectured by E. A. Schiebe that the iron of the banded quartzose ferruginous slates and schists (itabirite) of Brazil has been derived from underlying and interbedded basic igneous rocks (*Zeits. d. Deutsch Geol. Ges.*, **84**, 1932, 36–47).

The borings of sediment-dwelling animals have been found preserved in flint by K. Gripp in the Cretaceous of Holstein (*Mitt. Min.-Geol. Staatsinst., Hamburg*, XIV, 1933, 23–40). This is held to prove that flint is due to replacement of chalk in Danian and Montian times, when chalk formed the land surface. Silicification is found to be greatest near the old land surface, and diminishes with depth.

E. H. Kranck describes some radiolarian rocks from Tierra del Fuego (*Bull. Soc. géol. France* (5), II, 1932, 275–83). These appear to belong to several distinct horizons, and are of two petrographic types, namely, cherts and fine-grained greywackes. The former have been deposited in deeps at some distance from a coast-line; the greywackes, rich in detrital matter, are of comparatively shallow water origin.

In his paper on "Relations of Cherts to Stylolites at Carthage, Missouri," E. S. Bastin (*Journ. Geol.*, XLI, 1933, 371–81) shows that the cherts have arisen largely by replacement of limestones, with frequent preservation of fossils and bedding. They alternate with stylolites along the stratification planes, and were mostly developed earlier than those structures.

J. H. Adamson and G. F. Wilson present the results of macroscopical and microscopical examination of the sediments of the Lower Carboniferous in north-east Ireland (*Proc. Roy. Irish Acad.*, XLI, Sect. B, No 14, 1933, 179–90). The fresh feldspars indicate mechanical weathering, and the rocks are shallow marine sediments probably derived from a land constituted by the metamorphic rocks of northern and north-western Ireland.

Exhaustive mineralogical and textural analyses of "The Permian Yellow Sands of North-East England" have been made by M. B. Hodge (*Proc. Univ. Durham Phil. Soc.*, VIII, Pt. 5, 1933, 1–49). The heavy minerals agree in species and type with those of the Scottish Carboniferous rocks. The appropriate palæogeographic inferences are drawn.

The memoir by R. Eigenfeld on "Die Kulmkonglomerate von Teuschnitz in Frankenwalde" (*Abh. Math.-Phys. Kl. Sächs. Akad. Wiss.*, XLII, No. 1, 1933, 64 pp.) is an outstanding example of the value of an exhaustive petrographical examination of a conglomerate horizon for stratigraphical purposes. The petrographical-geological analysis of this conglomerate has resulted in a much more accurate conception of the structural bodies and their temporal succession amongst the Pre-Variscian members of the Variscian orogen than was possible before.

METAMORPHISM AND METAMORPHIC ROCKS.—P. Eskola has further elaborated his conception of the origin of certain granites by differential anatexis (*C.R. Soc. géol. de Finlande*, No. 7, 1933, 12–25). He regards the deepseated zone of differential anatexis as possessing a temperature high enough to fuse the low-temperature (*i.e.* quasi-granitic) constituents of most rocks. Migmatites are especially characteristic of this zone. Above this zone the fused matter, together with juvenile magma, collects in plutonic intrusions associated with pegmatites and potassic metasomatism. The zone of igneous intrusion passes upward into a hydrothermal zone characterised mainly by quartz veins.

Dr. W. J. McCallien has made an interesting summary and analysis of the process of metamorphic diffusion (*C.R. Soc. géol. de Finlande*, No. 8, 1934, 1–19), which leads to metamorphic differentiation. The phenomena of porphyroblasts, quartz-, chlorite-, quartz-albite-, and quartz-kyanite-veins, and accentuations of junctions between differing rock types, are examples of pronounced metamorphic diffusion. The porphyroblastic albite-schists of the Scottish Highlands are discussed at length in this connection.

The quartz-kyanite rocks in Unst (Shetland) have been described by H. H. Read, and discussed in regard to their bearing on the problem of metamorphic differentiation (*Min. Mag.*, XXIII, 1933, 317–28). They occur as veins within the chloritoid-kyanite-schists of the Saxa Vord Group. Professor Read believes them to be due to a process of endogenous secretion during the period of metamorphism. The material of the quartz-kyanite veins represents the difference in composition between the normal country-rock and that immediately associated with the veins.

Professor K. H. Scheumann writes " Über die Bedeutung der mineralfaziellen Analyse für die Auffassung der metamorphe Gesteine " (*Ber d. Math.-Phys Kl d. Sächs Akad Wiss., Leipzig*, LXXXIV, 1932, 63–90). He deals with three main topics ; the mineral-facies mode of consideration, parallelism in mineral facies (between magmatic, perimagmatic, and telemagmatic processes), and correlation between magmatic and metamorphic processes.

In his paper on " The Genesis of Oligoclase in Certain Schists," F J. Turner (*Geol Mag* , LXX, 1933, 529–41) discusses mineralogical changes in green schists and related quartzo-feldspathic schists of sedimentary origin. He shows that while oligoclase normally appears in the almandine and higher metamorphic zones associated with deep green hornblende, it also occurs within the higher parts of the chlorite zone associated with pale aluminous hornblende.

A. G. Hutchison has described the metamorphism of the Deeside Limestone of Aberdeenshire (*Trans. Roy. Soc. Edin.*, LVII, Pt. 2, 1933, 557–92), which may be regarded as a feldspathic facies of the Loch Tay Limestone. In the south-western Highlands the Loch Tay Limestone lies between the chlorite and almandine zones ; on Deeside, however, it reaches the sillimanite zone. Some anomalous mineral assemblages are regarded as departures from sillimanite-grade metamorphism arising from local absence of the stress factor.

The petrology of the Hecla Hook Formation in Central Spitsbergen is described by P. E. Fairbairn (*Geol. Mag.*, LXX, 1933, 437–54). Rocks belonging to the western schist and gneiss facies of the Pre-Devonian series, and of the eastern slate and quartzite series of low metamorphic grade, are discussed. Unfortunately no further evidence was found as to the relationship between these two very different facies which come into close contact in the field.

C. A. Lamey has described the extensive contact-metamorphism which has been produced by the intrusion of the post-Upper Huronian Republic Granite of northern Michigan into quartzites, dolomites, iron-formations, slates, and greywackes (*Journ. Geol.*, XLII, 1934, 248–63) Widespread sericitisation has been caused in quartzites. Folding prior to, or possibly accompanying, the granitic intrusion has resulted in the presence of characteristic stress minerals such as staurolite and chloritoid

An exhaustive study of the contact-metamorphism of two slate formations by large granite masses and by the Duluth Gabbro has been conducted by F F. Grout (*Bull. Geol Soc. Amer.*, **44**, 1933, 989–1040) This work has brought out the important point that the two kinds of magma have produced very different metamorphic effects Granite changes the slates to biotite-schists of constant composition, with lit-par-lit injection and pronounced dynamic effects The gabbro tends to produce hornfelses devoid of notable schistosity but showing considerable metasomatic change.

C. E. Tilley and A. R. Alderman describe " Progressive Metasomatism in the Flint Nodules of the Scawt Hill Contact-Zone (*Min. Mag*, XXIII, 1934, 513–18) Zonotlite-wollastonite rocks and wollastonite-pyroxene-melilite rocks have been produced by increasing intensity of metasomatic alteration These changes, in which the characteristic forms of the flints have been perfectly preserved, are striking examples of replacements unaccompanied by volume change (Lindgren's rule of equality of volume)

Peculiar rocks produced by the action of burning oil and gas on marls in the Persian oil-fields have been described by W. F. P. McLintock (*Min. Mag.*, XXIII, 1932, 207–26). The initial effect of heat has been the production of diopside, ægirine, wollastonite, bytownite, and melilite, with residual calcite ; the final effect is the formation of abundant melilite and diopside. A novel discovery is the presence of pseudo-wollastonite in some of the more altered types which must have been subjected to rather high temperatures.

Dr. E. B. Knopf has performed an important service to geologists by setting out in discussion form the views of Professor B. Sander on " petrotectonics " (*Amer. Journ. Sci.*, XXV, 1933, 433–70). This is the study of the orientation of rock fabric ; its basis is that the characteristic motion accompanying the formation of rocks is recorded in their fabrics, under which term is comprised not only the *external* space relations of the constituent minerals (shape), but also the *internal* arrangements of their space-lattices (crystallographic orientation). This line of study can be made to yield much information on the conditions of deformation and metamorphism of rocks.

P. Bearth describes some interesting " gangmylonite " (or pseudotachylytes) which occur abundantly in the banded amphibolites of the Lower Engadine " window " in the Silvretta Decke (*Schweiz Min. u. Petr. Mitt*, XIII, 1933, 347–55).

The mylonites occurring in the crystalline rocks of Georgia are described and discussed by G. W Crickmay (*Amer. Journ. Sci.*, XXVI, 1933, 161–77) They usually appear along major thrust-faults, and locally have pseudo-eruptive relations to the parent rock which seems to have been a porphyritic rock of granitic composition. Mylonitisation occurred during the Appalachian orogeny at the close of the Palæozoic era

H. Ebert's memoir " Das Grundgebirge im Elbtale nördlich von Tetschen " (*Abh d Sächs. Geol Landesamts*, **14**, 1934, 78 pp.) is a stratigraphical investigation on metamorphic rocks, in which the principles of metamorphic petrology are effectively used. Interesting cataclastic phenomena in granite are described.

J. Gilluly believes that the albite-granite of Sparta, Oregon, and albite-granites in general, are of hydrothermal origin (*U S. Geol. Surv.*, *Prof. Paper* 175—*C*, 1933, 65–81). The features which have led to this conclusion are common association with crushing phenomena, widespread occurrence of graphic textures, epidote and other low-temperature hydrous mafic minerals, milky and rutilated quartz, and an unusual degree of replacement.

The mode of occurrence of myrmekite growths in a meta-toscanite from Victoria has led E S. Hills (*Geol. Mag.*, LXX, 1933, 294–301) to the conclusion that myrmekite is produced under physico-chemical conditions favouring the transfer of mineral matter in the solid state.

J. K. Gustafson describes " Metamorphism and Hydrothermal Alteration of the Homestake Gold-bearing Formation " (*Econ. Geol.*, XXVIII, 1933, 123–62). The Homestake Formation was a heterogeneous sandy, clayey, partly calcareous and ferromagnesian sediment. During regional metamorphism garnet, cummingtonite, biotite and chlorite were formed in the order given. Subsequent hydrothermal action introduced vein quartz, ankerite, a different variety of chlorite, and the ore minerals.

The " Hydrothermal Metamorphism of an Ultrabasic Intrusive at Schuyler, Virginia," in which the world's most important soapstone deposits were formed, is described by H. H. Hess (*Amer. Journ. Sci.*, XXVI, 1933, 377–408). In the rocks studied it was found that the lower-temperature mineral always replaced the higher. Talc, which replaces chlorite, is considered to be the low-temperature end-member of the series. These changes are consequently believed to have taken place under conditions of decreasing temperature. Dynamothermal metamorphism is shown to have played only a subordinate role in the formation of these rocks.

W. H. Wilcockson and W. H. Tyler describe an area of ultrabasic rocks in the Kassala Province of the Anglo-Egyptian Sudan (*Geol. Mag.*, LXX, 1933, 305–20). These consist mainly of serpentine and talc-carbonate rocks. Talc-magnesite rocks are believed to have been derived from serpentine by the action of carbon dioxide.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

Developmental Anatomy.—In most species of flowering plants in which the production of adventitious roots has been investigated they have been found to arise either from the cambium or from the pericyclic region In *Roripa austriaca*, however, the origin of the adventitious roots was traced, by Wilson, to epidermal and cortical cells. F. Wolfe (*Bot Gaz.*, XCV, 686, 1934) now reports that in *Cotoneaster Dammeri* the adventitious roots, which arise in an axillary position, develop from one of the two groups of parenchymatous cells in the divided bud-gap. The roots formed on cuttings of *Lonicera japonica* have been investigated by S. Sandison (*New Phyt.*, XXXIII, 211, 1934) who finds that these are dimorphic. Some of the adventitious roots which arise from pre-existing root initials are relatively thick unbranched structures which possess a medullated stele consisting of separate xylem strands. Other adventitious roots develop from the callus tissue and these compared with the others are thin, furthermore they are branched and contain a solid non-medullated xylem strand. Comparison is made with the normal dimorphism of the roots of *Fragaria* where branched and unbranched roots likewise occur and the branched roots have copious lignification.

As comparatively little is known regarding the earliest stages of leaf development, the paper by G. H. Smith on the anatomy of the embryonic leaf (*Amer. Jour. Bot.* **21**, 194, 1934) is a welcome addition to our knowledge. He finds that in all the species investigated the form of the adult leaf is assumed at a very early stage and that the embryonic leaf consists of from five to eight layers of cells characterised by their uniform size and regularity. From the middle layer provascular areas are formed by horizontal division of contiguous units. The whole tissue of the young leaf is meristimatic throughout and it is not until after the bud swells that differentiation, as regards rate of maturation, is recognisable. It would appear that extreme "shade-leaves" are not only relatively juvenile types of foliage as indicated by their adult morphology but that they are juvenile also in respect to retaining the same number of cell layers as the leaf rudiment. In the thicker "sun-leaves" the number of layers, however, undergoes increase.

The observations of Harvey-Gibson that two species of *Selaginella* possessed xylem consisting partly of true vessels has been found by Duerden to hold true for six other species. In these as in *S. oregana* and *S. rupestris* the xylem is stated to consist mainly of vessels. In *S. spinosa* the xylem is entirely tracheal and this is true also of all the heterophyllous species examined (*Ann. Bot.*, XLIII, 459, 1934).

ECOLOGY AND DISTRIBUTION.—A map of the former distribution of the Hornbeam as indicated by pollen analyses as well as its present distribution is furnished by Erdtman (*Svensk Bot. Tids.*, **28**, 359, 1934). The pollen occurrences indicate that *Carpinus betulus* may formerly have had a much more extended range than at the present day. The resemblance of the present distribution to that of the Beech is commented on, but whereas *Fagus* is regarded as having extended from the Mediterranean region the Hornbeam is held to have spread from the Balkans.

From a study of the two closely allied species of *Ajuga*, namely *Ajuga chamæpitys* and *Ajuga chia*, Dr. Turrill (*New Phyt.*, XXXIII, 218, 1934) concludes that there has been a differentiation from a common stock. *A. chamæpitys* is very uniform almost throughout its range which extends over western Eurasia whereas *A. chia* characterised by larger flowers and transversely corrugated seeds, is very polymorphic and has a considerable range over eastern Eurasia. In the areas of contact intermediate types occur.

The rapid spread of the introduced *Croton sparsiflorum* in India is described by A. C. Joshi (*Current Science*, II, 344, 1934). The species first appeared in Bengal about 1898 and since then has spread chiefly along river banks. By 1908 it had established itself about Calcutta and is now abundant in Bengal, Assam, and Orissa.

Experiments of ecological interest have been carried out by Clark and Shive on the absorption of ammonia nitrogen and nitrate nitrogen by the Tomato (*Soil Science*, XXXVII, 203, 1934). They find that a high *p*H favours absorption of ammonia nitrogen, this absorption having a value at *p*H 7 nearly double that at *p*H 4. On the other hand, the maximum rate of absorption of nitrate nitrogen was at *p*H 5 ; whilst at *p*H 7 this rate was approximately halved. But the effect of the reaction of the nutrient medium on the nitrate absorption was markedly less in older than in younger plants. Sudden changes of reaction in either direction appear to retard absorption, which effect may be partly responsible for the ameliorating influence of buffering substances such as organic material in soils liable to attain high acidity or alkalinity. In a further paper (*ibid.*, p. 459) it is shown by means of analyses that the conditions which favour the highest rates of absorption of ammonia nitrogen or nitrate nitrogen are also those which are most favourable to their assimilation.

An interesting study of the influence of light at the upper limit of forest in Switzerland is reported by P. Collaer in *Bull. Soc. Bot Suisse*, **43**, H 1, 1934. He finds that above 1,650 metres, *Picea excelsa*, when growing in full illumination, exhibits a diminution in the quantity of chlorophyll which it contains and a diminution of diastatic activity; the latter is attributed to the intensity of the violet and ultra-violet rays above this altitude, whilst the rays of the red and orange region are held responsible for the lowered chlorophyll content. *Larix decidua* above its limit at 1,950 metres shows no diastatic diminution but a reduction in chlorophyll content. *Pinus cembra* shows no diminution in either its chlorophyll content or diastatic activity up to the altitudinal limit of its occurrence. It is concluded that the upper limit of the Spruce is not determined by wind action but by light, and in support of this, attention is called to the rarity of evidence of wind injury of this tree whereas *Larix decidua* and *Pinus cembra* the limit of which is not determined by light frequently exhibit injury due to this cause.

From a study of the twelve species of *Cupressus* belonging to the section Eu-Cupressus, A. Joubert and P. A. Burollet conclude that, though at present exhibiting restricted areas of distribution in the warm temperate region of the northern hemisphere, the species of this genus had a much more extensive circumboreal range in Tertiary times. Human influence is held to be largely responsible for the diminution of the Mediterranean species which is essentially characteristic of progressive stages in the plant succession whereas the Fir (Abies) is a feature of retrogressive phases. The Cypress is regarded as a feature of the *Quercus Ilex* region and attention is called to the association of sclerophyllous species of Oak with species of Cypress not only in the Mediterranean but also in the Himalaya, in China and in North America (*Revue des Eaux et Forêts*, 1–30, 1934).

Data regarding the biology of *Cirsium arvense* are furnished by A. Hayden (*Amer. Jour. Bot.*, XXI, 354, 1934). Its functionally diœcious habit limits its spread by seed, since plants of only one sex may often be introduced as a weed, but its rapid vegetative spread diminishes the value of measures to prevent the plant fruiting. Effective pollination appears to occur in Iowa when plants of the two sexes are not separated by more than 200 feet, though from two to three seeds in a head were found where the sexes were separated by from 500 to 600 feet. Extension, probably vegetative, of from 20 to 40 feet in a season has been recorded. The roots penetrate to a depth of from 7 to 18 feet and buds develop either from the underground stems or from the numerous horizontal roots which extend considerable distances in the top 3 feet of soil. Fragments of root only $\frac{1}{8}$ inch long will develop into plants whilst each fruiting capitulum will form about 50 achenes and of these from 10 per cent to 95 per cent are viable From a few scattered locations in Iowa forty years ago *Cirsium arvense* has spread throughout the state, but accompanying maps show that even to-day the density of occurrence is greatest in the areas of its original settlement.

An account of the sublittoral ecology of a gully in South Devon is given by J. A. Kitchen, T. T. Macan and H C. Gilson in *Jour. Marine. Biol. Assn.*, XIX, 677, 1934 The investigation was carried out by means of a diving helmet. Two Algal Associations were recognised. The one dominated by large Brown algæ, chiefly *Laminaria Cloustoni*, *L. Saccharina*, *L. digitata*, *Hiamenthalia lorea* and *Saccorhiza bulbosa*, which occurred on all upward-facing rock surfaces. The undergrowth here consisted of an upper layer of *Rhodymenia palmata*, *Chrondrus crispus*, etc., a middle layer of Corallina and a basal layer of animals. The light intensity below the Laminaria fronds was from 0·6 per cent to 1 per cent of that in the air above, whilst above the fronds the illumination in the water was 5·3 per cent. The second association was found on the vertical and overhanging rock surfaces. Here the larger brown algæ are apparently excluded by some factor other than inadequate illumination and Red algæ especially *Myriogramme Bonnemaisoni*, *Rhodomenia Palmetta*, and *Lomentaria articulata* are characteristic. In this community the dominant life is not algæ but the sponge *Halichondria* and the Tunicate *Distomus*. In the same journal H. W. Harvey (p. 775) furnishes quantitative data with regard to the amount of phytoplankton present in the sea which tend to show that from April to July the amount present is regulated by the intensity of " grazing " by herbivorous zooplankton. An August minimum is coincident with an almost complete absence of dissolved phosphate.

The Plumbaginaceæ present various abnormal features in the development of the embryo-sac to the understanding of which the condition in *Plumbago,* described by A. W. Haupt, contributes (*Bot. Gaz.*, XCV, 686, 1934). The single ovule in *Plumbago capensis* is of cauline origin, being derived from the apical meristem. The archisporium consists of one or two cells and after division two layers of parietal tissue are formed, a feature of some interest in view of the usual absence of this tissue in members of the Sympetalæ The megaspore mother cell gives rise to the embryo-sac directly and an eight nucleate stage is developed, but the mature condition normally exhibits one large egg cell and a primary endosperm nucleus derived by the fusion of four, instead of two, nuclei The remaining three of the original eight nuclei normally degenerate It would thus appear that *Plumbago capensis* presents a less specialised condition than the embryo-sac of *Plumbagella* described by Dahlgren in which only four nuclei are stated to be formed and in which the mature condition presented an egg cell and a fusion nucleus to which three nuclei contribute.

A detailed study of the cytology of *Scilla non-scripta* (*nutans*) has been made by Miss Hoare (*La Cellule,* XLII, 3, 269, and XLIII, 1, 7, 1934). Both in meiosis and mitosis the chromosomes are held to consist of two intertwined spiral chromonemata. The megaspore mother cell exhibits two nuclear divisions and a wall is formed which separates two binuclear cells. Of these the upper eventually becomes an embryo-sac with the normal eight nuclei but the lower, which may be regarded as an abortive embryo-sac, never passes beyond the four nuclear stage and these nuclei may degenerate. The embryo-sac is fully developed and ready for fertilisation some weeks before the flower opens Some thirty-five to seventy-four hours is required after pollination for development of the pollen tube and for fertilisation to be completed. The pollen tube traverses the style thence along the surface of the axile placenta and the funiculus to the micropyle After the first transverse division of the fertilised egg a further division produces a three-celled stage each member of which starting from the terminal cell undergoes longitudinal division. Fertilisation and triple fusion take place in the normal way. In a paper on the subject of sex in plants, Bressman (*Amer. Jour. Bot.*, XXI, 328, 1934) states that there are four kinds of plants of *Humulus lupulus.* Some Hop plants bear male flowers only, others female flowers, a few bear both types of flower whilst still others bear small flowers that are sterile. Of 123 root cuttings taken from male plants 56 developed a proportion of female flowers ranging from 1 to 75 per cent. Whilst sex reversal from male to female is not infrequent in the Hop the development of male flowers on female plants would appear to be extremely rare, but the author records an instance in which a single whorl of male flowers was present in the middle region of an otherwise female inflorescence.

PLANT PHYSIOLOGY.

By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

TRANSLOCATION.—Some important works on the transport in plants of assimilates and other dissolved substances have appeared during the last few years. Chief among these must be reckoned the further studies on this subject by E. J. Maskell, T. G. Mason and E. Phillis, dealing with the cotton plant. The transport of nitrogen is dealt with in a series of six papers published during the years 1929 to 1934 under the general title "Studies on the Transport of Nitrogenous Substances in the Cotton Plant." The first five of these papers are by Maskell and Mason, the sixth by Mason and Phillis. The first of them is entitled "Preliminary Observations on the Downward Transport of Nitrogen in the Stem." (*Annals of Botany*, **43**, 205–31, 1929). By chemical analyses of leaves collected at intervals over a period of twenty-four hours or longer it was shown that the nitrogen content of the leaf rises during the day and falls during the hours of darkness. The nitrogen content of the outer tissues ("bark") of the stem just below the foliar region shows similar variation in non-fruiting plants, but in fruiting plants there is no appreciable variation in the nitrogen content of this region. If a stem is ringed, that is, if a ring of the outer tissues is removed, nitrogen accumulates in the stem above the ring and decreases below it, thus indicating that movement of nitrogenous material is stopped by ringing, and so affording evidence that nitrogenous substances are transported in the outer tissues. This conclusion, together with the observed diurnal variations of nitrogen content of leaf and bark, afford strong evidence for the view that nitrogen compounds are synthesised in the leaves and transported from them in the tissues external to the xylem. It is significant that ringing does not affect the movement of inorganic nitrogen into the leaf, a finding which is in agreement with the generally accepted view that the nitrogen compounds absorbed by the root travel in the xylem dissolved in the water of the transpiration stream.

The second paper deals with "Observations on Concentration Gradients" (*Annals of Botany*, **43**, 615–52, 1929). The authors found that, contrary to what might be expected, the vertical concentration gradient of nitrogen in the bark is such that there is a higher concentration of organic crystalloidal nitrogenous compounds at a lower than at a higher level. With the leaves and wood, however, the gradient is in the opposite direction, and the authors hold this to support the view that the gradient of *mobile* nitrogen in the bark is in the direction of downward movement, for the mobile nitrogen in the leaves and wood of the stem should be in dynamic equilibrium with the mobile nitrogen in the bark. That the gradient of total organic nitrogen should be in the opposite direction might be due either to chemical masking, that is, the masking of the positive gradient of mobile nitrogenous substances by a negative gradient of immobile crystalloidal nitrogenous substances, or by regional masking, in which case the actual channel of nitrogen transport is limited to one part of the bark and the positive gradient in this channel is masked by negative gradients of nitrogen in the other tissues of the bark. But Maskell and Mason found firstly that there are definite negative gradients of amino-acids and asparagine in the bark as a whole and that these would more than account for the observed negative gradient of organic crystalloidal nitrogen, while secondly it was found that both the total organic crystalloidal nitrogen and the asparagine and amino-acid fractions exhibit a steeper negative gradient in the inner part of the bark which contains the sieve-tubes than in the outer part which consists chiefly of medullary ray cells and cortex. There is a possibility of masking by negative gradients in the companion cells, while it is also possible that part of the amino-acids and asparagine in the sieve-tubes is not really mobile but is rendered immobile in some way, as, for instance, by adsorption.

The analyses of different layers of the bark indicate that amino-acids and residual nitrogen (that is, crystalloidal nitrogen which is not accounted for by asparagine, amino-acids, nitrate and ammonia) are chiefly to be found in the sieve-tubes, while asparagine is present in highest concentration in the medullary rays, although it is also present in high concentration in the sieve-tubes. This suggests that organic nitrogen is transported chiefly as amino-acids or as the substances of unknown composition containing the residual nitrogen, while asparagine is a storage substance. It is important to note that the positive gradient observed in the leaf and petiole is due chiefly to residual nitrogen and to a less extent to amino-acids, while the gradients of nitrate, as we should expect, and of asparagine, are in the reverse direction.

In the third paper (" The Relation between Longitudinal Movement and Concentration Gradients in the Bark," *Annals of Botany*, **44**, 1–29, 1930) the question of the concentration gradients of nitrogenous substances is further examined. Stems were defoliated and ringed near the base, a treatment which has the effect of stopping the transport of dissolved material. In stems so treated there is still a marked negative gradient of crystalloidal nitrogen, as well as of protein, in the outer tissues of the stem ; indeed, the gradients are steeper than when translocation is still taking place. This is in marked contrast to what is observed with regard to sugars, for with cessation of movement the positive gradient of these latter substances disappears. Maskell and Mason therefore suggest that the observed negative gradient of crystalloidal nitrogen is the resultant of two components, a static negative gradient of storage nitrogen and a dynamic positive gradient of mobile nitrogen. When conditions lead to the cessation of movement the static negative gradient persists, not being concerned in movement, while the dynamic gradient of mobile nitrogen, like the sugar gradient, disappears.

In the fourth paper ("The Interpretation of the Effects of Ringing, with Special Reference to the Lability of the Nitrogen Compounds of the Bark," *Annals of Botany*, **44**, 234–67, 1930) the authors record the results of analyses which show that changes in nitrogen compounds in the bark, such as conversion of protein to crystalloidal nitrogen and vice versa, may be very rapid. Such changes are observed, for example, as a result of ringing, or alterations in humidity, desiccation bringing about a conversion of crystalloidal nitrogen to protein. A change in the reverse direction results from a decrease in sugar concentration and perhaps of hydrogen ion concentration. Since nitrogen compounds in the bark thus appear to be very labile it would seem that all the nitrogen fractions in the sieve tubes may be involved in the translocation of nitrogen through the stem, since immobile nitrogen can be transformed to mobile nitrogen, and vice versa, with such rapidity.

The fifth paper is concerned with the transport of nitrogenous substances to the fruit ("Movement to the Boll," *Annals of Botany*, **44**, 657–88, 1930). Removal of flower buds and fruits leads to an increase in concentration of both carbohydrates and nitrogenous substances in the leaves and both wood and bark of the stem, as would be expected when a centre of utilisation of assimilates is removed. By a series of analyses of developing fruits it was found that the uptake of both carbohydrates and nitrogen increased after fertilisation. The authors point out that this effect might be due to either an increased rate of utilisation of assimilates or to their easier entry into the ovules and carpels. In the first case the concentration of the entering substance in the ovule (or carpel) diminishes and the gradient steepens, while in the second case the concentration of the substance in the ovule increases and increased growth results. With both increased rate of utilisation and increased ease of entry the concentration of the substance in the ovule may remain the same. The rate of utilisation can be calculated by determining the total amount of substance utilised in a certain time and the average concentration of the substance during that time. Maskell and Mason call the amount utilised in unit time per unit of substance (carbohydrate or nitrogenous compound) present the utilisation index, and such indices they calculated for ovules and carpels in respect to sucrose, reducing sugars, and the various nitrogen fractions (protein, crystalloidal, asparagine, residual and amino-acid). From the values they obtained they concluded that the increased uptake of sugars resulted from both increased ease of entry of sucrose and increased utilisation. The increased intake of nitrogen after fertilisation appears to result from an increased rate of utilisation of crystalloidal nitrogen which brings about a lowering of the concentration in the organ. In the ovules this behaviour is shown by both asparagine and residual nitrogen, but by the latter only in the carpels. An examination of the concentration gradients of the different nitrogen fractions suggests, however, that the nitrogen enters both carpels and ovules from the sieve-tubes in the form of residual nitrogen and not as asparagine.

The sixth of the studies on the transport of nitrogen is entitled "Concerning Storage in the Bark" (*Annals of Botany*, **48**, 315–33, 1934). In this paper the authors examine further the static negative gradient of crystalloidal nitrogen in the bark. With formation of flowers nitrogen is withdrawn from the vegetative parts of the plant, more being withdrawn from the bark of the lower part of the stem than from that of the upper part, and the nitrogen gradient ultimately changes in direction. From these observations the authors conclude that there is a greater proportion of storage nitrogen in the lower part of the bark than in the upper part and that the observations support their view of a steep negative static gradient and a less steep positive gradient of translocatory nitrogen, the former being made up of storage substances. They find, however, there is no withdrawal of this nitrogen from the bark even under conditions of nitrogen starvation induced by cutting off the supply of nitrogen to the roots. Hence the storage of nitrogen in the bark appears to be unrelated to the supply of nitrogen to the plant.

In another paper Phillis and Mason return to the problem of carbohydrate translocation ("Studies on the Transport of Carbohydrates in the Cotton Plant. III. The Polar Distribution of Sugar in the Foliage Leaf," *Annals of Botany*, **47**, 585–634, 1933).

A number of analyses were made of the various carbohydrates in different parts of the leaf at different times, values thus being obtained for glucose, fructose, total hexoses, sucrose and polyglucosides. From their determinations the authors conclude that carbohydrate is conveyed from the assimilating cells to the phloëm chiefly as sucrose, and that it is in this form that it also travels through the phloëm of the leaf and petiole to the stem. There is a positive concentration gradient of sucrose in the phloëm of leaf and petiole, but the concentration of this sugar is about ten times as great in the phloem of the petiole as in the assimilating cells. Since it follows from the existence of the positive gradient that the concentration in the phloem of the fine veins is higher than in that of the petiole, there must be a sudden change in concentration in passing from the mesophyll to the phloem of the bundle ends. At these bundle ends the veins contain a large proportion of phloëm consisting of small sieve-tubes and large companion cells and undivided mother cells resembling the companion cells. The two latter kinds of cells are grouped together as transition-cells They form a large proportion of the total cross-section of the fine veins and it is suggested that they are the cells in which sucrose accumulates and from which it passes into the sieve-tubes. This supposition leads to another suggestion, namely, that the function of companion cells is also to accumulate sucrose from neighbouring parenchyma cells and pass it on to the accompanying sieve-tubes.

Two more papers by Mason and Maskell remain to be considered In the first of these (" Preliminary Observations on the Transport of Phosphorus, Potassium and Calcium," *Annals of Botany*, **45**, 125–73, 1931) data with regard to the transport of various ash constituents are presented and considered much in the same way as those presented by the same authors concerning carbohydrates and nitrogen. The general conclusion is drawn that the ash constituents ascend the stem in the wood and that phosphorus and potassium at any rate, like carbohydrates, are translocated downwards in the phloëm, although there is no evidence of a movement of calcium in this tissue. There appears to be more of the ash constituents moving downwards than is necessary for growth, having regard to the quantities of carbohydrate moving downward. It is suggested that this excess of nitrogen, potassium and phosphorus may pass back into the tracheæ, analyses of the contents of which indicate a comparatively high content of ash constituents as compared with that of sugar, while organic nitrogen may amount to one-third of the total nitrogen present. It has already been noted that on fertilisation the rate of uptake of carbohydrate and nitrogen by the ovule and carpel increases. The same is the case as regards phosphorus and total ash, but there is no indication of any such increase in the case of calcium, which appears to enter by way of the xylem.

In the last of the papers by Mason and Maskell to be considered here ("An Ontogenetic Study of Concentrations and Vertical Gradients," *Annals of Botany*, **48**, 119–41, 1934), the authors return to the problems of the concentration gradients in the bark of the cotton plant stem. They reiterate the view that where storage of a substance takes place in the bark there results a negative vertical concentration gradient, but with substances which are not so stored there is always a positive gradient. Among the latter is potassium, while phosphorus is stored at first and utilised later, with the result that an initially negative gradient becomes ultimately reversed to a positive one.

When this subject was last reviewed in these pages reference was made to the earlier observation of Mason and Maskell that the rate at which carbohydrates move through the phloëm corresponds to an effective coefficient of diffusion about 20,000 to 40,000 times that of the actual coefficient of diffusion of sucrose in water. For this extraordinarily high rate of movement O. F. Curtis ("Studies in Solute Translocation in Plants," *Amer. Journ. Bot.*, **16**, 154–68, 1929), suggests that protoplasmic streaming in the sieve-tubes is responsible by carrying the translocated material in the stream.

A quite different mechanism to account for the rapidity of translocation is that proposed by E. Münch in his " Druckstromtheorie " (*Die Stoffbewegungen in der Pflanze*, Jena, vii + 234 pp., 1930). According to Munch's theory the movement of dissolved substances in the phloëm is not brought about by diffusion, with each substance travelling more or less independently down its own concentration gradient, but by a mass movement of liquid. On this view the assimilating tissue as a whole may be compared with an osmotic cell attached to a capillary tube, the phloëm elements corresponding to the latter. If such a cell, separated from water, develops an osmotic pressure, water will be drawn in and the contents of the cell will be forced up the capillary. The rate of movement through the capillary depends, among other factors, on the ratio of the surface through which water is absorbed to the cross-section of the capillary, and if this ratio is large the rate of movement of liquid through the capillary may be very rapid. On the Münch hypothesis the water absorbed by the assimilating cells from the xylem in a similar way leads to a mass movement of liquid into and through the sieve-tubes of the phloëm. For the mass movement of liquid to proceed in this direction it is immaterial if the phloëm elements at their far end terminate in a similar cellular osmotic system, provided the pressure tending to send water in at this end is less than at the other. This theory has been criticised by O. F. Curtis and H. T. Scofield (" A comparison of osmotic concentrations of supplying and receiving tissues and its bearing on the Münch hypothesis of the translocation mechanism," *Amer. Journ. Bot.*, **20,** 502–12, 1933). They point out that the hypothesis requires a gradient of turgor pressure, chiefly dependent on osmotic pressure, from the supplying to the receiving tissues, and examined the theory from the point of view of this requirement. But in material of various kinds, sprouting potatoes, seedlings, sprouting onion bulbs and so on, they found the osmotic gradient was always in the reverse direction from that required by the theory and was in fact from the receiving tissue to the supplying tissue. On the other hand, H. H. Dixon (" Bast Sap," *Sci. Proc. Roy Dublin Soc.*, **20,** 487–94, 1933) has made observations on the sap exuding from punctures of the phloëm of the ash tree at different levels, and has found osmotic pressure gradients of 2 to 9 atmospheres per metre in the direction required by Münch's theory. A gradient of osmotic pressure in the same direction has also been recorded by Mason and Maskell for the cotton plant in the last of their papers referred to above.

Lastly, brief reference may be made to a theory put forward by A. S. Crafts (" Movement of Organic Materials in plants, *Plant Physiol.*, **6,** 1–38, 1931), according to which translocation is accomplished by a mass flow of solution through the cell walls of the whole phloëm. Crafts's work has been destructively criticised by F. C. Steward and J. H. Priestley (" Movement of Organic Materials in Plants ; a Note on a Recently Suggested Mechanism," *Plant Physiol.*, **7,** 165–71, 1932), mainly on the ground that the use made by Crafts of Poiseuille's formula for the rate of flow of a fluid through a tube involved assumptions which would lead to quite incorrect conclusions.

ZOOLOGY.

By G. R. de Beer, M.A., D.Sc ; and J. A. Moy-Thomas, B.A. ; the University, Oxford.

The nephridia of Amphioxus have been the subject of two papers by E. S. Goodrich (*Quart J. Micr. Sci.*, **76,** 1934, 499 and 655), in which he has studied their development. In the unpaired Hatschek's nephridium as well as in the paired nephridia, the solenocytes and the nephridial tube arise from groups of cells situated between the endodermal wall of the pharynx and the coelomic epithelium, but derived from neither ; they must presumably be of ectodermal (or ectomesodermal) origin. At no time do the nephridia of Amphioxus possess an internal opening: they are true protonephridia, agreeing in structure and development with the similarly named organs in Invertebrates.

It is very much to be hoped that teachers will pay particular attention to Goodrich's researches on nephridia, for there is now no excuse for the shocking confusion between nephridia and coelomoducts so often shown by scholarship candidates, and from which even recent text-books are unhappily not free. It is therefore particularly unfortunate that P Gérard and R. Cordier (*Biol. Rev*, **9,** 1934, 110) and R. Cordier (*Arch. de Biol.*, **45,** 1934, 431) should have interpreted their studies into the histophysiology of the frog's kidney and the earthworm's nephridia on the supposition that these structures are homologous. Of few things is it possible to say with such authoritative conviction as in this case, that this is not true Gérard and Cordier find that in both organs, the cells have the property of absorbing electro-negative colloid particles at their apical poles: an interesting fact, but devoid of any bearing on the homology of the structures

Recent parts of the Archives de Biologie contain a number of papers devoted to the study of endocrine organs. L Desclin (*Arch. de Biol*, **45,** 1934, 503) has investigated the effects on the anterior lobe of the pituitary of castration and gestation He finds that the effect exerted on the pituitary by the gonads is due to a hormone associated with the presence of gametocytes, and not with the hormone produced by interstitial tissue. The modifications in the pituitary anterior lobe accompanying pregnancy are shown to be associated, not with the presence of the embryo or placenta, but with that of a corpus luteum.

The thymus, and in particular the method of origin of the small thymus cells, is the subject of a study by H. de Winiwarter (*Arch. de Biol.*, **44,** 1933, 741). Formerly, this author regarded these cells as epithelial derivatives. He now believes that the small cells arise in this manner at first, but that subsequently an invasion of lymphocytes takes place, and that the lymphocytes are then indistinguishable from the epithelial derivatives. This view is criticised by A. P. Dustin (*Arch de Biol.*, **45,** 1934, 339) who considers such an invasion (unless in the case of pathological inflammation) unlikely owing to the presence of a basement membrane surrounding the thymus at all stages. The last word has not been said, therefore, as regards the solution of this problem.

F. Thomas (*Arch. de Biol.*, **45,** 1934, 189) has devoted an intense study to the problem of the secretion of the thyroid in man. The gland possesses four types of epithelial cells, differing in form and in function : three of these types are concerned with very slow, slow, or rapid elaboration and secretion of colloid into the vesicles, and the fourth performs the extraction of thyroxine from the colloid and excretes it into the neighbouring vessels. The relative preponderance of these types and functions varies at different ages and under certain pathological conditions.

G. R. de Beer and E. J. Barrington (*Phil. Trans. Roy. Soc.*, B, **223**, 1934, 411) have investigated the segmentation of the head of the duck and the development of its cartilaginous skull They find the number of segments in the bird's head to be 9½ as against 9 for the mammal, the difference being due to the fact that the occipito-atlantic joint is intervertebral (intrasegmental) in the former and intravertebral (intersegmental) in the latter. There are reasons for believing the original skull to have occupied the first four segments ; the skull-region (if and where any) posterior to this representing absorbed vertebral bodies. The cartilaginous skull of the bird presents considerable resemblance with that of the crocodile.

J. A. Moy-Thomas (*Quart. J Micr Sci* , **76**, 1934, 482) has studied the relation of bony plates to teeth in Belone, Salmo, Amia, and Lepidosiren, and presented further evidence in favour of the view that bone did *not* arise as a result of the fusion of the basal plates of denticles Bone is frequently formed precociously at the base of developing teeth, but is *never* part of the tooth, although frequently interpreted as such.

H. W. Lissmann and A. Wolsky (*Zeitschr. für vergl. Physiol.*, **19**, 1933, 555) have made use of the well-known fact that in crayfish an antenna may be regenerated in place of a lost eye, to perform experiments of the greatest interest on the nature of the stimuli to which the heteromorphic regenerated antenna responds, and the reflex arcs with which it is connected. The result of application of chemical, tactile and rheotactic stimuli show that this antenna perceives chemical stimuli (which the eye does not) although it is innervated by the former optic nerve.

C. H. Waddington (*J. Exp. Biol.*, **11**, 1934, 211) has continued his experiments on the action of organisers, and has shown that the primitive streak of the chick is capable of inducing the formation of neural plate in rabbit tissue.

PALÆONTOLOGY.—Recent research had left no doubt that the Macropetalichthyids were Elasmobranchs, but the relationship of the so-called " Euarthrodira " (Coccosteus, Dinichthys, etc.) was a very vexed question. Briefly, the two schools of thought were that on the one hand the group had jaws which were not comparable to those of a true Gnathostome, but were on a line of their own, and on the other hand that they were related to the Macropetalichthyids and consequently to the Elasmobranchs. Stensiö (*Kungl. Svensk. Vetens. Handl.*, **13**, No. 5, 1934) has given descriptions of the heads of these Euarthrodires—Pholidosteus, Leiosteus, and an Acanthaspid—which show that in the structure of the braincase and jaws they resemble the Elasmobranchs and Holocephali, particularly the latter. Therefore, there is no need for any separation of the Macropetalichthyids from the Euarthrodires and the two can be classed together as the Arthrodira, a Devonian group of bony Elasmobranchs, some forms of which closely resemble present-day sharks and rays in general body form. One of the most interesting points about the paper lies in the fact that the nature of the jaws of Arthrodires, which has been a problem since the days of Hugh Miller, has been finally elucidated.

PHYSICAL ANTHROPOLOGY.

By L. H. Dudley Buxton, M.A., D.Sc., Exeter College, Oxford.

An historical epoch is usually not noted until it has passed, but there does seem to be a definite tendency on the biological side of anthropology which has become very evident in the publications of the last few months. There has, for many years, been considerable differences of opinion between the morphologists on the one hand and the statisticians on the other without any real specialisation on either side. But it now looks as if with the increasing specialisation of modern science the two branches are beginning to mark out a territory which they will explore and that each will study in the manner that seems best to them. Such a division of labour is likely to prove fruitful to both Recent papers suggest that at the moment special interest is being focussed on primitive man, both by the morphologists and the statisticians, on the anatomy and physiology of the soft parts as opposed to the skeleton by the morphologists, while the biometricians are setting their house in order by attempting to get uniformity of technique and by exploring methods of measurement which are either new, or have so far been neglected.

Among the studies on primitive man the most outstanding is the late Dr. Black's Croonian lecture which appears in *Phil. Trans.*, B, **223**, and incorporates additional material since the lecture was delivered. Dr. Black, whose paper is entitled *The Discovery, Morphology and Environment of Sinanthropus Pekinensis*, gives us what must be, alas, owing to his premature death, his last pronouncement on his great discoveries. The paper is of special importance since it not only summarises previous publications but also includes a great deal of new material. Those features which had previously been discussed in detail are only briefly referred to. Although *Sinanthropus* occurs irregularly throughout the whole cave deposit in special association with a cultural ashy layer, up to the present, except in one locality, no skeletal material has been found in undoubted association with the fossiliferous deposits ; there is, however, no doubt as to the lower pleistocene date. The length of occupation is an interesting and important point, both archæologically and morphologically Judged by the occupational layers Dr. Black concludes that the essential cultural characters of *Sinanthropus* suffered no change during a period at least three times as long as was required in a cave near Santander in Spain to evolve from an early form of Mousterian to Azilian, practically the whole of the Middle and Upper Palæolithic and part of the Epipalæolithic cultures. Dr. Black suggests a modified classification for the artifacts, but the important part of his paper is its discussion of the morphology. Previously he had dealt in detail with the endocranial cast and this subject is now treated briefly. The morphological problems however raised by the teeth, jaws and braincase are of extraordinary interest and importance in the study of the human race. It will be recalled that a good many years ago Sir Arthur Keith proposed the term *taurodont* for the peculiar form of teeth found in Neandertal man. In this form the pulp cavity is enormously enlarged in the molar teeth and extends downwards, the teeth having the shape, more or less, of a double cube. The condition in modern man Sir Arthur termed *cynodont* , here the pulp cavity is small and the roots of the molars separated, but even where they are fused, a not uncommon condition, the pulp cavity still remains small. Black proposes the new term *megaphanic* for the large cavity type, and *microphanic* for the type found in modern man and in the anthropoids. He believes that the taurodontism of the Neandertalers is a development of the condition found in *Sinanthropus*, while the type of cynodontism in modern man is a special microphanic condition in the opposite direction, and indeed a form of dental degeneration. It will be of great importance if this can be proved as Neandertal man's teeth have always been a difficult problem. Black also suggests that in other ways Neandertal and modern man represent divergent lines of evolution from a generalised stock represented by *Sinanthropus*. By using dioptograph tracings, which were not previously available, the lateral view suggests to him on the basis mainly of height the following order, a juvenile Gorilla, *Sinanthropus*, Rhodesian man, a Chapelle-aux-Saints (a typical Neandertaler), and the modern " North China " type, but when tracings in the plane at right angles to the last are taken, *i.e.* the transverse plane of the skull, he gets the following order, Neandertal, Rhodesian, North China, *Sinanthropus* and Gorilla, and deduces from this and other evidence that Neandertal man is specialised, while *Sinanthropus* is a stage advanced from the anthropoids in a progressive direction.

In regard to the jaw, *Sinanthropus*, both juvenile and adult, is in Black's opinion unexpectedly "neanthropic," that is like modern man, but the chin, which is only represented in the young condition, is very archaic and recalls the anthropoids, while certain parts of the horizontal ramus—that is, the part of the jaw which runs from the chin to the angle—recalls the Mauer jaw. There are as yet no remains of the lower limb, but what little there is of the upper limb is singularly human in shape Further, as Black's previous publications have been at pains to show, it is suggested that probably *Sinanthropus* was right-handed and had evolved a "nervous mechanism for the elaboration of articulate speech." This is the human side, but relationship to the Ape man of Java, *Pithecanthropus erectus*, is not less interesting. Black believes that the upper teeth of *Pithecanthropus* are as different from those of *Sinanthropus* as those of Neandertal are from modern man and suggests that on the basis of general morphology it may be concluded that *Pithecanthropus* represents a specialised conservative form, while *Sinanthropus* is a generalised progressive Finally Black, after recapitulating the general morphology of *Sinanthropus*, emphasises his truly human status and suggests that he was fully capable, in view of his mental and physical development, of being the sole artificer of the cultural debris associated with his remains With regard to the generic status of *Sinanthropus* Black says that the generic name was simply created to be a useful tool. Further light on the very interesting problem of the mental development of early man is provided by a paper by Dr. Joseph Shellshear in the same volume of *Philosophical Transactions* in which the primitive features of the human brain are discussed in special relation to a brain of a bushwoman, which Marshall had described many years ago. The special importance of this paper is the comparison with early hominid types, *Pithecanthropus*, *Sinanthropus* and *Eoanthropus*.

Although Dr. Black's dioptograph tracings were not published earlier they were communicated to Professor W. J. Sollas for the valuable paper on "The Sagittal Section of the Human Skull" (*Journ. Royal Anthropological Institute*, LXIII, 389). The Professor suggests that the difficulty in dealing with sections is the proper method of superposition. In order to avoid this difficulty he places the tracings on a card which is cut to the shape of the tracing and then the centre of gravity is found by the rough method of swinging the card. He then draws radii to the periphery and subsequently takes a series of angular measurements by taking a point on the major axis of the foramen magnum. His paper covers a wide field and shows the angles obtained on anthropoids, Neandertal man and various types of modern man when studied by this method.

The anthropology of the soft parts of the human frame is discussed in an important paper by Dr. Léon Pales (*L'Anthropologie*, XLIV, 1934, 45). Dr. Pales had considerable opportunities for study of the Congolese Negro when working on the railway at Brazzaville in French Equatorial Africa. His paper represents a special study ; after measuring stature, weight and chest breadth—standard measurements—he then took observations on muscular strength and made a study of special muscles among his subjects, and made other morphological observations In addition, he took blood pressures and considered the general question of the racial pathology of the district. At present work of this type has only been comparatively infrequently undertaken and the accumulation of material especially of scope represents real progress in this interesting but as yet very little explored branch of anthropology. Blood grouping continues to interest a number of researchers Dr. D J Burning has a long paper (*L'Anthropologie*, XLIV, 1934, 77 and 315) on blood groups in the Dutch Indies There is considerable variation in this region in the bodily form of the people and Dr. Burning concludes that though there is a certain amount of correlation between the blood groups and racial form yet the association is by no means invariable.

The form of the face has been for a very long time a character which the layman as opposed to the anthropologist has probably thought to be one of the most important criteria in diagnosing racial differences. Unfortunately, however, the ordinary physical measurements which have been taken for a number of years fail to disclose these differences satisfactorily. This difficult problem has been carefully and elaborately studied by Drs. Morant and Woo in *Biometrika*, XXVI, 1934. They advocate the use of two additional indices to those which are usually taken by anthropologists, the *Rhinal* and the *Simotic*. Avoiding technicalities these two indices may be described briefly as representing the shape of the nose on a skull in horizontal section at the most prominent point and at the root. The Rhinal index is the percentage ratio of the subtense from the tip of the nasal bones, called by most anthropologists the *Rhinion*, to the chord of the mid-orbital breadth. The *Simotic* index is calculated in a similar manner but in this case the point at which the two measurements are taken is what the authors term the *Simotic Chord*, which is the minimum horizontal breadth of the two nasal bones taken from points on the nasomaxillary suture. The other measurement represents the subtense of the ridge of the nose from this chord. Both these indices are, then, ratios of nasal flattening, one at the root of the nose and the other corresponding to points approximately half-way down on the living subject. It will be common knowledge that a European, a Chinese and a Negro appear to differ very considerably if this part of their facial anatomy is superficially examined, but on the whole there is a general resemblance in this respect between all Europeans and similarly between all other representatives of, say, the Negro stem. It is exactly this conclusion which the elaborate measurements of this paper show to be true. The authors consider that the characters are practically constant for any groups belonging to a single major race, but when comparison is made between the major divisions of mankind they appear to be excellent criteria, comparable to skin colour, the nasal index, and the various measurements of prognathism, indeed they are better than some of these suggested criteria, for some who are undoubtedly black are certainly not Ethiops and the nasal index among the aborigines of America varies from the narrowest to not far short of the very wide among mankind. The disadvantage of the index is that it needs special instruments and a somewhat elaborate technique to use. A more orthodox series of measurements is found in a paper by J. Szombathy in the current number of the *Journal of Anthropological Society of Vienna* on Bronze Age skeletal material from lower Austria. The paper is of special interest to us in England as it appears to throw some light on the problem of the racial history of this country. The author himself is naturally content to deal with his own problem and to give an account of the measurements and morphology of the material before him, but it so happens that though the measurements do appear in fact to differ in many ways from any series in our country, more especially in the great height of the cranial vault, it would seem, from an examination of the figures, that they differ very much from the Bronze Age peoples of these islands, but on the other hand probably show affinities with later comers. It is not possible to speak definitely till proper comparisons have been made, but the collection of such early material and its publication in a clear form is valuable and instructive.

The biological problems concerned with the density of populations have recently been attracting considerable interest, especially among zoologists. Among mankind the study has for the most part very naturally been focussed on civilised man where accurate data are available. Anthropologists are, however, beginning to consider the problem presented by primitive man, and Dr. A. L. Kroeber in *The American Anthropologist,* 1934, p. 24, discusses at some length the aboriginal population of America previous to 1492. He puts the figure at 8,400,000, but admits that there is a wide margin of error and suggests that in general terms it may be said to be between seven and ten millions.

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Compositio Mathematica, Volume I, Fasciculus I (E. M. W.)

All mathematicians will welcome this new international periodical, edited by Messrs. Bieberbach, Brouwer, de Donder, Julia and Wilson, assisted by a large committee of well-known mathematicians. The names of the editors, the advisory committee and the contributors are a sufficient guarantee of the high quality and the interest of the articles published We may indicate the wide range of the subject matter by quoting the titles of some of the papers :—Paul Lévy, " Sur la convergence absolue des séries de Fourier " ; J. G. van der Corput, " Zur Methode der stationären Phase " ; G. N. Watson, " Generating functions of class-numbers " ; J. von Neumann, " Zum Haarschen Mass in topologischen Gruppen " ; T. Levi-Civita, " Terne di congruenze sopra una superficie ed estensione della trigonometria " ; A Khintchine, " Eine Verschärfung des Poincaréschen ' Wiederkehrsatzes ' " ; and many others. The printing and presentation generally of this volume of 192 pages are excellent; it is published by P. Noordhoff of Groningen.

Magnetic Materials at Radio Frequencies (N. L. Y.-F.)

Laminated iron cores have long been used for the construction of inductances and transformers at commercial A C. and audio frequencies, but it is only recently that materials having a permeability appreciably greater than that of air and capable of being used at radio frequencies have been produced. These materials consist of finely divided particles of iron or magnetic alloy dispersed throughout some insulating medium. Our present knowledge of the behaviour of such compositions, which is still rather meagre, is surveyed in a recent publication [1] of the Radio Research Board.

The impossibility of using the ordinary type of core at high frequencies is due to the magnitude of the eddy-currents. Apart from the direct loss of energy due to these currents, their presence changes the distribution of the magnetic flux, which decreases the apparent permeability of the material and increases the hysteresis loss. The specific behaviour of magnetic substances at such frequencies is thus difficult to investigate, but it appears to be established experimentally that when due allowance has been made for such effects of the eddy-currents the actual initial permeability of iron and ferromagnetic alloys is independent of frequency at least up to the highest practical radio frequencies. The initial slope of the μ-H curve, however, seems to be less at the higher frequencies. There is some evidence that the true hysteresis loss per cycle increases with frequency, but its value at radio frequencies is unknown.

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[1] *Magnetic Materials at Radio Frequencies :* A Critical Survey of Present Knowledge. By F. M. Colebrook. Radio Research Board Special Report No. 14. H.M. Stationery Office. 6*d*. net.

For a given shape and size of the particles there is a " bounding frequency " (*Grenzfrequenz*) above which the effect of eddy-currents is important. It is desirable to make this greater than the highest working frequency by sufficiently diminishing the size of the particles ; their apparent permeability is then equal to the actual permeability of the magnetic material. The effective permeability of the composition is however very much less, and depends mainly upon the spacing between the particles, the reductance of the gaps being much greater than that of the particles if the permeability of the latter is of the order attained with iron.

The energy losses in a core of such material can be formally represented by ascribing to it a complex permeability, analogous to the complex dielectric constant of liquid dielectrics. This leads to the conception of a " power factor " of the material. So far, practical compositions have been produced having power factors of the order of 1 per cent associated with effective permeabilities of 4–10 and suitable for frequencies up to 2,000 kilocycles per second. Inductances wound on cores of such material occupy less space than air-cored coils of the same inductance and resistance, this reduction in bulk being most marked in the case of screened coils.

It is not known how far the behaviour of such material at radio frequencies is consistent with theory, nor is there any certain knowledge of the constitution of the losses. There are no published data of the effect of such materials on wave form as a function of frequency and flux density. There is no evidence that the limit of improvement in such materials has yet been reached.

Mammoth Remains (E. H.)

The recent discovery of mammoth remains in the U.S.S.R., so remarkably well preserved that flesh and even the brain are in perfect condition, recalls the former discoveries of such wonderful examples of nature's cold storage in the north. It is about 15,000 years since the last mammoths on earth died out on the North Siberian tundras, but the first find of a mammoth "in the raw," frozen in the snows and preserved just as New Zealand lamb or Argentine beef is preserved in the refrigerator, was found by Ides, a Dutch ivory collector who came across a frozen head and foot in Siberia in 1707, though the first complete animal was found on the banks of the River Lena in 1799. In 1869 Baron von Meydell discovered three such cold-storage specimens in north-east Siberia, and others since have been so perfectly preserved that dogs, foxes and wolves devoured their flesh with gusto. Until these mammoths in the flesh were discovered, it was unknown that these great elephants were covered with thick reddish wool and long black hair. The mammoth was a vegetarian too, for leaves of pine, fir and willow, and grass, wild thyme and seeds of poppies and buttercups have been found in their stomachs and sticking to their teeth. The famous Beresovka mammoth is mounted just as it was found in the snows, in Leningrad museum. Mammoth bones, tusks, and teeth are of course common in collections, and mammoth teeth have been dug up in Cheshire at Marbury and other places, and in the Derbyshire caves. What are believed to be the world's biggest mammoth teeth are a pair each 16 feet 6 inches long in the Geological Museum, Mexico. A tooth in South Kensington Museum is 14 feet long, while one found the other year in the Pleistocene deposits in Texas showed that the mammoth suffered badly from toothache.

The Minerals that cause Silicosis (G. W. T.)

No stranger contact between widely separated fields of science has been recorded than that between petrology and pathology which has been established by the work of Dr. W. R. Jones on the minerals suspected of causing silicosis. His results bid fair to revolutionise current views as to the origin of this disease, and will doubtless, after the usual time lag, bring about important changes in silicosis legislation. The three papers in which this work has been published are: "Silicotic Lungs: the Minerals they Contain" (*Journ. Hygiene,* XXXIII, 1933, 307–29); "Silicosis" (*Journ. Chem., Met., & Min. Soc., S. Africa,* Sept. and Oct. 1933, pp. 3–52, reprint), with the full report of a valuable discussion by South African pathological, geological and mining experts; "Silicosis" (*Trans. Inst. Min. Met.,* 1934, pp. 1–42, reprint), with the full report of a discussion by British experts. Dr. Jones investigated the inorganic mineral particles found in silicotic lungs from British workmen and miners, and from Witwatersrand gold miners. He found that the great majority of these mineral fragments consisted of fibres of sericite, a scaly and fibrous form of white mica, which is chemically a hydrated silicate of aluminium and potassium. Free silica in the form of quartz was also present, but in such relatively large particles that chemical analysis of the residues reveals free silica in quantities out of all proportion to its possible effect in producing silicosis.

The definition of silicosis which was accepted at the International Congress on Silicosis (Johannesburg, August 1930) was as follows: "a pathological condition of the lungs due to the inhalation of silicon dioxide," and that this substance must react in the lungs in a chemically uncombined state, *i.e.* it must be present in the form of the mineral quartz. Dr. Jones claims that his examination of residues from numerous silicotic lungs under the petrological microscope shows that quartz cannot be the chief cause of silicosis, and that this equivocal honour must be conferred on sericite. He supports this view by considerations based on the distribution of the disease in relation to the nature of the rocks and minerals worked in mines and quarries. For example, the gold-bearing quartz-conglomerate of the Witwatersrand, which contains innumerable fibres of sericite, gives rise to dust which has caused thousands of cases of silicosis, but the gold-bearing quartz rock of the Kolar goldfield (Mysore), which contains even larger quantities of quartz but is free from sericite, has caused no case of the disease, although it is worked under conditions of mining, labour, and medical examination which are comparable to those of the Rand. Many mines in which quartz is extensively drilled, but in which no cases of silicosis have occurred, can be cited. Again, certain British factories which have crushed quartz for industrial purposes for many years have nevertheless been free from the disease. On the other hand, silicosis is rampant in some industries and in mines which use and work materials containing only small percentages of quartz, as, for example, the cryolite factories of Denmark, and the Broken Hill mines (New South Wales). The rocks drilled at these mines contain very little quartz, but large quantities of fibrous minerals such as sericite and sillimanite. Dr. Jones further cites the contrast in respect to silicosis between the South Wales and Scottish coal-fields. Silicosis is frequent among the anthracite miners of South Wales, whereas (up to quite recent times) the disease had not been reported from Scottish mines. Dr. Jones ascribes the difference to the abundance of sericite in the altered sandstones of the South Wales Coal Measures, and its rarity in Scottish Carboniferous sandstones. Recent work, however, has shown that this contrast can no longer be maintained. Several cases of undoubted silicosis have unfortunately been demonstrated by Dr. J. Crocket in Scottish miners within recent years, and the writer has found abundant sericite in the sandstones, and especially in the sandy shales (faikes) which are drilled in the mines along with coal.

Dr. Jones states that no quartz-bearing dusts investigated by him are known to have given rise to silicosis except those which also contain an abundance of fibrous aggregates of sericite, or of other fibrous silicate minerals, loosely held together and easily freed into the atmosphere when the rock is drilled or blasted.

Longitude Observations in Greenland and Continental Drift (G. W. T.)

Wegener's theory of drifting continents involves a westerly drift of Atlantic land masses such as Greenland, and there should be an appreciable shift of longitude at any given place if measurements are made over a sufficiently long period of time. A favourable series of measurements is that which has been carried out at intervals during the last century on Sabine Island in East Greenland. The longitude of Sabine Island was first determined by Admiral Sabine in 1823. It was again measured in 1870 by the astronomers of the *Germania* Expedition, and in 1906 by a Danish expedition under J. P. Koch. Comparison of the positions thus obtained led Wegener to postulate a westerly drift of Greenland of about 30 metres a year.

It will be remembered that Prof. Wegener recently lost his life on an expedition in Greenland. The Norwegian Government, in an attempt to test what is perhaps crucial evidence of continental drift in Wegener's sense, sent out Hans S. Jelstrup to determine the longitude of Sabine Island with the aid of the latest instruments, the most modern methods, and with the great advantage over previous workers in this field of being able to use electrically controlled chronometers and wirelessed determinations of time. The results of Jelstrup's work have recently been published ("Détermination astronomique à Sabine-Oya au Groenland orientale, avec discussion touchant une dérive continentale possible," *Skr. om Svalbard og Ishavet*, Nr. 58, 1933, 27 pp.).

Taking the *Germania* observations of 1870 as the most accurate of the older measurements, Jelstrup finds an apparent displacement of 615 metres to the west in the 62 years between 1870 and 1932, which amounts to a drift of 10 metres a year in the direction demanded by the Wegener theory. According to Jelstrup, the older observations are subject to a systematic error of at least ± 83 metres, his own to an error of ± 22½ metres; but it is probable that the real error, systematic and accidental, of the older measurements may amount to a few hundreds of metres. Nevertheless, after minute examination of all possible sources of error, Jelstrup is convinced that the positional difference obtained by him (615 metres) is much greater than the sum of all possible errors in the measurements. That is to say, there is a true drift toward the west in the sense predicated by Wegener, but of much smaller amount than was supposed by him. As Wegener showed, comparisons of the two successive pairs of observations up to Koch's expedition of 1906 both give displacements toward the west; and the fact that Jelstrup's new and extremely accurate measurements are consistent with this result is regarded as valuable evidence in favour of Wegener's views.

Making all allowances for sources of error in his own and the older measurements, Jelstrup finds that a longitudinal difference in the position of Sabine Island between 1870 and 1932 of 250 metres must be ascribed to real displacement in accordance with the Wegener theory. This amounts to an annual drift of 13·2 feet, or a mile in 400 years. Assuming the truth of Wegener's theory, and taking the mean distance of Greenland from Europe at 2,000 miles, it has taken 800,000 years to effect the present separation of the two land masses This figure is in approximate agreement with estimates of the date of the Glacial Epoch, subsequent to which Wegener places the separation of Greenland from north-western Europe.

Miscellanea

The honours list published on the occasion of the King's birthday included the following names well known in scientific circles. *K.B.E.:* Dr F. G. Banting, of insulin fame *Knights:* Mr. A. W. Flux, past president of the Royal Statistical Society; Dr. C. E. Saunders of Canada, discoverer of Marquis, Ruby, Reward and Garnet Wheat; Prof. G. Elliot Smith, professor of anatomy, University of London, University College. *C B.:* Dr. R. E. Stradling, director of Building and Road Research, D.S.I.R. *C.M.G:* Prof. R. S. Troup, professor of forestry in the University of Oxford. *C.B.E.:* Dr. W. L. Balls, chief botanist, Egyptian Ministry of Agriculture; Mr. L. St. L. Pendred, editor-in-chief of the *Engineer*; Dr. L. J. Spencer, keeper of minerals, British Museum (Natural History). *O.B.E.:* Dr. S. G. Barker (for his research work on behalf of the Empire Marketing Board); Mr. A. D. Cotton, keeper of the Herbarium and Library, Royal Botanic Gardens, Kew ; Miss E H. Ekins, principal of the Studley Horticultural and Agricultural College for Women ; Miss A. Lorrain-Smith (for her work in mycology and lichenology) ; Dr. C. Raeburn, of the Geological Survey, Nigeria.

Prof. H. L. Lebesgue, professor of mathematics in the Collège de France and Prof. O. Warburg of the Kaiser-Wilhelm Institut für Zellphysiologie, Berlin-Dahlem, have been elected Foreign Members of the Royal Society. Viscount D'Abernon has been elected a Fellow of the Society under the special statute which permits the admission of persons who " have rendered conspicuous service to the cause of science " or whose election " would be of signal benefit to the Society."

Prof R Robinson, Waynflete Professor of Chemistry in the University of Oxford, has been elected Foreign Associate of the U S. National Academy of Sciences.

The Albert medal of the Royal Society of Arts for 1934 has been awarded to Sir Frederick Gowland Hopkins, P.R.S.

The Royal Society of Edinburgh celebrated the end of the hundred and fiftieth year of its foundation on July 2 when a number of honorary fellows were appointed. These included Prof. H. E. Armstrong, Prof. J. S. Haldane, Prof. Karl Pearson, Prof. E. B. Poulton, Prof. Sir G. Elliot Smith and Prof. W. W. Watts.

The gold medal of the Linnean Society has been awarded to Sir Sidney Harmer and the Simms gold medal of the Royal Aeronautical Society to Prof. Sir Gilbert Walker for his work on cloud formation.

Prof. L. R. Wilberforce has announced that he will retire from the Lyon Jones chair of physics in the University of Liverpool on or before September 30, 1935.

Mr. E. Barnard has been appointed director of food investigation in the Department of Scientific and Industrial Research and Dr. F. Kidd superintendent of the Low Temperature Research Station at Cambridge. Both these posts were held by the late Sir William Hardy.

We have noted with regret the announcements of the death of the following well-known scientific workers during the past quarter: Harriet W. Bigelow, professor of astronomy at Smith College, Northampton, U.S.A ; Dr. N. L. Britton, until 1929 director of the New York Botanical Garden; Sir Walter P. Buchanan-Riddell, Chairman of the University Grants Committee; Prof. F. W. Burstall, emeritus professor of mechanical engineering in the University of Birmingham; Prof. A. P. Chattock, F.R.S., of the University of Bristol; Dr. L. Cockayne, F.R.S., of New Zealand, botanist; Prof. G. C. Comstock, astronomer, University of Wisconsin; Mme Curie—on July 4, aged 66 years; Father Giuseppe Gianfranceschi, president of the Pontifical Academy of Sciences; Mr H. S. Hall (Hall & Knight); Mr. H. O. Larsen, horticulturist; Carl O. Lundholm, expert on explosives; Prof. M. S. Pembrey, F.R.S., of Guy's Hospital Medical School, physiologist; Dr. T. G. Pinches, assyriologist; Maj.-Gen. George O. Squier, K.C M G., of the U.S A., an expert on electrical communication; Prof B. D. Steele, F.R S. physical chemist (on April 12); Dr. J. P. van der Stok of Utrecht, mathematician and oceanographer.

Dr. A W. Rogers has been elected president of the Royal Society of South Africa ; Dr. W. T. Calman president of the Linnean Society of London and Prof. W. J. Dakin president of the corresponding Society in New South Wales; Prof. H. L. Hawkins, professor of geology in the University of Reading, has been elected president of the South-Eastern Union of Scientific Societies.

Sir N. Sicar replaces Sir C. V. Raman as president of the Indian Association for the Cultivation of Science and Dr. S. K. Mitra succeeds Prof. K. S. Krishnan as secretary. The new president is a physician and was at one time vice-chancellor of the University of Calcutta.

The inaugural meeting of the Royal Society of New Zealand was held at Wellington on May 11. Prof. R. Speight, professor of geology at Canterbury College, Christchurch, being first president. The new society has developed out of the New Zealand Institute which was founded in 1867. It has forty-eight fellows.

The International Council of Scientific Unions held its first triennial meeting under the new statutes in Brussels on July 8–13 when delegates of the various unions represented on the Council gave accounts of their work during the past three years. Dr. N. E. Nörlund of Copenhagen was elected president for the period 1934–37 and Sir Henry Lyons general secretary. The next meeting will be held in London in 1937.

At the International Congress on Theoretical Physics held at Kharkov in May E T. Williams stated that certain Wilson chamber photographs obtained by Kunze show tracks which may be due to negative protons or negatrons Tolansky has pointed out (*Nature*, July 7) that the fine structure of the line spectra of atoms of odd atomic weight possessing even atomic charges could be explained by supposing that nuclear neutrons are of two types, *i.e.* either proton + electron or negative proton + positron. Whether this be so or not there are now good reasons for supposing that the atomic nucleus does not contain any electrons.

Simon and Kürti, writing to *Nature* (June 16) from the Clarendon Laboratory, Oxford, state that they have found cadmium to be a superconductor with a transition temperature about 0·6° K. In the course of their experiments they reached a temperature of 0·1° K by the magnetic method using 0·5 gm. of manganese ammonium sulphate. The cadmium was cooled by actual mixture with this salt.

The *Bell System Technical Journal*, April 1934, opens with a paper by Dr. F. S Goucher dealing with the theory of the carbon microphone By a series of ingenious and elaborate quantitative experiments which involved the measurement of forces of the order of 1 dyne and movements of 10^{-7} cm it is shown that "the current is carried through regions in intimate contact, and changes in resistance under strain are due both to a variation in the number of microscopic hills which form the carbon surface and to area changes at the junctions of these hills arising from their elastic deformation in accordance with the well-known laws of elasticity." Variations of contact resistance with voltage up to 1·0 volt were shown to be due to Joule heating.

In one set of experiments the force holding two carbon granules in contact was controlled by suspending one granule from a helical spring of fused quartz while the other was fixed to a supporting plate itself suspended by platinum wires which could be heated (and so varied in length) by an electric current. In another set the force between a carbon granule and a carbon plate was varied electrostatically using the principle of the attracted disk electrometer while the resulting elastic deformation of the surfaces in contact was measured by arranging matters so that their displacement altered the distance between the plates of a condenser. This condenser formed part of an oscillating circuit and the change in its capacity was determined in the usual way with a wavemeter. Deformations as small as 10^{-8} cm could be measured in favourable circumstances. The April issue of this *Journal* contains also six papers giving details of the elaborate arrangements used by the American Telegraph and Telephone Co for the wire transmission of music and its reproduction in auditory perspective.

In his eighth and last *Annual Report* Lieut -Gen. Sir William Furse, director of the Imperial Institute, emphasises the unsatisfactory financial position of the Institute. Its income from the "original endowment" and the "chance letting of rooms" is less than £10,000 a year, while £40,000–£50,000 is actually needed. It does not receive a regular Government grant, and has been able to exist these last ten years only as the result of private generosity. Even the Cinema, attended in 1933 by 146,000 persons, including scholars sent by 1,500 schools, is threatened It was maintained by a grant from the Empire Marketing Board, which ceased in September 1933, and the cost for the next six months was provided by a Scottish benefactor. The Institute does valuable commercial work and in its exhibition galleries provides a panorama of Empire appreciated at least by the schools, which sent nearly 3,000 organised parties during the year It seems rather deplorable therefore that the Colonial Office and the Board of Education cannot each include a part of the small sum required to keep the Institute free from the perpetual threat of bankruptcy in their annual estimates. It must be regarded as unfortunate that the Report itself does not contain a proper financial statement. Confronted with the Director's statement of financial difficulty the reader naturally looks to see how far the payments for the numerous investigations carried out in the Institute laboratories on behalf of various overseas Departments, firms and private persons meet their cost. Did the Secretary for the Colonies pay for the legal advice he asked in connection with mining problems in Kenya, Nyasaland, Fiji, Sierra Leone and elsewhere ? Could not the Intelligence Departments make some moderate charge for their work in connection with the 2,000-odd inquiries put to them during the year ?

The *Report* shows that the Institute was called upon, as usual, to help in solving all sorts of queer problems—the marketing of chilled asparagus from Southern Rhodesia, ghee from Northern Nigeria and goat skins from Somaliland ; the commercial possibilities of hemp rope from Mauritius, geranium oils from Kenya and resins from British Honduras; the quality of hydnocarpus oil (a remedy for leprosy) from Ceylon, " blister beetles " from Khartoum and ipecacuanha root from Malaya—so that the members of its staff cannot complain of lack of variety in their " routine " work. The Institute acts as a clearing house for information concerning the resources of the Empire and anyone using raw materials from a source outside can obtain details of Empire production from the Director—free of charge.

The *Report of the Radio Research Board* for the period January 1, 1932 to September 30, 1933 (H.M. Stationery Office, 2*s*. 6*d*. net), covers the period of reorganisation consequent on the amalgamation of the Radio Research Station at Slough and the Wireless Division of the National Physical Laboratory to form a new Radio Department of the Laboratory under the direction of Mr. R. A. Watson Watt. The personnel of the Board has been changed only by the retirement of Prof. C. L. Fortescue on the completion of his term of office and the appointment of Mr. E. B. Moullin in his place. The Board has however decided that it is unnecessary to maintain large group committees to supervise specific aspects of the research programme and has substituted six very small *ad hoc* committees for the larger ones mentioned in the 1931 Report. Much of the present Report is of a highly specialised character scarcely to be understood by any but those actually engaged in the work. This is, to a large extent, inevitable in a subject of such scope and technicality, yet, after reading certain sections, *e.g.* the discussion of the Ionosphere as a Doubly Refracting Medium, it seemed that without much extra space the account might have been made sufficiently intelligible for it to be understood by the ordinary physicist. There is, indeed, continual reference to the 1931 Report—even the explanation of the notation must be sought for there—and such reference might perhaps serve to elucidate much which is obscure in the 1932–3 Report.

The staff of the Department has been heavily engaged in the intensive study of the ionosphere involved in the programme of the Second International Polar Year which ended in 1933. The complete analysis of the observations made during this period will take some time, but it is clear " that ultra-violet light from the sun accounts for the normal ionisation of the two main regions of the ionosphere and for daily and seasonal variations in its intensity. At the same time there appears to be strong evidence that ' abnormal ' variations in the ionisation of the lower region, which occur mainly at night and during magnetic storms, can probably be best accounted for by the hypothesis that charged particles enter our atmosphere from outside and are carried towards the Poles by the action of the magnetic field of the earth." There is an "indication of a correlation coefficient as high as 0·75 between thunderstorm activity and the increase in ionisation in the lower layer, suggesting that thunder-clouds contribute to the ionisation of the ionosphere, either through intense ionisation currents or lightning flashes."

A matter of considerable interest which is discussed in the report is the manner in which wireless waves are reflected back to the earth by the ionosphere. A knowledge of the complete mechanism of this process is essential in arriving at the whole story of world-wide travel of radio signals. Recent addition to the knowledge of this subject lies in the proof, which has now been obtained, that when a wireless wave reaches the ionosphere it is, in general, divided into two parts by the action of the earth's magnetic field and these two components—two waves as it were—are returned to earth with a rotational spin, one spinning left-handedly and the other right-handedly. Moreover, the two do not generally take the same time to travel back to earth, one being more heavily delayed than the other. The interference between these waves accounts for much of the fading on long-distance communications especially on short waves.

The nature of the soil round the wireless transmitter has an important influence on the efficiency of the station as a source of radiation and in consequence measurements have been made of the conductivity and dielectric constants of samples of soil from various sites and at different frequencies. It appears that the conductivity of the surface soil varies from less than 10^5 e.s.u. for dry soil up to 10^8 e.s.u. (equivalent to a resistivity of 9,000 ohm-cm.) for normal moisture content. The dielectric constant for very high frequencies appears to be about 20.

The Bureau of Standards *Journal of Research* for March 1934 contains a paper by the late Edward W. Washburn and Edgar R. Smith describing an examination of water from various natural sources with a view to the determination of any variations in their isotopic composition. The tests were made with a differential pyknometer capable of measuring differences of density with a precision of one part in a million and the samples of water employed were, of course, freed from all non-aqueous constituents by distillation and other appropriate treatment. The only natural waters found to have an isotopic concentration different from the normal were (*a*) those which have been subjected to evaporation for many centuries, and (*b*) the sap water of a willow tree and water obtained by burning the dry wood. In class (*a*) is the water of crystallisation of kernite which probably represents almost the last water left after the complete evaporation of an inland sea; it is denser than ordinary water by seven parts in a million. Ocean water, water from Salt Lake and from the Dead Sea are denser by two parts in a million, sap water from a young willow tree by 2·8 parts and water from the dry wood by 5 4 parts per million. Among the other samples tested and found to be normal were water from cow's milk and from slaughter-house blood. Lewis and Luten found the density of H^2_2O at 25° C. to be 1·1056 times that of H_2O but Taylor and Selwood (*J. Amer. Chem. Soc.*) obtained a somewhat higher value, 1·1079. Lewis and Hanson have determined the triple point of H^2_2. The temperature is 18·66° K and the pressure 45·40 cm.

The March issue of the *Journal* contains also a paper by J. L. Thomas on the reproducibility of the ice point in which it is concluded that with great care this temperature can be reproduced to within 0·0003° C. or even to 0·0001° C. if corrections are applied for variations in barometric pressure. This latter figure gives also the uncertainty in the triple point. It may be noted that air-saturated ice melts at a temperature 0·0023° C. below that of air-free ice and that the triple point is 0 0074° C. above the melting point of air-free ice at standard pressure.

The April number contains two papers which may be mentioned here. The first by Ralph E. Gould deals with the comparative performance of watches with elinvar and steel hairsprings and shows that the temperature-rate errors and the effects of magnetism are much less in the former case. This work is published separately as *Research Paper*, R P 670, and can be obtained for five cents from the Superintendent of Documents, Government Printing Office, Washington, D.C. It includes two plates which will interest teachers of physics. One shows the construction of the balance wheels and the other the distribution of iron filings round the balance wheels and hairsprings after they have been placed in a magnetic field. They would make excellent lantern slides. The other paper by Van Dusen and Shelton describes a simple guard ring method for the comparison of the thermal conductivities of metals at temperatures up to 600° C. In the May issue Roeser and Wensel describe their determination of the freezing-point of rhodium, using the method previously employed for platinum and iridium. With rhodium, however, it is necessary to use a vacuum furnace because the metal "when melted in air absorbs gas which, during freezing, is released rather violently with the formation of 'sprouts.'" Their final result was 1,966° ± 3° C. Burgess in 1916 found the freezing point to be 1,974° C., Henning and Heuse (1924) obtained 1,958° C. and Swanger (1929) 1,984° C. The rhodium employed was purified by Wichers at the Bureau and probably contained less than 0·01 per cent of metallic impurities. Its resistivity at 0° C. was 4·3 microhm-cm. and the temperature coefficient of resistance between 0° C. and 100° C. 0·00457 per deg. C.

Modern trawling is too often needlessly destructive of fish life. In addition to fish of economic size, large quantities of small fish of little or no commercial value are taken. Many of these are thrown overboard again, but few of them survive; they represent therefore, a useless depletion of the available stocks of fish. Moreover, the quantities of small fish which are put on the market fetch very low prices, and tend to depress the values of the remainder of the catch.

The ideal fishing net would allow all these unwanted small fish to escape, while retaining those of commercial value. Unfortunately the ideal is not attainable, but a step towards it was made in the United Kingdom in July last by the issue of an Order under the Sea Fishing Industry Act, 1933, prohibiting the use of a mesh of less than 1½ inches from knot to knot in the cod-end of trawls used for deep-sea fishing. This has the effect of allowing some of the undersized round fish to escape. With a view to testing the practicability of a still larger mesh, a series of experiments has been carried out by the Ministry of Agriculture and Fisheries with the assistance of Messrs. G R. Purdy Trawlers Limited. The method employed was to fish two sister ships fitted with trawls identical except for the cod-end, one "normal" with 24 rows of knots per yard when in use (the statutory minimum), and one with a larger mesh of 21 rows per yard. Trawling and the sale of the catches were carried out under conditions as far as possible identical.

The results showed that, as anticipated, the larger mesh released more small fish, and also retained substantially smaller quantities of useless material (weeds, stones, etc.). A more interesting and important result of the experiment, however, was that this saving of small fish was accompanied by a slightly larger gross value of catch for the larger mesh (£1,276 against £1,265) and a slightly larger value of catch per ten hours' fishing (£20 3*s*. 10*d*. against £19 12*s*. 9*d*.). In the case of haddock, for instance, whereas fewer fish up to about 11½ inches long were retained by the larger mesh, above that size the position was reversed.

ESSAY REVIEW

MORPHOGENETIC FIELDS. By C. H WADDINGTON, M.A., Strangeways Research Laboratory, and Sub-department of Experimental Zoology, Cambridge. Being a review of **The Elements of Experimental Embryology**, by J. S. HUXLEY and G. R. DE BEER. [Pp. xiv + 514, with 221 figures.] (Cambridge: at the University Press, 1934. 25*s*. net.)

As physiology and biochemistry grow more and more successful in discovering the mechanisms involved in animal functions, so does the problem of form increase in importance for biology as a whole. For nearly all the processes which have been discovered can only take place in a mechanism with a structure. The transport of oxygen by the blood depends not only on the chemical properties of hæmoglobin, but also on the lungs, diaphragm, heart, blood-vessels, sympathetic nervous system, adrenal bodies, etc. The obvious question arises, how did this complicated mechanism come to be there? It is not sufficient merely to say that the body probably functions like a physicochemical machine without giving some account of its origin; all the comparable machines we know of are man-made.

The study of this question of morphogenesis, or the development of structural form, is the province of experimental embryology. Huxley and de Beer have recently performed the very great service of collecting and systematising the enormous mass of facts which have been discovered. It is actually the systematisation which they have attempted which makes their book peculiarly important. Most of the facts, except the very newest, were collected a few years ago by Schleip in his *Determination von der Primitiventwicklung*, but that book is avowedly an encyclopædia, with no general theoretical outlook. The only book of comparable size in English is Morgan's *Experimental Embryology*, which again failed to marshall the facts into any semblance of a coherent story, missing the general importance of some of the recent work from this point of view.

The theoretical basis of Huxley and de Beer's exposition is derived from a union of two recent lines of work: on the one hand, the strictly embryological researches of Spemann and his school, which have issued in the theory of the organiser, and on the other, work on regeneration leading to the cognate ideas of axial gradients (Child) and morphogenetic fields (Weiss). Huxley and de Beer associate these notions in the concept of gradient-fields, and show how widely such a concept can be applied. From the facts which are already known, they have been able to deduce a series of laws of field action which hold in the most diverse organisms (Chaps. 8 and 9).

Such synthetic theoretical work is of the greatest importance for the progress of experimental study of morphogenesis at the present day, and the labours of Huxley and de Beer, if only for their comprehensiveness, mark a considerable advance on anything which has existed before. But unfortunately their conceptual scheme does not seem to have been as fully worked out as has its application to the facts We could have spared some of the experimental details if room could have been found for a more thorough and precise discussion of the meanings of the words used. The authors have used the field concept with such success that it is assured of a place in the present-day theoretical scheme, but they have not made it quite clear just what they mean by the word "field." It is well worth while inquiring what meanings have been, or can be, attached to this word in embryological theory, and attempting to decide which meaning can be most usefully adopted.

One ambiguity in Huxley and de Beer's use of "field" is obvious after a very cursory reading. In some contexts the field is thought of as actually affecting or limiting the differentiation of tissues within it; in others it merely means a place where something is happening and only implies location. The second use seems to be unnecessary and confusing, but it is common throughout the book, particularly in the first half. Thus one of the two references to Field in the Index refers to the following passage (p. 221): "The determination and localisation of organ-rudiments is revealed sooner or later by the presence of chemo-differentiated material or morphogenetic substances in certain places which constitute what may be called *fields*, or areas of differentiation of organs." A sentence later there is a discussion of the "presumptive fore-limb area, or field." It would lead to a great increase in clarity if the word field was abandoned in this connection, and such a word as area, or region, used to mean location without implying any formative agency. There is, as a matter of fact, great need for some careful thinking on the various possible sorts of region which must be considered. In respect of any given organ, there are for instance, the region from which it normally develops (presumptive region); the region from which it will develop if the presumptive region and surrounding tissue is extirpated; the region isolated parts of which will develop into the organ; the region from which the organ can be formed in response to a specific, or a non-specific stimulus, and so on. It would have been a considerable advantage, if the authors, before attempting their comparative work, had fashioned a suitable vocabulary for all these different entities.

It is also necessary to discuss, if only in a preliminary way, how the word field can be used to denote an agent affecting differentiation. The idea of morphogenetic fields has been extensively discussed by Gurwitsch and we may begin by considering a simple example to which he (1921) has drawn attention The mushroom develops into a hat-like plant with a fairly definite shape, subject of course to the slight variation around the normal which is found in all biological shapes Internally the mushroom consists of a felted mass of hyphal threads, interlacing in all directions. This apparently homogeneous internal structure gives no hint as to the reasons for the external shape. Gurwitsch suggested that we can account for this shape by supposing that there is a " morphogenetic field " which causes the mass of threads to form a hat-shaped body. But this hypothesis as it stands is merely a description of what we see ; and it can only be justified by showing that it is a useful method of description.

Anikin (1929) has described in greater detail a case where the concept of a field provides a useful descriptive method He studied the shapes of the nuclei in transverse sections of the cartilage of developing amphibian limbs, and found that near the centre of the mass of cartilage the nuclei are circular in outline, while farther away from the centre they become oval and finally kidney-shaped or bean-shaped. He was able to discover a mathematical expression for these changes of shape, such that it is possible to calculate the shape of a nucleus if its distance from the centre is known. Here, then, we are dealing with a region, the mass of cartilage, throughout which a certain mathematical expression holds. We can adopt the usual terminology of physics, and speak of the region as a field and the mathematical expression as the field law. Clearly, this field and its field law is a convenient way of summarising the descriptions of all the nuclei in the mass of cartilage.

It seems easiest to suppose that such field laws describe certain positions of equilibrium. We can imagine that there are interactions of some kind between the nuclei and their surroundings, perhaps there is a substance which affects their shape diffusing outwards from the centre of the mass of cartilage, and the field law then describes the shape of a nucleus which is in equilibrium with its surroundings. Such a hypothesis recognises clearly that the field concept is being used only descriptively. It does not suppose that the discovery of the field has enlightened us as to why the nuclei have their characteristic shapes ; the determination of these shapes is still ascribed to unknown interactions with the surroundings. But the hypothesis that the field describes an equilibrium position, does imply that there should be a tendency for the system to return to the same position if it is disturbed.

It is well known that such tendencies exist and are manifested in the phenomena of regeneration. If part of a newt's leg or arm is cut off, the wound closes, and a little cap of cells, the regeneration bud, appears as a swelling under the new skin This regeneration bud grows and develops into the missing part of the limb, which is thereby restored to its normal shape The process can be regarded as a return to a state of equilibrium. When we are dealing with the regeneration of an adult organ such as a newt's limb, the equilibrium towards which the regeneration takes place is a stable one ; once the whole limb is regenerated, the change stops, except perhaps for growth changes. In the regeneration of embryonic organs this is not so, and regeneration takes place towards an equilibrium which is constantly changing with the development of the embryo, so that the regenerated organ still continues its normal development after becoming restored to its normal complete state.

It is noteworthy that a given fragment of an organ may be at liberty to pursue a number of different paths towards its equilibrium condition. Dragomirov (1933) has recently described a good example. He showed that if small fragments of the first rudiment of the newt's eye are isolated, they regulate themselves to form a whole eye ; that is to say, they regenerate the missing part by changing a part of themselves into it. Dragomirov describes several different sets of changes which can occur, all of them leading to the same end-result, the formation of a complete embryonic eyecup.

The regeneration bud of the newt's leg is worthy of further consideration, since it has been very fully investigated and is probably typical of a common method by which regeneration is brought about. (It is fully dealt with by Huxley and de Beer, where references are given.) The bud is at first entirely unspecific and can become a tail or an arm instead of a leg. If, for example, it is removed from the leg soon after it has appeared, and is transplanted to the stump of an amputated arm, it will contribute to the regeneration of the arm. Its equilibrium in the arm field is therefore different from that in the leg field. These arm and leg fields can be carried by very small pieces of tissue. For instance, Weiss has performed the following *experiment. He amputated the distal half of a leg and grafted part of an arm on to the wound. The wound healed up well and the presence of the arm, covering the cut end of the leg, prevented any regeneration. Then, after the wound was perfectly healed he cut off most of the arm section, leaving only a small disk of arm attached to the stump of the leg. The regeneration formed from this* leg + tiny disk of arm, was an arm and not a leg ; thus the fate of the regeneration bud was determined by the tissue which lay immediately near it. Weiss also showed that the whole equilibrium is inherent in any part of the field. He amputated the leg and also removed a longitudinal half of the stump. The stump does not regenerate the missing longitudinal half, presumably because in the adult state each longitudinal part of the limb is in equilibrium within itself. But the regenerate bud at the end of the stump developed into a distal end with a complete normal cross-section. Therefore the conditions determining the complete cross-section are inherent in the longitudinal half, and can be manifested by the "embryonic" regeneration bud.

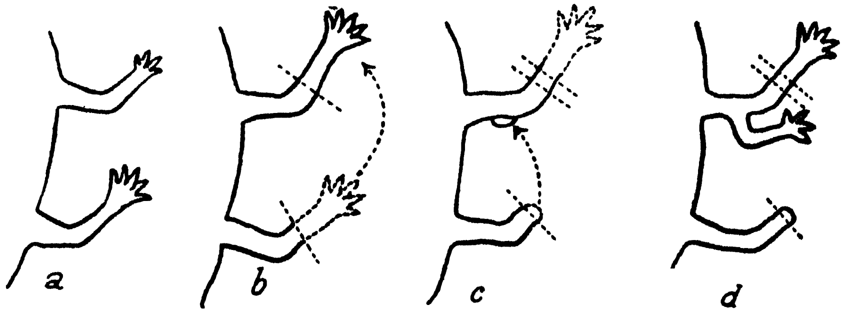

FIG. 1.—Diagram of two regeneration experiments of P. Weiss.

a, the right side of the body of a newt, with four-fingered arm and five-fingered leg In *b* both limbs have been amputated and the foot transplanted to the stump of the arm In *c* the transplanted foot is amputated, leaving only a small disk of leg attached to the stump of the arm , the regeneration bud from the leg has also been transplanted to the side of the upper arm. *d* shows the regeneration of the transplanted leg to form a five-fingered foot, and the development of the leg regeneration bud after transplantation into the arm region to a four-fingered arm.

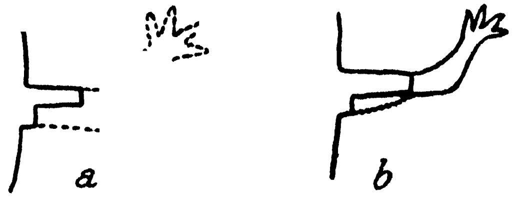

FIG. 2.—Regeneration by a longitudinal half of the arm in the newt (*after* Weiss).

The operation is shown in *a*, the dotted part being removed The regenerated arm is shown in *b*.

Paul Weiss does not use the field concept merely to describe the conditions of equilibrium to which a system tends to return when it has been disturbed in various ways. He invokes a morphogenetic field as a causative agent, determining and moulding the structures developing in it. He defines the morphogenetic field as follows (1928, p. 1569): " Die organisierte—wenn ich mich so ausdrücken darf—' Ladung,' welche dem organisierten Material im Gegensatz zum unorganisierten, wenn auch lebenden, zukommt, wollen wir als sein ' Feld ' bezeichnen. Es ist das nichts anderes als der Inbegriff von organisatorischen Wirkungen, die von einem solchen organisierten Material auf seine eigenen Teile und auf fremde ausgehen, dynamisch als Einheit aufgefasst." (We will refer to the " organisation charge "—if I may so express myself—which is possessed by organised, as opposed to unorganised but living, material, as its " Field." This is nothing other than the concept of the organising actions, which are exerted by such organised material on its own parts and on other material, considered as a dynamically potent entity.) I do not find this definition very clear; Weiss seems to equate the degree to which a piece of tissue is itself organised with its power to organise its surroundings. But, be that as it may, his discussion makes it clear that he regards the field as the actual causative agent involved in the determination of the regeneration bud to become a leg or an arm. Most other authors who have discussed biological fields finally agree with Weiss in attributing an active formative role to such fields. For example, Bertalanffy (1928, quoted in the translation by Woodger, 1933) says, discussing the field of a flower head which had originally been analysed by Gurwitsch: " There thus exists a state in space distinguished by the fact that it constitutes a ' direction field ' for flowers, forming them into paraboloid cups. . . . This field is not a physical field of force but rather a stimulus field. . . . We may assume that (*e.g.* in *Matricaria*) the field factor signifies an impulse proceeding from the depths of the plant to the flower bud and influencing cell-division and cell-growth." The final sentence of this quotation occurs word for word (with the substitution of " Mit Recht wird auch angenommen " for " Man darf annehmen " in the original German) in the discussion by Rudy (1931, p. 21), although Rudy had stated on page 20 in the same work that " the field is always to be thought of as a geometrical, not as a physical concept." Huxley and de Beer do not make their attitude clear on this point. As they use the word " field " with such a wide connotation that it includes areas, they do not differentiate between the various more subtle shades of meaning. But sometimes they, too, give the impression that a field is able to compel tissue within it to assume a certain shape.

Weiss refers to the determining influence of the blastopore region in the amphibian gastrula (Spemann's organisation centre) as an example of a field reaction, but Huxley and de Beer, with the more recent work to guide them, are more cautious. Let us see whether this assimilation of the concepts of the organisation centre and morphogenetic field is useful. The main outline of the phenomena involved in embryonic induction by the organisation centre is well known ; it is shown that the type of differentiation undergone by the ectoderm of the gastrula depends on its position relative to the organisation centre, which in normal development is situated near the blastopore The most crucial experiment (Spemann and H. Mangold, 1924) involves the transplantation of the blastopore region of one embryo into the belly or some other region of another embryo. The grafted " organiser " is then able to induce the formation of a secondary embryonic axis, and particularly a neural plate, by the surrounding ectoderm of the host embryo Such induced embryos may be surprisingly complete, with head, trunk, tail and so on, but more usually it represents a more limited region of the body. If a large number of cases of induction are examined, it will be found that the induced axis always lies in immediate contact with the grafted organiser, and is clearly caused by this organiser alone. On the other hand, the determination of the regional character of an axis, that is to say, the determination whether it shall be a head or a trunk or a tail, is often influenced by the position of the axis with respect to the host embryo. For instance, if a certain organiser is implanted into the anterior part of the host's body, it will induce a head, but if the same organiser is implanted into the posterior part of the body near the tail, it will not induce a head but a tail. If we are going to apply the field concept, we must consider at least two fields, the field of the grafted organiser and the field of the host embryo. Moreover, there must be two more or less separate processes involved, since the host field has no effect on the position of the induced axis, but only on its regional character. The complex process of induction may be separated into two, as " Evocation," the determination that a new embryonic axis shall be formed ; and " Individuation," the determination of the regional character of such an axis (Needham, Waddington and Needham, 1933). We must examine the application of the field concept to each of these processes separately.

A field has necessarily a structure ; but it has recently been shown that the organisation centre does not lose its power to evocate when its structure is destroyed. Spemann (1931) first showed that organisers in which the crude structure was destroyed by crushing could still evocate ; then Bautzmann, Holtfreter, Spemann and

Mangold (1932), and Waddington (1933) showed that even the more drastic treatment of boiling did not damage the efficacy of the evocator; and finally, Spemann, Fischer and Wehmeier (1933), Holtfreter (1933*b*) and Waddington, Needham and Needham (1933) have all obtained active cell-free extracts of organiser-tissue. Evocation, in fact, seems to be due to a single chemical substance, which Waddington, Needham and Needham, have shown to be soluble in ether and probably related to the sterols. It is therefore unnecessary, if not impossible, to apply the field concept to the process of evocation.

Much less is known about the process of individuation. Spemann (1931) rather hesitatingly suggested that the determination of regional character might be carried out by the same agents as are responsible for the regulation, which occurs in many animal groups, of a half egg to form a whole embryo. This suggestion has been developed in terms of the field theory by Waddington (in Waddington and Schmidt, 1933). It seems that the region round an organisation centre is such that if the material lying within this region is not too abnormally arranged, it tends to form a single embryo. Using the idea of Paul Weiss, we could say that this single embryo is moulded by the action of a field of "morphogenetic force." But we have seen that the best known active causative agent which we know of in embryology, the evocator, is not a field of any sort of force, but is a single substance. For this reason, and to avoid the necessity of postulating a pervasive moulding force, it is better to regard the field as a means of describing the equilibria attained through the mutual interaction of the contiguous pieces of material lying within the region under consideration, and to acknowledge that the nature of these interactions is still unknown. If the distribution of material is very irregular, the equilibrium may not be a single embryo, or rather, it may be impossible for that equilibrium to be attained, and another inherently less stable one may be substituted. Thus if a large induced neural plate is lying near the head of a host embryo, the ideal equilibrium is for it to be incorporated and built into the head of the host. But if it is too big, or lies too far away, it may develop into a head but fail to be incorporated into the host head (Waddington and Schmidt, 1933). We can say then, using a convenient short locution, that the hosts "individuation field" has individuated the foreign neural plate as a head, but this is only justified if it is remembered that this locution really means only these two things, that the equilibrium state for a neural plate in that situation is to be a head, and that this equilibrium is at least partly determined by the position of the host embryo.

The comparison of the organisation centre with a morphogenetic field is therefore only justified as regards its individuating action. The process of evocation is due to the action of a specific chemical substance. This substance is indeed usually distributed in a diffusion field, but this diffusion field has nothing in common with a morphogenetic field moulding the form of the induced neural plate. The evocation is due to the mere fact that the evocator substance is present and is not dependant on the field in which the substance is distributed.

If an equilibrium is disturbed by the addition or subtraction of material, the mutual interactions of the parts of the new system usually leads to an equilibrium similar to the old one. For instance, if part of a leg is removed, the mutual interaction of the parts of the stump leads to the formation of a new limb similar to the original one. But this restoration of the original condition does not always occur. Child and his associates (summary in Child, 1929), working chiefly with various species of invertebrates, have shown that in many cases the actual equilibrium position towards which regeneration takes place is dependent on the nature of the system which is isolated. For example, in some species of *Planaria*, transverse segments will only regenerate a pharynx if they contain a head, either the original head or a regenerated one. From facts of this kind, Child was led to postulate the existence of gradients along the main axes of such animals, such that in the example given above, a pharynx would only be regenerated if there were present a certain high level of the gradient found in the prepharyngeal region. This could be expressed by saying that if the regenerating system includes the high level, this level can only be accommodated within an equilibrium of a certain type, whereas if no such high level were present, there could be various other equilibria, which do not include the presence of a pharynx. This hypothesis of Child's has many applications and seems to be a valuable advance in describing the character of one of the commonest types of field. It states that the formal properties of the forces tending towards equilibrium within many fields can be described by the analogy of a gradient or gradients along the main axes of the field.

Child has carried his theory farther and attempted to determine what these gradients are gradients of. This is quite a separate question from the question as to whether there are gradients at all. Child considers that they are gradients of metabolic rate or respiration, but recent surveys (*e.g.* Needham 1931) suggest that the evidence for this view is doubtful for adult animals and for embryo almost non-existent. In any case it seems certain that the equilibria which we are here discussing are not *only* equilibria between regions with different rates of respiration, though they may be so in part. The primary organisation centre, or rather the primary evocator, in the amphibian egg is one example of a morphogenetic agent which is not itself a rate of respiration, and it is probable that such agents play a large part in the interactions whose equilibria are described by the field laws. The agencies at work are probably numerous and complexly interdependant Lehmann (1933) has recently emphasised the complex nature of the various part-processes which work together in the formation of the neural plate in normal development in the amphibia ; he claims that induction by the organisation centre is only one element in the whole process. Huxley and de Beer emphasise the same point. It is possible that they represent the process as even more complex than it really is ; but Lehmann's conception of the co-ordinate formation of a unit ("koordinative Einheitsbildung," "das synergetische Princip ") almost certainly applies to the reactions going on in the fields which we are considering.

Summarising the argument of this article, it may be said that the field concept is most properly to be used as a method of description. A field is a region of space within which certain reactions in which we are interested are going on. The field concept is useful when we can find field laws which describe the conditions under which these reactions are in equilibrium. The nature of the reactions is a subject for future investigation ; but the analysis of the morphogenetic reaction of evocation has shown that the diffusion of specific chemical substances may be of great importance. We hope eventually, not merely to describe the stable configurations by field laws, but also to be able to say why those configurations are stable.

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REVIEWS

MATHEMATICS

Elementary Calculus. Vol. II. By C. V. DURELL, M.A., and A. ROBSON, M.A. [Pp. xii + 308, with 62 figures.] (London: G. Bell & Sons Ltd., 1934. 7*s*. 6*d*. with appendix, 6*s*. 6*d*. without appendix.)

THE first volume of this text-book contained the matter needed for "additional mathematics" or "mathematics more advanced" in examinations of school certificate standard. This second volume deals in full with work of higher certificate standard and covers many of the special requirements of distinction papers. It also contains those parts of the subject required especially by physics and engineering students.

The explanatory matter is clear and full. There is a large number of examples throughout the book, while the appendix contains further examples for each section.

E. M. W.

Principles of Geometry. Vol. V: Analytical Principles of the Theory of Curves. Vol. VI: Theory of Algebraic Surfaces and Higher Loci. By H. F. BAKER, Sc.D., LL.D., F.R.S. [Pp. x + 247 and x + 308.] (Cambridge: at the University Press, 1933. 15*s*. and 17*s*. 6*d*. net.)

THESE are the concluding volumes of Prof. Baker's important work on the principles of geometry. These two volumes lead up to, but do not attack, the modern invariantive theory of surfaces and algebraic loci in general. They provide a useful introduction to this theory and contain an account of the fundamental notions which are needed for an understanding of the subject. The compiling of the whole six volumes must have involved an enormous amount of work and mathematicians owe a debt of gratitude to the author for this valuable contribution to the expository literature of the subject.

Vol. V contains an account of the analytic principles of the theory of curves, of the rational functions belonging thereto and of the methods of loops and of Riemann surfaces used to find integrals of these functions. In the succeeding volume is a discussion of the theory of correspondence and the theory of surfaces in general.

E. M. W.

Statics. A Text-book for the use of the Higher Divisions in Schools and for First-Year Students at the Universities. By A. S. RAMSEY, M.A., President of Magdalene College, Cambridge. [Pp. xi + 296.] (Cambridge: at the University Press, 1934. 10*s*. 6*d*. net.)

IN the early chapters of this book the author sets out to present the subject in as simple a way as possible, and with very detailed explanations. In this essay he has undoubtedly been successful. The first six chapters, dealing with forces in one plane, and leading to the solution of problems on smooth uniplanar statics, contain an admirably clear and detailed exposition of this part of the subject, and while well suited to beginners, contain much that is well worth reading by those who have already some knowledge of statics. In the chapters on Centre of Gravity and Virtual Work, however, two branches which are generally considered to come within the scope of "elementary" statics, a degree of familiarity with the methods of the calculus is assumed which, I am afraid, is rarely found in students until well after the Intermediate stage. Thus, while in these sections the book may be too advanced for the individual study of such students, it may, if taken as a guide by teachers, lead to the introduction of the calculus at an earlier stage than usual: a result which would be to the great advantage of both student and teacher.

The later chapters, dealing with Stability, the Equilibrium of Chains and Strings, Elasticity, and Forces in Three Dimensions, are suitable for more advanced students, and they complete a course which is quite adequate for a general or pass degree.

The examples which are worked out in the book are many and they are very well chosen to illustrate and explain the mechanical principles involved without introducing an excess of algebra or geometry, while nearly 500 examples are given for solution.

The book should prove of great value both to students and to teachers of applied mathematics.

H. T. J.

Introduction to General Topology. By W. SIERPINSKI. Translated by C. C. KRIEGER. [Pp. x + 238.] (Toronto: The University of Toronto Press; London: Humphrey Milford, 1934. 17*s*. net.)

THIS is a translation from the Polish of the second volume of Professor Sierpinski's *Theory of Aggregates*. The first volume on *Transfinite Numbers* has been translated by the author into French and published in the Borel series under the title *Leçons sur les nombres transfinis*. The translator has added to the present volume an appendix providing a brief and very interesting summary of the first volume. This appendix contains an excellent account of the theory of transfinite numbers.

Prof. Sierpinski's achievements in the field of the theory of sets are well known and it is good to have this work of his available in English. "The theorems of any geometry follow, as is well known, from a series of axioms, *i.e.* hypotheses about the space considered, and from accepted definitions." In this book axioms are introduced gradually and in each of the seven chapters those theorems are proved which can be deduced from the axioms of that and preceding chapters. In the first chapter is considered an abstract space, which is a set K of any elements, and the only assumption made is that certain parts of the space considered are called open sets, and that open sets have certain simple properties. From these suppositions and the corresponding definitions a series of conclusions is deduced. In each of the following chapters new axioms are added and a new series of theorems deduced.

E. M. W.

Continuous Groups of Transformations. By L. P. EISENHART. [Pp. ix + 301.] (Princeton: Princeton University Press; London: Humphrey Milford, 1933. 19*s*. 6*d*. net.)

THERE are very few books on continuous groups, although the subject is most attractive in itself and of increasing importance to specialists in other branches of mathematics. Here we have a full account of the basic ideas of the subject written by an expert.

The book opens with three preliminary sections on the fundamental existence theorems. The remainder of the first chapter considers a set of transformations satisfying the usual axioms as a group and discusses groups depending on a finite number of parameters. The first three chapters contain in the main the results due to Lie and his contemporaries. Chapter IV is devoted to the theory of the adjoint group and of semi-simple groups, mainly due to Cartan, Weyl and Schouter. In the fifth chapter we find the geometrical applications of the theory and, in particular, the concepts of the new differential geometry. Contact transformations and their applications to geometry and mechanics are discussed in the final chapter.

E. M. W.

Mathematical Facts and Formulæ. By A. S. PERCIVAL, M.A., Trinity College, Cambridge. [Pp. v + 125, with 6 figures.] (London: Blackie & Son Ltd., 1933. 4*s*. 6*d*. net.)

THIS book is described as a "manual for laboratory workers" and it does indeed contain much of the mathematical material required by the chemist or physicist whose main interest is in experiment and whose mathematics, once well known, has become somewhat stiff from infrequent use. The title is a little misleading in that the book is not restricted to lists of formulæ but gives also simple proofs.

Briefly the contents are (i) algebraic formulæ including cubic and quartic equations, (ii) analytical geometry (very brief), (iii) circular and hyperbolic functions, (iv) finite differences and observational equations, (v) differentiation and integration, (vi) differential equations (including simple partial equations). Among the tables is one giving log $\Gamma(p)$ from $p=1{\cdot}000$ to $p=1{\cdot}999$.

There is no index and, what is more important, no chapter devoted specifically to complex quantities and formulæ dependent thereon.

D. O. W.

PHYSICS

Experimental Elasticity: a Manual for the Laboratory. By G. F. C. SEARLE, Sc.D., F.R.S. Second edition. [Pp. xvi + 189, with 63 figures.] (Cambridge: at the University Press, 1933. 7*s*. 6*d*. net.)

THIS book has established itself so firmly in the physical laboratory since it first appeared in 1908 that to praise it now or even to describe its contents would be ridiculous. There is, however, one important addition, namely a Note giving the usual simple theory of the infinitesimal uniform bending of a uniform beam. The treatment in Chapter II, which applies to a small but finite bending, has always proved puzzling to the beginner who inevitably failed to see why "Searle's method" was so much more difficult than that which he found elsewhere. The change adds three pages just before the index, otherwise the pagination is unaltered.

D. O. W.

Introduction to Theoretical Physics. By JOHN C. SLATER, Ph.D., and NATHANIEL H. FRANK, Sc.D. International Series in Physics. [Pp. xx + 576, with 83 figures.] (New York and London: McGraw Hill Book Co. Inc., 1933. 30*s*. net.)

THIS book is a recent addition to the International Series in Physics, with which the English reader is familiar on account of its important works on Atomic Physics. These have taken a prominent position on our bookshelves as being amongst the most complete and authoritative accounts of modern theories. The volume which we have now to review is not wholly concerned with recent developments; more than one-half is written upon classical theories.

The authors tell us in the preface something of the history of the development of the volume and one sees, perhaps with a little envy, that, on the other side of the Atlantic, a teacher may break away from conventional methods of presenting his subject. Evidently the dictatorships of Boards of Examiners are not taken too seriously over there—not at any rate by our authors.

The division of Physics into Heat and Light and other sections is here disregarded. The aim of the book is to demonstrate the unity of the subject by applying the same principles in the different branches. The unity is traced through the mathematical form of the notation. This method of teaching shows very well the place of Mathematics in Physics. To quote a famous writer: "Mathematics is the librarian whose duty it is to collect the works, to place them in their proper places and to show what gaps remain upon the shelves."

To come to some detail of the book; one sees that there is a tendency towards modern developments and the subject matter is treated in such a way as to take the student easily into the Quantum theory. Thus the relevant branches of Mechanics are brought to the front and classical wave problems are treated in considerable detail.

The subjects studied in the classical part of the book are those with which students of advanced Physics in this country are expected to be acquainted; but, naturally, from the method of treatment and from the size of the volume some parts of the conventional branches of the subject are omitted. A little less than half of the volume is taken up with modern applications to atomic structure, to the quantum theory of a gas, to statistics, collisions and so on.

In the second chapter the introduction to power series is rather laboured. A simple equation for a body falling in a viscous medium is taken and solved by means of a series method. The example serves well as an illustration, but, unfortunately, doubtless by a slip of the pen, the authors say that the equation cannot be solved by direct integration. Having now adequately illustrated the method it seems redundant to take, in the next chapter, the equation of simple harmonic motion, solve it by series and recognise the solution as the sum of the sine and cosine series.

But in general the treatment is clear and the book well worth recommending to students, while teachers will enjoy a method of presenting the subject which they must occasionally have wished to follow.

H. T. F.

The Crystalline State. Edited by Sir W. H. Bragg, O.M., K.B.E., D.Sc., F.R.S., and W. L. Bragg, D.Sc., F.R.S. **Vol. I: A General Survey.** By W. L. Bragg. [Pp xiv + 352, with 187 figures, including 32 plates.] (London: G. Bell & Sons Ltd., 1933. 26*s.* net.)

The appearance of this book marks a stage in the development of crystallography in this country. It means, among other things, that the products of some twenty years' research on the part of workers in various institutions have become sufficiently definite, and their implications sufficiently realised, to enable such a general survey to be not only acceptable, but eagerly awaited. Those engaged in original investigations, teachers and advanced students, will all find in it much to satisfy their needs.

To begin with, one must notice that this is the first of three volumes, but that, at the same time, the intention has been to make it complete in itself; in fact the reader is given as detailed a view of the crystalline state as the size of the book permits. But much more than "a little of everything" is provided, and, for the most part, the treatment and discussions are remarkably full and readable. Some little disappointment may be experienced that no mention whatever appears to be made (in Chap. VII particularly), of the contribution which infra-red studies have made, not only to crystal chemistry, but even to crystal structure. That they can serve as an illuminating check upon standard methods has been stressed by Schaefer, with considerable experimental and theoretical support. It is true that we may get all this in the last volume; meanwhile it seems a trifle strange to omit these topics now from an otherwise detailed account of the physical chemistry of the solid state and its relationship to structure.

The authors' work in unravelling the inner nature of the silicates is known to all crystallographers, who will be grateful for this clear account not only of this highly specialised subject, but also of its application and extension to become, in effect, the foundations of a Systematic Inorganic Chemistry. In some ways the ideas here described are analogous to Kekulé's far-reaching postulate of the tetravalency of carbon, and have, in like manner (in conjunction with Pauling's conceptions) been quite as significant for the inorganic world as his were for the organic. Important and interesting as technique in all its branches may be, it is these pages of broad generalisations and new vistas which will strike the reader with admiration. In a word, they show what the new crystallography can accomplish for physics, for chemistry, for metallography—pure and applied—and for the biological sciences.

In another direction, the later chapters will make a distinct appeal: they deal with a number of points in pure physics for which studies of the finer details of the crystal lattice and its properties have proved exceedingly fertile. Such is "zero-point-energy," which now appears as a quantitative concept in terms of atomic vibrations. Again, in the language of wave-mechanics, atoms, like electrons, cannot be said to be definitely "localised"; their zero-point-energy registers the probability that they will be found near their mean position.

The book concludes with a number of appendices, all of which will be valuable for reference. Incidentally the short summaries at the ends of many of the chapters (especially those concerned with analysis) are a welcome feature in that they provide just that modicum of condensed information which the non-specialist needs to carry away with him after a first reading.

Perhaps it is scarcely possible to do justice to the whole work from a consideration of Volume I. Those who are interested in special problems (and especially honours students grappling with the details of interpretation) will look forward to finding solutions of their particular difficulties when the remaining parts appear. Meanwhile, here is a store of information, which should go far to add recruits to the body of workers engaged in the new crystallography.

F. I. G. Rawlins.

Practical Acoustics for the Constructor. By C. W. Glover, A.M.Inst.C.E., M.I.Struct.E., L.R.I.B.A. [Pp xi + 468, with 193 figures and illustrated Appendix.] (London: Chapman & Hall Ltd., 1933. 25*s*. net.)

As its title implies, the chief appeal of this book is to the constructor. Within the past few years a series of books has been published dealing with the subject of architectural acoustics, some mainly from the architect's point of view, with the emphasis on design, and some from the physicist's point of view, treating the matter as a scientific problem. Mr. Glover's book, to quote his preface, sets out to be "a convenient vade-mecum or handbook for the student and the practical man interested in architectural acoustics." It is difficult to be equally successful for both classes and the book will make most appeal to the man interested in practical details, of which it contains a wealth hitherto unpublished.

While the book deals with the whole range of building acoustics the particular method of presentation by a large number of illustrated examples of successful practice is of special value in the case of sound deadening, where the data obtained in different laboratories are so difficult to compare owing to the different circumstances of their measurements, and where the immediate application of the underlying principles is often far from obvious.

Of particular interest and value are the chapter on Acoustical Specifications, Appendix III giving absorption coefficients over a wide range of frequencies for some seven hundred materials, and Appendix IV giving full-size illustrations of a representative range of special acoustic materials obtainable in Great Britain, together with a summary of their physical properties and of suitable methods of application and treatment. From the photographs it is possible to see at once whether any given material is suitable for incorporation in a particular design.

Heavy type is too freely used throughout the book for it to have its full value as a means of emphasis and selection, and confidence in the book generally is diminished by the inclusion in this type of such a careless statement as "the period of reverberation varies directly *as the dimensions* of the room" (italics ours) and such a debatable one as "The ideal absorbent has a level straight-line graph showing no selectivity." There is a general absence of exercise of the critical faculty on the matter presented. If a careful selection of the material to be included had been made this would matter the less, but where the treatment is so full and the reader is presumably not qualified to assess the comparative value of the different matter presented to him the absence is serious.

Fig. 83 purports to show "a range of plan shapes which have been made acoustically satisfactory by appropriate treatment." Actually it is a range of plan shapes, some of them acoustically atrocious, submitted in competition for the new League of Nations Assembly Hall at Geneva; its misdescription is bound to give a constructor the impression that it is quite immaterial what shape his plan is—that can all be set right afterwards.

Something seems to have gone wrong with the figures quoted on p. 69. The reverberation times for the Queen's Hall, using the W. C. Sabine and Eyring formulæ, are given as 1·70 and 0 95 seconds respectively. But, using the curves on page 66, it is clear that for the given average absorption coefficient of 0·39 the difference should be far less than this, and the reader is completely puzzled as to which of the formulæ to choose, although the author has previously apparently recommended Eyring for such a case.

There is so much in this book that is dealt with more fully and illustrated more lavishly than in any previous textbook on the subject that we are reluctant to dwell on its defects which are partly at least due to the attempt to cater for incompatibles, the practical man who presumably likes an orator that pounds the desk and uses graphic if crude illustrations, and the student who prefers to have the available material presented to him less spectacularly and is impressed by the weight of evidence more than the blackness of the type.

G. A SUTHERLAND.

Colour Science. Part II: Colour Measurement and Colour Harmony. By WILHELM OSTWALD, Sc.D. Authorised Translation by J. SCOTT TAYLOR, M.A. [Pp. xii + 173, with frontispiece, 2 plates and 18 figures.] (London: Winsor & Newton, Ltd., 1933. 10*s*. 6*d*.)

IN this conclusive volume, Dr. Ostwald gives us in full detail the methods by which he arrived at his colour standards. He begins by describing the instrument used for their measurement by what he calls the Polarising Colour Mixer. There is then an exhaustive chapter on the chemistry of pigments, followed by an interesting analysis of the different binding media.

But perhaps the most significant passage in this book is the one (on p. 14) in which Dr. Ostwald describes the *Indirect Process* of the measurement of hue. He says: "For every colour there is one *complementary* colour in existence with which it can be mixed to form a neutral grey." This sentence is, I think, the key to Dr. Ostwald's theory of harmony. When any two complementary colours are mixed, the fact that neutral grey is the result proves that these two colours *are* complementary. The value of such a simple process is illustrated by his assertion on page 148 that a sufficient variety of colour schemes can be arrived at to decorate each room of a whole city with its own colour scheme without ever repeating it.

As to the vexed question of Discords, Dr. Ostwald recognises their significance but shrinks from their introduction into a picture or woven fabric because, he argues, unlike a discord in music, they can never be resolved by a subsequent harmony, colour arrangements not possessing, as music does, the temporal element. But surely a picture or decoration is only fully appreciated when the eye takes it in as a whole so that any discord that the artist has thought fit to include in it *is resolved* by its own intrinsic value in accentuating the harmony of the whole.

Chap. IX treats of colour as a means of representation. This opens up a very interesting field. Colour symbols, as a means of distinguishing differences of meaning, are of course already used for many purposes, but there is great scope for their further development as a substitute for formal or phonetic symbolism.

One hopes that with such a complete survey of the study of colour as this volume and the two preceding ones give, our artists, designers and architects will be encouraged to still greater efforts to make our world a more beautiful one.

F. O.

Modern Thermodynamics by the Methods of Willard Gibbs.

By E. A. GUGGENHEIM, M.A. [Pp. xvi + 206, with 10 figures.] (London: Methuen & Co., Ltd., 1933. 10*s*. 6*d*. net.)

THE author of this excellent monograph must enjoy a measure of satisfaction, rare these days, in the knowledge that he has produced something of permanent value. As indicated by the title, his intention is to demonstrate the elegance and power of a special thermodynamic technique devised by Willard Gibbs. Many generations of students have been exasperated by the barbaric methods almost invariably employed in textbooks for deriving delicate thermodynamic relationships. Mr. Guggenheim has swept all this away, the cycles, reservoirs, cylinders, pistons, and the whole box of tricks. In place of them we are presented with a clear-cut application of the calculus which leaves little room for ambiguities or doubts concerning the significance of the operators. All types of equilibria are controlled by the "chemical potential," μ, of each constituent, a term invented by Gibbs and characteristic of his mode of procedure. Particular pains have been taken to examine critically the important equations, especially those where slovenly derivations have led to serious errors.

The book has many novel and excellent features. In the first place it is cheap—dirt cheap—at ten and sixpence. A "List of Most Important Symbols" is provided for ready and convenient reference. "Notes and References" appear at the end of every chapter in which original papers are quoted, and contrasts drawn between the treatment herein and that in other standard texts. Apart from considerations of appearance, these extremely valuable comments might with advantage have been placed at the beginning of each section.

The first chapter summarises concisely the inter-relationships between fundamental quantities, E, S, F, G, etc., according to the 1st and 2nd Laws, and reasons are given for preferring certain groups of these variables to others as threads from which the whole fabric is to be woven. The general formulæ of Thermodynamics are then applied to "Systems of One Component," "Gaseous Mixtures," and "Condensed Phases of Several Components." Solutions are discussed under three heads, Ideal, Semi-Ideal, and Non-Ideal, a classification suggested by the observed properties of salt solutions. Semi-ideal solutions (which mix with very slight heat effect or volume change, and yet are far from being ideal) are represented by "most aqueous solutions of strong electrolytes below tenth molar." The activity coefficients of solvent and solute are functions of composition only, independent of temperature and pressure, and for this reason the thermodynamic relationships are simpler than for non-ideal solutions. Bjerrum's term, the "osmotic coefficient" of the solvent, is introduced and its importance clearly explained. The exact thermodynamic expressions for membrane equilibria are made the basis of a very pretty and original discussion of electromotive force. Galvanic cells are treated as systems of connected membranes, the liquid-metal and metal-metal interfaces being "permeable" to one only of the components, ions or electrons as the case may be. The familiar Nernst equation is shown to arise as a consequence of the functional form of the chemical potentials. Other topics include the Gravitational Field, Insoluble Films spread upon water, and Radiation. The concluding chapter, "So-called Third Law and Chemical Constants," seems out of place in this volume since the subject properly belongs to statistical mechanics and quantum theory; moreover, it is merely a catalogue of formulæ without adequate explanation. It does, however, serve the useful purpose of bringing to the notice of chemists generally the fascinating researches of Giauque and his collaborators at the famous thermodynamical laboratory of the University of California.

The reviewer desires most heartily to recommend this fine book to those interested in works of art. It is a really important contribution to scientific literature and should encourage others to follow in the way of Gibbs.

C. H. J.

Electricity. By John Pilley. The Clarendon Science Series. [Pp. xiv + 348, with 181 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1933. 7*s.* 6*d.* net.)

This book does not follow the normal textbook lines in its presentation of the facts and theories of electricity and magnetism, and, frankly, it is not likely to recommend itself in this respect to those who desire the accustomed treatment. For instance, the average teacher may wisely preface his course on electricity with an introductory talk on the electrical structure of matter, but it is unlikely that he will postpone his remarks on magnetic phenomena until he has almost reached the end of the course. However, few teachers will have difficulty in recommending this cleverly illustrated and well-printed book to students who wish to read a pleasing account of electrical experiments and practical applications of electricity in which full advantage of modern instruments and technique is taken. The book may also suggest to teachers how much modern knowledge of electricity may fittingly be included in an elementary course.

L. F. B.

Symmetrical Components. As applied to the analysis of unbalanced electrical circuits. By C. F. Wagner and R. D. Evans. [Pp. xvi + 438, with 218 figures.] (New York and London: McGraw-Hill Book Company, Inc., 1933. 30*s.* net.)

Until the introduction in 1918 of the method of symmetrical components by Dr. C. L. Fortescue, the analysis of unbalanced alternating current electrical circuits presented difficulties which could only be overcome in a comparatively few cases. Since that date, the method has been developed with such outstanding success that all A.C. network problems involving asymmetry are soluble. Not least among those responsible for the application of this most powerful analytical tool to engineering work are Messrs. C. F. Wagner and R. D. Evans, the joint authors of this book.

The method of which the fundamental principles are explained in the first two chapters, depends upon the fact that the simultaneous presence of three sets of balanced voltages or currents, as represented by "positive-sequence" vectors, "negative-sequence" vectors, and "zero-sequence" vectors, results in a set of unbalanced voltages or currents respectively.

Any unbalanced voltage or current system can thus be built up from the three fundamental sequences, each of which is in itself symmetrical. The currents and voltages of different sequences do not react upon each other so that each of the three symmetrical systems can be treated separately, just as a balanced polyphase system can be analysed by considering the various quantities with regard to each phase. It is the fortunate fact that the three sequences are independent that enables the method to be applied with such substantial success and simplicity. The authors have taken numerous examples throughout the pages of this volume and shown how beautifully the method works. They consider in turn the calculation of unbalanced faults, the constants of synchronous machines with special reference to short-circuit conditions, the constants of transformers, the constants of transmission lines, taking first the case of lumped inductance and negligible capacity and afterwards the more exact representation employing distributed inductance and capacity, the constants of cables, power system voltages and currents under fault conditions, simultaneous faults at two or more points in a transmission system, the measurement of sequence voltages and currents, the measurement of power quantities on polyphase circuits, multiphase systems, induction motors and finally unsymmetrical systems with the application of phase balancers. A very good bibliography is appended followed by a most useful summary of various mathematical relations which have been employed together with transmission and apparatus constants frequently required.

As far as the writer is aware, the book is the only one of its kind, that is, a treatise specially concerned with the theory and applications of the method of symmetrical components. It fills a definite gap in the publications on A.C. circuit analysis and should have a wide appeal to electrical engineers generally.

H. M. Barlow.

The Theory of Atomic Collisions. By N. F. Mott, M.A., and H. S. W. Massey, Ph.D. The International Series of Monographs on Physics. [Pp. xvi + 283, with 52 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1933. 17*s.* 6*d.* net.)

The main object of this work is to give an account of the modern theory of collisions between electrons, nuclei, atoms and molecules, but it contains also much that is of general importance in Quantum Mechanics. The opening chapter, for example, contains a short introduction to some general features of the wave equation and in the fourth chapter the theory of Dirac's equation is introduced. The book keeps, in general, very closely to its subject and is an application of the wave equation to a variety of practical problems. It is essentially intended for those who have already some knowledge of the theoretical side of the subject. Those who tend to be absorbed in the successes and difficulties of its more formal aspect will find in the problems treated the intellectual satisfaction of their solution and a guide to further progress in the practical side of the difficulties which arise. For those whose interest is in the practical aspect of collision problems the work may be described and recommended as a modern book of reference in this subject.

One must sympathise with the authors in that in the present state of the subject with which they are concerned, it is difficult to present it in an attractive sequence. They have given it unity by means of the wave equation and the probability function which satisfies it and they have made the presentation concise by dividing the book into small chapters and the chapters into short paragraphs. The reader has a sense of discontinuity in going through the book, but, on the other hand, it is all very much to the point.

There is some mention of experimental work, but the subject is treated in its theoretical aspect throughout, and it might be suggested that a very useful addition in the future, when more experimental material becomes available, will be a careful comparison of the theoretical predictions with the actual findings.

The authors prefer to speak very definitely of " the wave " in connection with material particles. Possibly it would be better to use this term with more reserve. In connection with Dirac's later theory they speak of " the complete relativistic theory " of the motion of a particle, thereby revealing themselves as optimists with regard to the present state of quantum mechanics. But it would appear that this most important advance of Dirac's is neither complete nor relativistic This is, however, not directly concerned with the object of the book, and the authors have made a very valuable and definite contribution to an important series of monographs at a time when it is very urgently needed.

H. T. F.

CHEMISTRY

A Text-book of Inorganic Chemistry. By Dr. FRITZ EPHRAIM. Second English edition revised and enlarged by P. C. L. THORNE, M.A., M.Sc., Ph.D. [Pp. xii + 873, with 85 figures] (London : Gurney & Jackson, 1934. 28*s*. net.)

THIS book is so well known and so much appreciated as an advanced text-book of inorganic chemistry as distinct from a work on physical chemistry masquerading under a false title that a revised edition is welcome. Since 1926 it has grown by 68 pages and much recent work has been incorporated; the subject index has also been considerably improved, being some 50 per cent longer.

The electron theory of valency appears for the first time, but the use made of the theory is somewhat spasmodic; for example, the structure of the boron hydrides is discussed from its standpoint, but in regard to sulphur hexafluoride it is merely stated that " there is obviously a very complete internal saturation of its chemical affinity which makes it behave almost like nitrogen." The present edition is, in fact, rather hesitant between the classical and the modern style. However, the chemical facts are provided in an attractive setting, giving a very readable book which will be as much in demand as ever by advanced students of the subject.

O. L. B.

A Text-book of Inorganic Chemistry. By J. R. PARTINGTON, M.B.E., D.Sc. Fourth edition. [Pp. viii + 1062, with 390 figures.] (London : Macmillan & Co., Ltd., 1933. 15*s*. net.)

ALTHOUGH only three years have elapsed since a third and completely revised edition of Prof. Partington's well-known text-book appeared, the rapid advances made in some branches of the subject have necessitated further revision and the fourth edition is now to hand. The principal changes are in the chapter on atomic structure, some obsolete matter having been deleted and a modern interpretation by Werner's theory included. Electronic formulæ are more fully used throughout the text and new matter, *e.g.* the newly discovered isotopes of hydrogen and some other elements, have been incorporated. As in earlier editions, special emphasis is laid on modern industrial processes, both in the case of the metals and non-metals. Tables of atomic weights and isotopes have been omitted from the new edition on the grounds that they are frequently revised and therefore rapidly tend to become obsolete. A few mis-prints have been noted: MnO_4, 23 gm., and Chap. XXXV (pp. 237, 312, and 352) should read Mn_2O_7, 2 3 gm., and Chap. XXV; a proper name has been misspelt on p. 459, the electronic formula of N_2O_5 (p. 584 B) contains a redundant oxygen atom, and the maximum covalency of nitrogen is given as three (p. 476) and four (p. 584 B). To recommend "Partington" is superfluous. The second edition established it as the one indispensable text-book for all chemistry students and subsequent editions have consolidated its position. The parts of more direct interest to advanced students are concisely written, but full explanatory details are given in all the elementary sections so that students in all stages find the book equally useful.

J. N. S.

Outlines of Organic Chemistry. By F. J. MOORE, Ph.D. Revised by WILLIAM T. HALL. Fourth edition. [Pp. xiv + 338, with frontispiece and 15 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1933. 16*s*. 6*d*. net.)

THIS book claims to be designed especially for the general student, and as such is excellent. Unlike so many elementary books it is obviously written by a scholar who has taken great pains to avoid inaccuracy of statement where modern views are beyond the capacity of the student for whom the book is meant. The treatment of stereochemistry is happy yet simple and the schemes of "genetic relationships" between certain groups of compounds will prove very helpful to the beginner. The publishers claim that the binding is waterproof and vermin proof, the first claim is justified but the writer has had no opportunity of testing the latter. One could recommend this book heartily to those starting the study of organic chemistry except for one thing, the price, which will, unfortunately, militate against its adoption in this country.

O. L. B.

Organic and Biochemistry. By R. H. A. PLIMMER, D.Sc. Fifth edition. [Pp. x + 624, with coloured plate and 69 figures.] (London: Longmans, Green & Co., 1933. 21*s*. net.)

WHAT was originally a book on practical physiological chemistry for medical students has in the course of successive editions undergone extension and alteration and now reappears with a new title. The author in his preface claims that it may serve both as a text-book and practical book on organic chemistry. Perhaps what he means by this can be best understood from the last paragraph of his preface "that the general treatment has been to consider organic chemistry as the basis of physiological chemistry or rather the two as one subject, bio-chemistry." The natural result is that the first 232 pages consist of an outline of descriptive organic chemistry, with special reference to compounds of biological interest with the elimination of practically all the fundamental ideas on which the science of organic chemistry is based. A number of simple preparations are described, some from biological sources, and qualitative tests are given for important classes of compounds. The rest of the volume is devoted to compounds of biological importance and considerable complexity, for example the carbohydrates, purines, alkaloids, plant and animal pigments followed by a number of chapters on fermentation, digestion, the blood, metabolism, etc., with the description of a number of suitable qualitative and quantitative experiments. The attempt to provide a course of organic chemistry in addition to physiological chemistry is a mistake, as, in order to keep the book within reasonable limits, it has been necessary to compress the latter to such an extent that it seems unlikely that the structural formulæ given for cholesterol, morphine, hæmin and so on will convey much to the student whose knowledge of organic chemistry is based on the earlier part of the book. Biochemistry has become so complicated that one wonders how much longer it can remain a part of the ordinary medical curriculum and it would seem that even now it were better left to those whose interests and adequate knowledge of organic chemistry lead them to specialise in this direction.

When the book comes to its proper business, the chemistry of compounds of physiological importance and the practical methods associated with physiological chemistry, it retains the virtues of its classic forbear.

O. L. B.

Fundamentals of Biochemistry in Relation to Human Physiology. By T. R. Parsons. Fourth edition. [Pp. x + 435.] (Cambridge: W. Heffer & Sons, 1933. 18*s*. net.)

In this new edition of a well-known text-book the subject matter has been brought up to date. The author is to be congratulated on the very readable and very useful volume which has resulted. The book is written so as to be easily understood by those who have no previous knowledge of the subject and so the fundamental ideas are discussed very simply and fully, whilst complicated masses of detailed facts have been avoided. Nevertheless, the author has contrived to cover a wide field and to convey a large amount of information of the less detailed but more important type. Two chapters deal in an elementary but not inadequate manner with the physico-chemical principles upon which so much of the theory and practice of biochemistry depends and possibly it is characteristic of the book that these have been put at the end and not at the beginning. For those who are interested in biochemistry mainly from the medical point of view the book can be thoroughly recommended, though some will regret that the author gives his blessing to the use of the word "tension" to denote the partial pressure of a gas.

W. O. K.

Fluorescence Analysis in Ultra-Violet Light. By J. A. Radley, B.Sc., A.I.C., and Julius Grant, Ph.D., M.Sc., F.I.C. Monographs on Applied Chemistry, Vol. VII. [Pp. xii + 219, with 13 plates and 16 figures.] (London: Chapman & Hall, Ltd., 1933. 15*s*. net.)

The authors of this work had two aims in view when they started their task: (1) to provide for the general reader a brief treatise on the theory and technique of analysis by means of the fluorescence produced in ultra-violet light; (2) to survey the various applications of the technique to different branches of pure and applied science. The second portion of the work collects up an enormous mass of material which will doubtless be of value to specialists in the various branches considered; but it is apparent that the subject is—as yet—still in its infancy. Indeed, it would be surprising to find the subject in any other state in view of the status of our knowledge concerning the fluorescence of solids and liquids.

A number of faults, both of omission and commission, have been noticed. These all appear in the first section of the work—that devoted to the theory and technique of the subject. The treatment of the theory is brief—this is to be expected in view of the lack of theoretical background to the subject, but when one comes to examine the technique it is surprising to find (in a chapter headed "The Production of Ultra-Violet Light") no mention of an extraordinarily useful source which is now some years old, *i.e.* the hydrogen lamp. At times the treatment becomes very loose, thus the unqualified statement that "Lenses of quartz coated with silver transmit ultra-violet but not visible rays" (p. 26) could mislead anyone not familiar with the absorption curve of silver. A similar looseness occurs when the transmission of various glasses is discussed (p 23). Errors which should not have escaped vigilant examination of the proofs are, for example, "spectrograph" in place of "spectrogram" (p. 141), or again, the displacement of the carbon arc lamp spectrum by some 3 mm. to the violet (Fig. 9).

A. H.

The Principles of Metallurgy. By Donald M. Liddell and Gilbert E. Doan, Ch E, Ph.D. [Pp. viii + 626, with 324 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1933. 30*s*. net.)

Any attempt to compress a subject such as the principles of metallurgy into a book of 600 pages so that each branch shall receive adequate treatment would appear an impossible task. The authors of this book, however, have produced a very readable and useful volume. The book is intended to perform two functions, *viz.* to give the engineering student who does not propose to take up metallurgy as a life work a general view of the subject and to give the student who intends to become a metallurgist a proper foundation for his vocation.

After an introductory chapter in which the economic aspect of metallurgy is properly emphasised, they divide the book into three sections. The first deals with the materials consumed in metallurgy such as ores, slags and fluxes, refractories, fuels, and air and water supply. The second part is concerned with extractive processes and apparatus. In this section the preliminary operations of metallurgy such as ore dressing, leaching, roasting, etc., and subsequent operations, for instance smelting, are discussed. In the final section physical metallurgy, embracing such matters as the metallic state, alloys, properties of materials as related to structure, etc., are treated in a somewhat detailed manner.

In general, each portion of the subject receives due consideration, a notable exception being that of "concentration by flotation" which occupies one and a half pages only, whilst "selective flotation" is dismissed in a few lines.

A defect of the book is the unrestricted use of words which have acquired a very definite meaning. An example of this is the employment of the word "metallics" as a general term for the metallic state. There are also a few misleading statements which have escaped the proof reader: these, no doubt, will be corrected in any future edition.

These blemishes, however, do not seriously lower the value of the book, which can be recommended as an introduction to the study of metallurgy.

E. W. Yeoman.

Industrial Heat Transfer. By A. SCHACK, Dr.-Ing. Translated from the German by H. GOLDSCHMIDT, Dr.-Ing., and E. P. PARTRIDGE, Ph.D. [Pp. xxiii + 371, with 40 figures.] (London: Chapman & Hall, Ltd., 1933. 31*s*. net.)

DR. SCHACK wrote his book on heat transfer in 1929, after he had had seven years' experience in the Wärmestelle des Vereins deutscher Eisenhüttenleute (the Heat Economy Research Department of the German Iron and Steel Institute). He had previously published a paper on "The Radiation of Furnace Gases and its Practical Calculation," in which, from physical measurements of the infra-red absorption bands of carbon dioxide and water vapour, he had shown how flame radiation could be calculated. This work is of supreme importance in the theory of furnace design, and his theoretical conclusions have since been verified experimentally. It is natural therefore that one looks more to the chapters on radiation in this book, and one is not disappointed, since the subject is treated in a more comprehensible manner than any other general book on heat transfer.

The author is not afraid of mathematics but has had sufficient practical background to realise its limitations. A detailed derivation is given for the problems relating to heat transfer by convection from the original hydrodynamical equations. The final formulæ, however, have been simplified to the point where they can be used with much greater facility than would otherwise be possible by the practical designer. The fact that he can, if necessary, check the assumptions leading to these simpler formulæ will enable him to use them rationally and with more confidence than others which have been proposed without such a theoretical backing.

Heat transfer by conduction through composite walls could be dealt with more concisely with advantage. The section of this chapter dealing with the case in which heat conduction varies with both time and distance, and the use of Fourier's series in its solution, is well done.

The book finishes with an analysis of heat exchanger calculations and economics and includes worked-out examples. It has been well translated, and is a book which will be indispensable to all those concerned with the design of furnaces, regenerators, etc., operating at high temperatures.

M. B. DONALD.

Chemical Calculations. Their Theory and Practice. By A. KING, M.Sc., A.R.C.S., D.I.C., and J. S. ANDERSON, Ph.D., A.R.C.S., D.I.C. [Pp. x + 181.] (London: Thomas Murby & Co.; New York: D. van Nostrand Co., Inc., 1933. 4*s*. 6*d*. net.)

NO part of the elementary education of the chemistry student is more important than adequate instruction in chemical calculations. It is unfortunately true that this rarely obtains the attention which it deserves and still more rarely do the methods employed show a real understanding of the principles involved. The correct answer to a problem is of little value, if to obtain it the student displays his mathematical rather than his chemical ability. Here perhaps for the first time we have a text-book written with a full understanding of the fundamentals of the subject. It is in fact more than a text-book of chemical arithmetic: it is a clear and concise exposition of the foundations of chemical theory. The chapters on Atomic Weights and especially on Volumetric Analysis are recommended not only to students but also to teachers, who too seldom employ the direct and logical methods described in this book. There are over 500 problems, half with answers and half without, the logarithm tables are conveniently placed for easy reference, and the logarithms of common chemical constants are specially emphasised in the type. A few misprints have been noticed, the most obvious being the date of Cavendish's work on page 38.

J. N. S.

Qualitative Chemical Analysis. By ROY K. MCALPINE, Ph.D., and BYRON A. SOULE, Sc.D. [Pp. xii + 697, with 3 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1933. 21*s*. net.)

THIS book is based on an earlier text on Qualitative Analysis by Prescott and Johnson, first published in 1901. Great changes, both theoretical and practical, have occurred since the book first appeared, and from a modern point of view the older work was lacking in theoretical treatment, the description of analytical procedure was rather scanty and the bibliography referred principally to the older literature. These shortcomings have now been remedied.

Part I of the new edition contains a section on the Theory of Analysis, including solutions to many numerical examples illustrating the theoretical treatment, and also a rather unusual chapter on the solubility problem which contains material not usually available in ordinary text-books. Parts II and III are sections on the reactions of the basic and acidic radicals respectively. Part II includes practically all the rarer metals and a very full and up-to-date bibliography of their history and reactions is given. This is the most valuable part of the book. Non-metals and acidic radicals are dealt with in a similar way in Part III. Part IV contains an account of the systematic examination of mixtures, and also an appendix of physical data, etc. English readers will doubtless find the group separations rather cryptic in form and some concessions to English practice would enhance the usefulness of the book in this country.

The authors have succeeded in compiling a very valuable work which should find a place on the reference shelf of all analytical laboratories. It is well produced and moderately priced, but the publisher's introduction is quite unworthy of a serious scientific text-book.

J. N. S.

Elementary Qualitative Analysis. By F. M. BREWER, B.Sc., M.A. (Oxon), Ph.D. (Cornell). [Pp. viii + 228.] (Oxford: The Clarendon Press; London: Humphrey Milford, 1933. 6*s*. net.)

MOST teachers engaged in the teaching of elementary qualitative analysis eventually write a small book on the subject. The difference between these books is usually very slight and more often than not reflects the personality of the publisher rather than that of the author. In this case, however, the author has succeeded in producing a text-book which reflects a new and original point of view and which may be confidently recommended to all who wish to acquire a real understanding of analytical procedure. The maximum use is made of the relationships (usually ignored) between the analytical groups and the Periodic Table, and the connection between the chemical properties which determine the group into which a metal falls and the way in which the metal occurs in nature is brought out. This treatment of the subject brings chemical analysis into much closer relationship to other branches of inorganic chemistry than is usually the case. Physico-chemical explanations are given where necessary, but formulæ and equations are very sparingly used and complicated tables of procedure are completely absent, consequently the text avoids that appearance of complexity too often found in books of this kind and which many beginners find so confusing. The book is well produced, pleasing to the eye and should prove very popular with both teachers and students.

J. N. S.

Volumetric Analysis. By H. P. Starck, M.A. (Cantab.). [Pp. viii + 228, with 11 figures.] (London: Baillière, Tindall & Cox, 1934. 7*s.* 6*d.* net.)

Volumetric analysis forms a very important part of the curriculum of every chemistry student, but it is unfortunately true that a large majority of students, especially those who take chemistry as a subsidiary subject, have considerable difficulty in grasping the fundamental simplicity of the principles involved. This is undoubtedly due to the way in which the subject is first presented to them. If the equivalent weight be clearly defined, all chemical equations reduced to their simplest terms, and if, from the beginning, students learn to think in equivalent weights and litres, and never in grams and cubic centimetres, then difficulties vanish and the subject is seen as a whole. The value of the book under review would have been enhanced if these points had been emphasised. Its chief merit lies in the wide range of exercises it contains, many of interest to pharmaceutical students, not being usually found in the smaller text-books. An introduction contains two short chapters on the use and theory of indicators and an account of B.D.H. universal indicator is given in the appendix. About 100 problems (with answers) are included and also a detachable booklet of tables of qualitative analysis. This latter may be purchased separately, price 6*d.*

J. N. S.

GEOLOGY

Thin-Section Mineralogy. By A. F. Rogers, Ph.D., and P. F. Kerr, Ph.D. [Pp. xiii + 311, with coloured plate and 261 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1933. 18*s.* net.)

This book covers much the same ground as H. G. Smith's reviewed on p. 167, but it is conceived and written on a much larger scale. It is divided into two parts: I, Mineral Optics; II, Descriptions of Individual Minerals. To quote from the preface: "The treatment is intended to develop optical mineralogy for the beginning student, whether he happens to be interested primarily in the field of mineralogy, petrography, geology, chemistry, physics, or engineering. The principles outlined apply considerably beyond the field of mineralogy, although the application is restricted to minerals." We must say that while the treatment of mineral optics is admirable throughout, the American "beginning student" must be an accomplished fellow to assimilate it all in an introductory course, in which it is stated to occur. In this country the material assembled in the first part of this book is usually given to second- and third-year students. A special feature of the book is the excellence of the diagrams which, throughout, are fresh, clear, informative, and exceedingly helpful to the student. There is also a valuable chapter outlining a procedure for the identification of minerals in thin section. As stated above, this text is likely to be of most value to second-year and advanced students of mineralogy and petrology. We think, however, it is above the heads of students newly introduced to the subject.

G. W. T.

The Determination of the Feldspars in Thin Section. By Dr. K. Chudoba. Translated by W. Q. Kennedy, D Sc. [Pp. 74, with 50 figures.] (London: Thomas Murby & Co.; New York: D. van Nostrand Co., Inc., 1933. Cloth 6*s*. 6*d*. net, paper 4*s*. 6*d*. net.)

From the point of view of classification the determination of the feldspars is the most important routine operation in petrographical work. Hence Dr. K. Chudoba's concise summary of the various methods which are available for the determination of feldspars in thin section is most valuable, and Dr. Kennedy's excellent translation from the German now makes the work available for English-reading students. The book is especially valuable in that the Fedorow technique is explained and used according to the zonal method invented by A. Rittmann, but many other methods are carefully described and illustrated. The treatment of each method is subdivided into "Principles of the Method," "Recognition of Suitable Sections," "Determination," and "Scope of the Method and Control of the Determination"—a thoroughly practical and scientific system which is supplemented by numerous clearly drawn diagrams and graphs. We are glad to see that Dr. Kennedy uses the term "feldspar" in his translation instead of the grossly incorrect "felspar" which has held the field for so long in English literature. Petrologists and mineralogists will be deeply grateful to Dr. Kennedy for the addition of a valuable weapon to their armoury.

G. W. T.

The Dinosaurs. A Short History of a Great Group of Extinct Reptiles. By W. E. Swinton, Ph.D., F.R.S.E. [Pp. xii + 233, with 25 plates and 20 figures.] (London: Thomas Murby & Co., 1934. 15*s*. net.)

The Dinosaurs, almost from the time of their first discovery and description in 1824, have attracted the attention of the general public, at first because of their usual great size, more recently when the whole structure of some of them became known by their grotesque appearance, and finally they attained notoriety when an American Museum expedition in Mongolia found nests of deinosaur eggs, each clutch neatly arranged in an inner circle of five and a much larger outer circle!

Thus there is clearly room for an up-to-date popular exposition of their characters and it is curious that only one such book has previously appeared, the late Dr. W. D. Matthew's admirable guide to the collection in the American Museum, which is difficult to procure except in New York. Dr. Swinton's book fills the gap admirably. It gives a short non-technical account of the general structure of deinosaurs, and then passes on to review the whole group, discussing each important genus in turn.

In general, Dr. Swinton tells us where each animal was found, where it lived and in what museums its more important remains are preserved; he then describes its salient anatomical features and discusses its probable habits and mode of life.

The whole is prefaced by an account of the geography of the "Deinosaur

Period " and closes with an account of Deinosaur diseases and a discussion of the possible causes of the extinction of the group.

One Appendix is a glossary of technical terms and another is a most useful alphabetical list of the described species of British Deinosaurs, an enormous series of about 120 names applied to animals known in the main from miserable fragments. But we have in England some few splendid specimens, the skeleton of Scelidosaurus from Lyme Regis for example, which are much in need of a renewed examination and critical discussion; and indeed the whole of our series would yield interesting new facts to anyone who is really familiar with the much more abundant and complete American materials.

Dr. Swinton's book is pleasantly written and none of its errors seem to be of importance, but the Connecticut River is far removed from Boonton, New Jersey, despite the statement on page 59.

D. M. S. W.

BOTANY

Pflanzen-Thermodynamik. By KURT STERN. [Pp. xi + 412, with 20 figures.] (Berlin: Julius Springer, 1933. R.M.32 paper covers, R.M. 33.20 bound.)

THIS book which forms Vol. 30 of the *Monographien aus dem Gesamtgebiet der Physiologie der Pflanzen und der Tiere* is divided into two parts devoted respectively to the " Physical Basis of Plant Thermodynamics " and to " The Applications of Thermodynamics to Processes occurring in the Plant." As explained in the preface the first part is not intended to compete with the many introductions to Thermodynamics written by physicists and chemists, with the object of teaching the fundamentals of the subject; the present author has been guided in his choice of the aspects of thermodynamics which he describes not by their importance to thermodynamics but by their significance or application to biology and, as a consequence, quite important aspects of the subject have been entirely omitted. The guiding principle in writing this first part has been to provide biologists with the necessary equipment to enable them to appreciate the applications described in the second part. The author admits that the first part will not make light reading and recommends those who do not understand him on a first reading to try again. The subject matter of Part 1 is divided into seven chapters which deal successively with the first law of thermodynamics, the second law, the phase rule, and chemical, electrical, light and surface energy. Part 2 is likewise divided into seven chapters, entitled the applications of the first and second laws, thermodynamics of phase changes of chemical processes and of electric phenomena in the plant, the thermodynamics of carbon assimilation and of surface phenomena in plants, and the general thermodynamics of physiological efficiency of the plant. Among the many aspects of plant physiology subjected to critical thermodynamic study may be mentioned the work of Osterhout on Permeability, the subjects of Suction and Turgor Pressure and the Cohesion Theory of Renner and Dixon; the longest chapter in the book is, however, devoted to a selection of chemical processes classified into two groups according as they involve an increase or a decrease of thermodynamic potential. The author has been at great pains to render a difficult subject as intelligible as possible to biologically trained readers in an attempt to bridge the gap between them and the more mathematically minded exponents of thermodynamics; the success which he has achieved will depend largely on the intellectual equipment of his readers and the fault will not be his if he falls short of his ideal.

P. H.

Virus Diseases of Plants. By JOHN GRAINGER, Ph.D., B.Sc. [Pp viii + 104, with 6 plates and 5 figures.] (London: Oxford University Press, 1934. 6*s*. net.)

THIS book contains in 104 small pages of large type six chapters on the relations between viruses and their hosts and their insect vectors, the properties of viruses, the classification of virus diseases, their economic effects and the possible measures of control. A seventh chapter is added on general experimental methods, including inoculation, grafting, sterilisation, the technique of greenhouse management and the like. Nearly a quarter of the book is given to a bibliography of 445 references. The author's aim, as given in the preface, is to "introduce the student to the phenomena associated with Virus Diseases rather than to provide him with descriptions of all known viruses; it is typical rather than complete." It suggests the notes taken by a student from a series of lectures and demonstrations in the laboratory and glasshouse rather than a consecutive account of its subject matter. The treatment is inevitably sketchy, since so much ground is covered in so small a space, and amounts to little more than an enumeration of the points which would require consideration in a study of the subject, together with directions to the relevant items in the bibliography.

J. H. S.

A Manual of the Timbers of the World. Their Characteristics and Uses. By ALEXANDER L. HOWARD. Second edition. [Pp. xxiv + 672, with 91 illustrations.] (London: Macmillan & Co., Ltd., 1934. 36*s*. net.)

THE new edition of this book is confined to a descriptive catalogue of timbers. Many new woods have been dealt with, and the fact that the catalogue now occupies nearly double the space it did in the first edition is a striking illustration of the large number of new woods which have come to hand within the last fifteen years. With a lifelong experience of timbers, the author is able to write with authority on the applied side of the subject, and it is to this that the book owes its value.

The arrangement of the woods is, as far as possible, under their principal trade names; this involves some separation, as for example, *Acacia melanoxylon* (Australian Blackwood) from other *Acacias*, while species of the genus *Eucalyptus* are scattered throughout the book. The carefully prepared indexes, however, enable any wood to be traced with ease, but it may be questioned whether any useful purpose is served in having an index of vernacular names separate from the general index.

The book contains several errors in the scientific nomenclature: mention may be made of *Africana Bingeria* for *Bingeria africana*, *Lovca* for *Lovoa*, *Oldfieldiana* for *Oldfieldia*, and *Tenuifolium* for *Pittosporum tenuifolium*.

The descriptions of wood structure are inadequate for the identification of unknown timbers, and it would have been preferable if the space occupied by these descriptions had been used for a fuller treatment of those applied aspects of the subject with which the book deals in so admirable a manner.

Despite these criticisms the book is likely to remain one of the standard reference works for those who handle wood.

F. W. J.

ZOOLOGY

An Introduction to Biology. By L. M. Parsons, D.Sc., D.I.C. [Pp. viii + 178, with 87 figures.] (London: Macmillan & Co., Ltd., 1934. 2*s*. 6*d*. net.)

A little volume for use in schools. To quote the Preface, it "attempts to give a broad general survey of biology" and "may appeal to the general reader who wishes to obtain information upon common biological phenomena brought before him from time to time, as much as to the student preparing for an examination."

Much of the text is neither accurate enough for the serious student, however elementary, nor lucid enough for the general reader, however young. Most of the book is taken up by brief descriptions of plant and animal types; at the end there is a short chapter on the human body, and another on "Man's Fight against Disease." There is no serious attempt to explain the Cell Theory, and no mention of the sexual organs or processes of any animal (except for passing references to the hermaphroditism of the earthworm and snail). In appendices, there are suggestions for outdoor work, and a list of over a hundred test questions. Suggestions for further reading would be valuable.

G. P. W.

Man's Place among the Anthropoids. Three Lectures on the Evolution of Man from the Lower Vertebrates. By William King Gregory, Ph.D., F.G.S., F.Z.S. [Pp. vi + 119, with 11 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. 6*s*. net.)

In this series of lectures, of which three were delivered in London and one in Oxford in May and June, 1933, Prof. Gregory reviews the present position of the problem of man's evolution from the lower vertebrates in the light of the evidence bearing on certain specific problems. These are the evolution of the locomotor skeleton, the evolution of the jaws and teeth and of the face and brain case. In substance the lectures are a reply to, and a counter criticism of, Prof. Wood Jones, whose recent book, *Man's Place among the Mammals*, has put forward the view that man is not descended from an ape-like form, the common ancestor of himself and the anthropoid apes, but is derived from a very early form of primate, which has long died out and through which he is in close relationship with the spectral tarsier. Such resemblances between man and the anthropoids as Prof. Wood Jones admits he attributes to "limited homœomorphy." His arguments are here examined by Prof. Gregory in detail in so far as they come under the three topics of his lectures. On this basis he restates the more generally accepted view of man's evolution in the light of the results of the most recent research. His careful and detailed review of the evidence of the comparative morphology of living and extinct forms should carry conviction to an open mind that the resemblances between man and the apes are too great to be consistent with anything but a close relationship. To hold any other view surely makes too great a demand on the theory of convergent evolution.

E. N. F.

The Behaviour of Animals. An Introduction to its Study. By E. S. Russell, O.B.E., D.Sc., F.L.S. [Pp. viii + 184, with 6 plates and 26 figures.] (London : Edward Arnold & Co., 1934. 10*s*. 6*d*. net.)

Amidst other introductions into the science of animal psychology Dr. Russell's lucidly written little book for different reasons takes a special place. Firstly, because of the theoretical standpoint of the author, who rejects all mechanical interpretation of animal behaviour, be it expressed in terms of tropisms of Loeb or of conditioned reflexes of Pavlov, and frankly defends the view that the behaviour of animals can only be conceived as a continued, persistent and varied effort, as a striving of the animal as a whole towards an end. The author calls this standpoint, that corresponds fairly well with the ideas set forth by MacDougall in his "hormic" psychology, the "organismal" or "holistic" point of view. In the second place, the book differs from most other books on this subject in so far as a special stress is laid on the importance of the simple observation of the acts of animals in free nature, the "good old-fashioned natural history observation," and much importance is attached to the knowledge gathered by field-biologists, shepherds and sportsmen. As to the first we certainly can fully agree with him. Animal psychology, raised to the rank of a critical science since its observations and experiments were brought from nature to the laboratory, has since then run the risk of getting too much estranged from nature, especially in its study of the lower animals, and certainly it would do it good if, more than in the past, observers, who are trained in the laboratory to critical observation and interpretation, would set themselves to work in the field. Whether we may trust this work to shepherds and sportsmen is a question on which one might be somewhat sceptical ; the results in the days of the old anecdotical school were generally not of a kind that make us wish for more. The mixing up of observation and interpretation, and the uncritical way in which observations were often carried out, make us rejoice that we have better sources of material for our study now. Also, it must not be forgotten that many problems, *e.g.* that of the intelligence of the higher animals, can only be studied with profit under the rigorous conditions of the laboratory. Yet "back to nature" may be a useful tendency to neutralise the one-sidedness of the laboratory work.

In his interpretation of animal behaviour the author lays stress on the biological or ecological meaning of its acts. The animal does not react to a single stimulus, but to a situation as a whole, and this situation is based on its ecological needs. Thus the so-called tropisms are regarded as efforts of the animal to get back to its normal ecological environment, of which the stimulus is only a sign. This interpretation certainly gives us a deeper insight into the real meaning of the actions of the animals than does the common physiological one. Yet this conception of reactions to a sign stimulus might give rise to the misunderstanding that we must believe that the animal understands the stimulus as a sign. It will be clear that it is only the human observer who recognises the sign in the stimulus, and that the animal only is compelled by its nature to react in a special way to a special perception, without any foresight of the ecological or biological results of its actions. Undoubtedly the author understands this quite well, but a few words of explanation would have prevented a possible misconception.

It is not possible to summarise in a short review the rich contents of the book, in which successively the actions of lower animals are treated (gathered up under the term of "maintenance activities"), the instinctive actions of the higher animals, and the learning process, in which the author rejects the physiological explanation of Thorndike and admits as essential of all learning the conative or goalseeking activity. The insight learning and the difficult question of animal perception in its relation to the modern Gestalt-theory are treated in the last two chapters. Certainly, everybody who is interested in the problems of animal behaviour and its interpretation will read Dr. Russell's book with much pleasure and profit, and we do not doubt but those, who are not yet acquainted with the ends and ways of animal psychology, will not easily find a better and clearer introduction.

J. A. BIERENS DE HAAN.

The Locust Outbreak in Africa and Western Asia in 1932. By B. P. UVAROV, Economic Advisory Council—Committee on Locust Control. [Pp. 74, with 11 maps.] (H.M. Stationery Office, 1933. 3*s*. net.)

THIS second survey of the Locust problem deals with the years 1931–32 (the first survey, published under the same auspices, also appeared in 1933 and dealt with the period 1925–31). A large number of countries, including some not officially adhering to the Imperial Institute of Entomology scheme, regularly send in reports on the locust situation and these are summarised in the present publication. Gradually, the picture of the seasonal migrations is being filled in. Though not yet complete, very great progress has been made in this direction as a result of the present survey. Even now, in many cases, the source of Locust swarms arriving at particular localities can be indicated, and if only such predictions could be made rather earlier and with greater accuracy, very great economies would result in the application of control measures. As Mr. Uvarov says, these measures are at present directed to the destruction of the hoppers which are already in occupation of cultivated land. With better knowledge of migration tracks, the flying swarms could be attacked, with the advantage that the time available would be longer and the insects would never reach the crops at all. The survey is particularly valuable in indicating what aspects of Locust ecology are most urgently in need of study, *viz.* those governing the times and directions of migrations. Already the data can be used to predict whether an existing infestation has reached or passed its maximum, but for more detailed prediction further research is still required.

O. W. RICHARDS.

Insects as Material for Study. By G. D. HALE CARPENTER, D.M., Hope Professor of Zoology (Entomology) at Oxford. [Pp. 38, with 1 plate.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. 2*s*. 6*d*. net.)

THIS book contains two Inaugural Lectures delivered at the end of 1933 and makes very interesting reading. The first lecture is a general review of insects and disease, parasitic control of insects, instincts in fossorial wasps and Empid flies, courtship of insects and scent, sight, sound and touch in relation to this and insects and flowers; all very interesting but the parts do not fit together.

The second lecture is a very clear and excellent argument showing that Mimicry and Protective and Warning Coloration are capable of interpretation upon the theory of Natural Selection and, unlike most papers upon Mimicry, this essay is remarkably free from the plethora of scientific names which usually obscure the subject. This lecture is a great improvement on the first because it can be read through without any of the jars one receives at short intervals in the previous one, but the whole book is written in non-technical language and is therefore intelligible to non-scientific readers.

FRANK BALFOUR BROWNE.

MEDICINE

The Mammalian Red Cell and the Properties of Hæmolytic Systems. By ERIC PONDER. Protoplasma-Monographien, Vol. VI. [Pp. xii + 311, with 52 figures.] (Berlin: Gebruder Borntraeger, 1934. R.M. 22.50.)

THIS most interesting and stimulating book is a compilation from the author's numerous papers, published during the last twelve years, and concerned with the physical properties of the mammalian red blood corpuscles. The subject is very technical and the author has treated it in his usual controversial manner, so that "Hæmatological Discussions" might perhaps have been a more suitable title for the volume.

Much of the book, but especially those sections dealing with the permeability of the cell, and with the different hæmolytic systems, will no doubt appeal to and be valued by physiologists and biochemists; but it must be admitted that to a large number of readers who are not endowed with a mathematical habit of thought, argument so dependent on figures and formulæ will be unintelligible. From the very wide and expert nature of his discussions it is obviously impossible in a short review to consider adequately the values of the author's results and opinions.

All readers will certainly accept the call for the further elimination of error so prevalent in laboratory work, such as faulty calibration of instruments and the use of inaccurate or unreliable solutions, yet most of us are compelled by the limits of practical convenience to be content with relative and comparable values and must restrain our zeal for the unattainable "absolute." The choice of methods and the interpretation of results is largely one of personal opinion, of psychological attitude, and it is not desirable that scientific work should be controlled by the pontifical laws of a dictator.

P. J.

The Origin of Cancer. By J. P. LOCKHART-MUMMERY, M.A., M.B., B.C. (Cantab.), F.R.C.S. (Eng.). [Pp. x + 150, with 29 figures.] (London: J. & A. Churchill, 1934. 10*s.* 6*d.* net.)

IN the matter and manner of this book Mr. Lockhart-Mummery has admirably adapted his material for study by lay and professional readers. The matter is clearly expressed and will enable them to understand it without being confused by too many technical terms. As a rule a demonstration of this kind seldom completely satisfies either of these respective readers. Mr. Lockhart-Mummery has accomplished this difficult task with commendable success. The chief elements of knowledge already acquired and the theoretical problems that require elucidation are shortly summarised and criticised. The author makes a point of describing matters outside his subject, but picks out the relation of heredity to cancer and places it in the forefront of the battle. This, according to him, should be the decisive line of attack on the problem.

The author states that Miss Maud Slye has definitely proved that the hereditary influence is of great importance in the consideration of this disease. He also explains that her intense, sustained and special work shows that the hereditary influence is more operative in the selection of the organ, gland or tissue affected than in being the absolute causal agent that is inducing the genesis of cancer in those regions. On page 67 he says, "Even in those families of human beings or mice in which there is a well-marked family history, the condition does not develop until late in life, and the children of such families are born normal and grow to be normal adults. It is only later that the disease develops. Therefore the hereditary factor is not cancer, but only a susceptibility to develop it." And again on page 68 he states, "It may, I think, be concluded that the genetic factor is one for susceptibility to a change in a certain tissue, which results in tumour formation, if other factors are suitable."

A little later on, on the same page, he states in reference to the part played by the genetic factor in the cancer problem: "That there are several different factors involved would seem probable and as regards malignancy the genetic factor is apparently not primarily concerned."

These statements represent the present position of heredity in relation to cancer and definitely explain that it is only a part of the process even in those instances where its influence seems predominant. It is not known whether its presence as a background or as a more intimate cause, is essential in the genesis of all cancers.

The omission of reference to the work of Warbourg is noticeable in a comprehensive review of Cancer problems.

This book appeals to all who are interested in the subject, and perhaps in a future edition Mr. Lockhart-Mummery might consider it wise to add a short glossary explaining such terms as "genes," "mutation," "filterability." Encountering these terms for the first time, a lay reader might have difficulty in understanding exactly what is meant in the wealth of condensed exposition the author has collected for his benefit.

G. L. C.

The Cancer Problem and its Solution. By HASTINGS GILFORD, F.R.C.S. [Pp. iv + 60.] (London: H. K. Lewis & Co., Ltd., 1934. Cloth 2*s*. 6*d*. net. Paper 1*s*. 6*d*. net.)

THE writer of this little book tells us that he "begins by discarding all that vast mass of material which has been thrown up so laboriously by those who explore this subject by experimenting on lower animals in laboratories." He then proceeds to postulate a theory of the origin of cancer based upon laboratory experiments on artificial fertilisation carried out with the eggs of invertebrates and frogs. As various stimuli induce eggs to develop independently of male germ cells, so cancer "appears before us as a new form of life, a new generation of cells, a protozoid parasitic growth derived from degenerate embryonoid cells of the human body by their activation with fecundating stimuli" (carcinogenic agents).

The solution of the cancer problem appears to be the avoidance of "degenerating circumstances," which are "leading us gently and pleasantly down the easy slope which leads to the waters of Lethe." Amongst the "degenerating circumstances" are "some which undermine and weaken the stability of that great influential system of organs which nourish the human body and of that still more important system in the female which maintains and nourishes the race."

An entertaining attempt to solve with pen and ink the problem of the malignant cell, but unsound cytologically.

R. J. LUDFORD.

Clinical Studies on the Physiology of the Eye. By J. GRANDSON BYRNE, M.A., M.D. [Pp. x + 144, with 49 figures.] (London: H. K. Lewis & Co., Ltd., 1934. 10*s*. 6*d*. net.)

THIS is an account of the practical application of the author's experimental work on the innervation of the eye. Stimulation of the affective part of sensation produces a reaction of avoidance including reflex widening of the eyelids, pupil and lens, while excitation of the critical system causes narrowing of the palpebræ, pupil and lens and an approach reaction. The action of various drugs on the mechanism of those eye movements is discussed.

Paradoxical pupillary widening following somatic lesions is next described; but the author considers the pseudo-paradoxical phenomenon the more important clinical sign as it has no incubation period. The effect of visceral lesions on the inequality in size of the pupils is also shown and taken together with the areas of referred pain and hyperalgesia the pseudo-paradoxical pupil is a reliable localising sign. The Argyll-Robertson pupil is fully discussed, including the post-mortem findings in one case. In the treatment of disorders of accommodation objection is taken to the permanent wearing of glasses; exercise of the external ocular muscles together with a complete investigation into the physical, mental and endocrine condition of the patient should be carried out in every case.

This book contains much that is new. The frequency of inequality of the pupils in association with visceral and somatic lesions is not stated. Also the value of various endocrine preparations in affections of the eye has still to be proved. There are many illustrations and a full bibliography.

W. A. G.

The Brain and its Mechanism. By SIR CHARLES SHERRINGTON, O.M., G.B.E., F.R.S. [Pp. 35.] (Cambridge: at the University Press, 1933. 1*s*. 6*d*. net.)

THE illustrious author of the *Integrative Action of the Nervous System* has here, in a few pages of non-technical language, made plain the broad lines of the method of function of the brain as a physiological mechanism. Few will realise the amount of work, so much of it his own, on which these few and carefully chosen words of Sir Charles Sherrington are based. But in addition to setting forth the kernel of what is known concerning the physiology of the brain, the author presents in a most lucid manner the problem to which there is as yet no answer, *viz*. the relation between nerve-action and mental process.

G. R. DE B.

Vitamins in Health and Disease. By BARNETT SURE, Ph.D. [Pp. xiv + 206.] (London: Baillière, Tindall & Cox, 1933. 11*s*. 6*d*. net.)

IN this little book the story of the vitamins is told in non-technical language and fulfills the claim which the author makes in his introduction that—it is a suitable text-book for students in nutrition classes, may be recommended to patients suffering from dietary deficient diseases, and will assist the dietetician, the nurse and the housewife in planning the menu. The different vitamins are described by the effects which their absence or deficiency produce, the accounts being preceded by short histories of their discovery. On pages 150–61 are set out useful tables showing roughly by signs +, + +, + + +, and – the comparative amounts of the different vitamins which are found in the common foodstuffs. In a short summary the author says: "If we consume daily one to two pints of milk, at least one egg, one-third to one-half a glass of orange or tomato juice, a generous portion of vegetables fresh or canned, whole cereals for breakfast two or three times weekly, meat at noon or at the evening meal, introducing liver, kidney, heart, or sweetbread at least once weekly, we need have little fear of vitamin deficiencies."

P. J.

A New Physiological Psychology. By W. Burridge, D.M., M.A., Professor of Physiology, Lucknow University. [Pp. vii + 158, with 6 figures.] (London: Edward Arnold & Co., 1933. 7*s.* 6*d.* net.)

The system of psychology built up by the author of this book is not without interest, but his attempt to establish it by inductive reasoning is not justified by the soundness of his premises. The physiological "facts" from which he starts are themselves assumptions based on inadequate experimental evidence; the insistence that they are correct, and the belittling of more conventional views, would have come more gracefully from another pen. The argument is not clear, and the reader's reaction to subversive contentions is to question, not the established order of things, but the validity of what is presented to him. As an example of confused thinking, the following sentence may be quoted: "The facts therefore strikingly confirm the second hypothesis, which is, moreover, actually derived from them." Anyone tempted to take this work seriously would be well advised to ponder the real significance of the author's original experiments, and not to neglect those made by other physiologists actually on the nervous system.

W. H. N.

Food and Character. By Louis Berman, M.D. [Pp. xxviii + 384.] (London: Methuen & Co., Ltd., 1933. 8*s.* 6*d.* net.)

According to the author "character may be defined as that which endures in the personality in spite of the vicissitudes and adventures of a life-time." It is something which is "crystallised by experience yet remains independent of it."

Assuming that there is something immutable in the mental interior, the author engages upon a discussion of the factors which may affect it, concerning himself chiefly, as the title of the book indicates, with the influence of diet.

Some years ago in a book entitled *Glands Regulating Personality* he expounded the provocative theory that man was just what his endocrine system allowed him to be. Here, using the methods there adopted of giving certain facts and relating them as cause and effect, he suggests that man is very much what his diet allows him to be, since his diet will influence greatly the endocrine glands.

Whether the author's views are accepted or not, readers will be interested in the unusual and important facts about food here presented, and very possibly stimulated to vigorous reactions to the opinion expressed as to the far-reaching effects upon character of variations in diet.

Such headings as "Metabolism of Leadership," "Lime and the Neurotic Constitution," "Unbalanced Diets and Unbalanced Minds," give some indication of the kind of problems tackled, though they can give no idea of the almost convincing way in which these propositions are put forward. Is a "wonder child" a "water plus individual" or a drowsy apathetic one a "water minus individual"? Can a person insensitive to social feeling and slow to accept new customs be converted into a more socially conscious and conforming person merely by increasing the calcium in his diet? These are some of the questions asked and answered. Sufferers from gout may get some comfort from learning that gout and genius often go together, whilst those not so intelligent may be encouraged by the suggestion that brains may prove as susceptible to feeding as are the larvæ of bees, which, as is well known, develop into queens or workers according to their diet.

Here is a provocatively interesting book for all, more particularly, of course, for those interested in the fundamental problem of right feeding. It asserts that the fullest development of the individual will only come with his adequate and proper feeding. It emphasises the necessity of securing for the children the proper start in life given by scientific feeding, which would mean "incalculable returns in increased ability of production, splendour of life and a superb quality of happiness that is to-day almost inconceivable."

W. C. C.

Recent Advances in Sex and Reproductive Physiology. By J. M. Robson, M.D., B.Sc., F.R.S.E. [Pp. ix + 249, with 47 figures.] (London: J. & A. Churchill, 1934. 12*s*. 6*d*. net.)

Sex is one of the cardinal facts of biology, but it is only during the present century and more especially during the post-war years that its scientific investigation has been systematically pursued. This book summarises our present knowledge of the female sex cycle and of the various hormones which control it. Naturally, a prominent place is taken by œstrin, the one hormone of the group which has been isolated in a pure crystalline form. But the hormones of the *corpus luteum* and of the anterior lobe of the pituitary are also familiar figures which are constantly being encountered. The book should interest a large variety of readers, biochemists, physiologists, biologists, even psychologists should find matter for thought in its pages, whilst to the clinician the results which have been obtained are potentially, if not actually, of the highest value and importance. This clearly written summary of our present knowledge comes opportunely at a moment when the very large number of investigations, some good, some bad and others indifferent, have made it almost impossible for any but an expert on the subject to form a balanced picture of an important branch of knowledge.

W. O. K.

HISTORY OF SCIENCE

A Leechbook or Collection of Medical Recipes of the Fifteenth Century. Transcribed and edited by W. R. Dawson. [Pp. vi + 344.] (London: Macmillan & Co., Ltd., 1934. 20*s*. net.)

The manuscript, of which this work furnishes the text and a transcript, is the property of the Medical Society of London and dates from the reign of Henry VI probably having been written in 1443 or 1444. It is a compilation largely based on the *De Medicamentis* of Marcellus Empiricus, itself a compilation of the fifth century. The work is published for the Royal Society of Literature under the terms of Dr. Richard's Trust and comprises an introduction of seventeen pages, in which the editor discusses the characteristics of the manuscript; a reproduction of the text itself; and a transcript into modern spelling.

The editor comments on a striking feature, having regard to the date of the manuscript, namely the absence of magical charms, and this enlightened point of view is emphasised by the frankness with which the author adds to one recipe the statement that by God's grace a cure would be effected.

Nevertheless, it is sometimes surprising to find to what remedies there is added the phrase *Probatum est*. Such "proven remedies" include the powdered ashes of a March Hare for the stone, or the following for restoring the growth of hair: The head is shaved and washed with an infusion of Maidenhair, oak-rind and Horehound, then anointed with honey and a powder made of burnt rat's dung, goat's hoofs, chestnut rinds, nut shells of the Indies. "Use this nine days for it faileth not."

The doctrine of signs is naturally in evidence in some of the recipes though fewer perhaps than might have been expected. Such, for instance, is the use of Saxifrage seed and (Gromwell) Lithospermum seed for the stone. Several remedies bear witness to the belief in the hypothesis that like cures like. Thus an interesting version of the "hair of the dog that bit you" is a recipe for a dog bite which consists of a drink prepared of Cress and Penny Royal accompanied by the placing of a hair of the offending dog upon the wound. Another example is furnished by the use of an ointment of flies rubbed into the wound as a cure for the bite of a spider.

An appendix of fifteen pages contains prescriptions from a Tudor manuscript in the author's possession.

A perusal of these recipes gives one an admirable picture of the state of medical knowledge at this period and the author has carried out estimably the task of rendering this work available to a wider public.

E. J. S.

On a New Chemical Theory and Researches on Salicylic Acid. Papers by ARCHIBALD SCOTT COUPER (1858). [Pp. 45.] (Edinburgh: Oliver & Boyd, for the Alembic Club; London: Gurney & Jackson, 1933. 2*s*. 6*d*.)

No. 21 of the well-known Alembic Club Reprints contains translations of Couper's two contributions to the *Comptes Rendus* of 1858, namely, his note "Sur une Nouvelle Théorie Chimique" (*C.R.*, 1858, **46**, 1157) and his paper "Recherches sur L'Acide Salicylique" (*ibid.*, 1107). The English version of the latter (*Edin. New Phil. J.*, 1858, **8**, 213) is also reprinted, as it differs from the French in some respects. Couper evidently wrote the French version first, and changed from C = 6 to C = 12 before publishing the English text. The French version of his more famous memoir appears to have been written a little later than the English "On a New Chemical Theory" (*Phil. Mag.*, 1858, **16**, 104), and paragraphs in the former have been translated and inserted here in the reprint of the latter.

The appearance of this reprint is a welcome tribute to Couper's great genius, a genius frustrated by disappointment and ill-health. If Würtz, to whom Couper handed his paper for submission to the Academy, had been a member of that body or had not been dilatory in passing it to Dumas for presentation, we should credit Couper, rather than Kekulé, with the theory of the linking of carbon atoms in organic molecules. As it is, Kekulé's paper preceded Couper's by a mere matter of weeks; but the consequences of Würtz's delay had a tragic effect on Couper's sensitive nature and may well have been to some extent the cause of those mental troubles that led him to terminate his chemical researches.

This number is an important addition to a most valuable series.

D. McKie.

Darwin. By R. W. G. Hingston. Great Lives Series. [Pp. 144.] (London: Gerald Duckworth & Co., Ltd., 1934. 2*s*. net.)

This delightful biography of Darwin is certain to have a wide appeal. It gives a sympathetic account of the life of the great Naturalist whose work, perhaps above all others, is both an example to men of science and an encouragement to amateurs. Through determination, unswerving accuracy, and marvellous powers of generalisation, Darwin made one of the great intellectual advances of the age; yet the experiments by which he so constantly tested his deductions were of the simplest, and his apparatus such as any ordinary house can provide.

It is surprising that the author has made such an attractive story from a life which, apart from one great adventure, was singularly uneventful. He might easily have given undue prominence to the voyage of *The Beagle*, or else written rather a dull book: in fact he has done neither. His picture of a man rising to the greatest heights by mental qualities of an unspectacular kind, handicapped by ill-health and the gradual loss of all æsthetic feeling, is indeed one of uncommon interest.

No man's work has formed a more integral part of his life than did Darwin's, and it is inevitable that considerable space in this biography should be devoted to discussing it. In so far as the account deals with its contemporary aspect, it is excellent; but the comments on its present value are not unexceptionable. Certainly the author's criticism of sexual selection will be thought on the whole well founded, but his lukewarm support of natural selection itself seems less easy to justify. His statement that "it is, indeed, little further than where Darwin left it" (p. 89) makes curious reading to anyone familiar with the great and absolutely fundamental advances in the analysis of this process achieved by Fisher, Haldane, and others during the last few years.

The most significant change since Darwin died is in our knowledge of heredity: one of the fundamental principles upon which his theories depend. No attempt to assess its effect is here made. It may be that such was thought out of place in a biography, though some post-Darwinian work is in fact mentioned (p. 91). But in one instance at least the omission is serious. The author rightly stresses the criticism of Fleeming Jenkin, that new advantageous variations must be swamped by intercrossing with the normal form. It was the most damaging objection to the theory of selection ever raised: the one to which Darwin himself bowed. It is imperative in any book on this subject to point out that now heredity is known to be particulate, instead of blending as Darwin supposed it to be, this outstanding difficulty has ceased to exist. Thus it is no longer necessary to suppose that new useful characters must be inaugurated afresh in many individuals simultaneously. Using modern terminology, it may be said therefore that the direction of mutation does not control the course of evolution.

E. B. F.

MISCELLANEOUS

Smoke and the Atmosphere. Studies from a Factory Town. By J. R. Ashworth, D.Sc. [Pp. xii + 131, with 30 figures.] (Manchester: at the University Press; 1933. 7*s*. 6*d*. net.)

An abuse which is intimately connected with industrial and domestic life is more difficult to suppress or mitigate than one in which only private interests are involved.

It is for this reason that any attempt at drastic legislation to stop the emission of smoke has received little encouragement from those in authority. Yet the nuisance is very generally recognised. There is a Smoke Abatement Society which carries on an active propaganda, there is a Government Departmental Committee on atmospheric pollution under the direction of Dr. J. S. Owens, which records systematic observations of impurities in the air. Books, pamphlets and letters to the Press on the subject are constantly appearing.

But, when all has been said and done, the smoke cloud still hovers over our cities (not always industrial), slowly descending and disseminating its sooty contents. In writing of this ever-present nuisance, it is only fair to say that those connected with the fuel, gas, and electrical industries have in many instances succeeded in producing effective smokeless heating appliances. There is nevertheless a certain unintelligent section of the community who still adhere to coal because it gives a cheerful blaze, regardless of the dirt from which they and their neighbours suffer, for it is the domestic hearth which is the chief criminal. The volume before us on " Smoke and the Atmosphere " by Dr. Ashworth belongs to a rather different category from the ordinary book or pamphlet on the smoke nuisance. It is the record of a very careful estimate, extending over many years, of the solid matter in the atmosphere by the use of ingeniously contrived apparatus. The experiments were carried out at Rochdale, which for the author's purpose may be regarded as a typical industrial town; for it contains within the radius of a mile no less than 160 to 170 mill chimneys.

There are chapters on the solid matter carried down by rain and its distribution in town and suburbs; the amount of solid matter carried and deposited by wind; the amount of daylight obscured by atmospheric particles and many other statistics, possibly of more interest to the meteorologist than to the ordinary person. The author has some criticisms to offer of what is known as the Standard Gauge used by the Air Pollution Committee for collecting rain water and which takes no account of the dust raised by wind, some of which must add to the solid content of the vessel.

It seems to the reviewer a little misleading to calculate the results on a basis of tons to the square mile from the minute quantity of solid collected within a few square feet, a process which might be compared with the estimation of the quantity of valuable ore in a very extensive region from a few ounces found in one particular spot.

Dr. Ashworth has done a valuable piece of work in this summary of his records and in the many pamphlets and papers he has published on the subject of atmospheric impurities, if it only served to keep alive public interest in this important matter.

J. B. C.

Differenzierungserscheinungen in einigen afrikanischen Gruppen. Ein Beitrag zur Frage der primitiven Individualität. By Dr. Sjoerd Hofstra. [Pp. viii + 214.] (Amsterdam: Scheltema & Holkema, 1933.)

This is a clear and valuable little monograph on a subject which has caused considerable debate among anthropologists,—the position of the individual in primitive society. The subject is a big one and the author has wisely restricted himself in this volume to Africa, but it is to be hoped that at a later date he will continue the study over a wider field—the Pacific for instance should prove a specially fruitful source. We should like to express gratitude that the author has written in German since Dutch is not a language with which students are usually acquainted. The general plan is simple and straightforward, though an index would have been welcome. The first chapter is devoted to a general statement of the problem, and a brief résumé of the conflicting views of authorities. In the succeeding chapters the problem of the individual in the various categories of primitive life is developed and discussed. The selected divisions are first social life, secondly religion, magic and medicine, and finally what may be roughly translated as folklore, that is tales, songs, and proverbs, the primitive equivalent of literature. The book closes with a general summary and a bibliography. The work is especially notable for the careful and critical use which the author makes of his authorities.

L. H. D. B.

Pagan Survivals in Mohammedan Civilization. By Edward Westermarck, Ph.D., Hon. LL.D. [Pp. viii + 190, with 74 figures.] (London: Macmillan & Co., Ltd., 1933. 8*s.* 6*d.* net.)

Islam, like Christendom, affords numerous examples of beliefs and practices, popular and otherwise, which are not organically a part of the official creed and ritual. In these lectures Prof. Westermarck has analysed these elements and shows that they are either survivals from a pre-Mohammedan stage of religious belief, or have been incorporated in Mohammedanism traditionally, but are of an extraneous origin. Not unnaturally, in view of his intimate knowledge of Morocco, he has directed his attention particularly to Mohammedanism as it is found in Egypt and North Africa, rather than in Arabia and the East. His selection of this area is, however, justifiable on other grounds, for it provides a peculiarly favourable body of material for the study of survivals. In Morocco a population indigenously Mediterranean has been subjected to the influence of Egypt, Carthage, Rome, the Vandals and the Arabs, while there has been throughout a steady, if not a too easily defined, inflow from negro Africa. The reference to Sudanese influence in the author's study of the belief in *jinn* is suggestive, although the remarkable product which has resulted from the amalgamation of these various cultural streams is perhaps best illustrated in his analysis of Berber and Roman survivals in the New Year and analogous calendrical rites.

E. N. F.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Elementary Dynamics. For Students of Science and Engineering. By Robert Cochran Gray, M.A., D.Sc., Companion I.E.E., Lecturer on Applied Physics in the University of Glasgow. London: Macmillan & Co., Ltd., 1934. (Pp xii + 211, with 94 figures.) 5*s*.

Differential Equations. By H. B. Phillips, Ph.D., Professor of Mathematics, Massachusetts Institute of Technology. Third edition. New York: John Wiley & Sons, Inc ; London: Chapman & Hall, Ltd., 1934. (Pp. vi + 125, with 17 figures.) 10*s*. 6*d* net.

Higher Mathematics for Engineers and Physicists. By Ivan S. Sokolnikoff, Ph.D., Assistant Professor of Mathematics, University of Wisconsin, and Elizabeth S. Sokolnikoff, Ph.D., formerly Instructor in Mathematics, University of Wisconsin. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xiv + 482, with 121 figures.) 24*s*. net.

The New Background of Science. By Sir James Jeans, M.A., D.Sc., Sc.D., LL.D., F.R.S. Second edition. Cambridge: at the University Press, 1934. (Pp. viii + 312, with frontispiece and 9 figures.) 7*s*. 6*d*. net.

The Architecture of the Universe. By W. F. G. Swann, A.R.C.S., M.A., D.Sc., Director of the Bartol Research Foundation of the Franklin Institute. New York and London: Macmillan & Co., Ltd., 1934. (Pp. xii + 428, with 23 figures.) 16*s*. net.

Red-Shifts in the Spectra of Nebulæ. Being the Halley Lecture delivered on 8 May 1934. By Edwin Hubble. Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. (Pp. 17, with 4 figures.) 2*s*. net.

Grundriss der Spharischen und Praktischen Astronomie. By Dr. Friedrich Becker, Observator an der Universitäts-Sternwarte zu Bonn. With contributions by Dr. B. Sticker and Dr. O. Wachtl. Berlin and Bonn: Ferd. Dümmlers Verlag, 1934. (Pp. 167, with 59 figures.) RM.4.80, stiff paper covers; RM.6.50, bound.

Introduction to Modern Physics. By F. K. Richtmyer, Professor of Physics at Cornell University. Second edition. International Series in Physics. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xviii + 747, with 222 figures and 6 plates.) 30*s*. net.

Heat, Light and Sound. Book I. By E. E. Whitehead, M.Sc., Senior Physics Master, Leyton County High School for Boys. Modern School Physics. London: George G. Harrap & Co., Ltd., 1934. (Pp. 256, with frontispiece and 149 figures.) 2*s*. 6*d*.

Heat, Light and Sound. Book II. By E. E. Whitehead, M.Sc., Senior Physics Master, Leyton County High School for Boys. Modern School Physics. London: George G. Harrap & Co., Ltd., 1934. (Pp. 298, with frontispiece and 148 figures.) 2*s*. 9*d*.

Properties of Matter. By A. E. Goddard, M.Sc., F.C.S., Senior Physics Master, Alderman Newton's Boys' School, Leicester, and H. F. Boulind, M.A., B.Sc., Physics Master, Cambridge and County High School. Part I. London: Methuen & Co., Ltd., 1934. (Pp viii + 119, with 63 figures.) 4*s*.

Electromagnetism. By Hector Munro Macdonald, M A, F.R.S., Professor of Mathematics in the University of Aberdeen London: G. Bell & Sons, Ltd., 1934. (Pp. xvi + 178.) 12*s*. 6*d*. net.

The Practice of Absorption Spectrophotometry with Hilger Instruments. By F. Twyman, F.Inst.P., F R.S., and C. B. Allsopp, M.A., Ph.D. Second edition. London: Adam Hilger, Ltd., 1934. (Pp. 140, with 45 figures, including 17 plates.) 12*s*. 6*d*. net.

Modern Acoustics. By A. H. Davis, D.Sc., Principal Scientific Officer in the Physics Department, the National Physical Laboratory. London: G. Bell & Sons, Ltd., 1934. (Pp. xii + 345, with 8 plates and 104 figures.) 26*s*. net.

Relativity Thermodynamics and Cosmology. By Richard C. Tolman, Professor of Physical Chemistry and Mathematical Physics at the California Institute of Technology. The International Series of Monographs on Physics. Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. (Pp. xvi + 502, with 13 figures.) 30*s*. net.

Thermionic Emission. By Arnold L. Reimann, B.Sc., Ph.D., F.Inst.P., member of the Research Staff of the General Electric Co., Ltd., and of the M.O. Valve Co., Ltd., Wembley. London: Chapman & Hall, Ltd., 1934. (Pp. xii + 324, with 64 figures.) 21*s*. net.

The Diffraction of X-Rays and Electrons by Amorphous Solids, Liquids, and Gases. By J. T. Randall, M.Sc., member of the staff of the Research Laboratories of the General Electric Co., Ltd., Wembley. London: Chapman & Hall, Ltd., 1934. (Pp. xii + 290, with frontispiece and 197 figures, including 30 plates.) 21*s*. net.

Alternating Currents. By Leo T. Agger, B.E., Head of the Electrical Engineering Department at the Municipal College, Leigh. London: Macmillan & Co, Ltd., 1934. (Pp. viii + 204, with 146 figures.) 5*s*.

Thorpe's Dictionary of Applied Chemistry. Supplement: Vol. I. A-M. By Jocelyn Field Thorpe, C.B.E., D.Sc., F.R.S., F.I.C., Professor of Organic Chemistry and Director of Organic Laboratories, Imperial College of Science and Technology, and M. A. Whiteley, O.B.E., D.Sc., F.I.C., Assistant Professor of Organic Chemistry, Imperial College of Science and Technology. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. xxii + 680, with illustrations.) 60*s*. net.

German-English Chemical Terminology. An Introduction to Chemistry in English and German. By Alexander King, M.Sc., A.R.C.S., D.I.C., Chemistry Department, Imperial College of Science and Technology, London, and Dr. Hans Fromherz, Privatdozent für physikalische Chemie an der Universität München. London: Thomas Murby & Co.; Leipzig, Max Weg, 1934. (Pp. xvii + 324.) 12*s*. 6*d*. net.

The Kinetic Theory of Gases. Being a text- and reference book whose purpose is to combine the classical deductions with recent experimental advances in a convenient form for student and investigator. By Leonard B. Loeb, Professor of Physics in the University of California. Second edition. New York and London: McGraw-Hill Book Company, Inc., 1934. (Pp. xx + 687, with 85 figures.) 36*s*. net.

The Kinetic Theory of Gases. Some Modern Aspects. By Martin Knudsen, Professor of Physics in the University of Copenhagen. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1934. (Pp. viii + 64, with 20 figures.) 2*s*. 6*d*. net.

The Adsorption of Gases by Solids. By S. J. Gregg, B.Sc., Ph.D., Assistant Lecturer in Chemistry, University College, Hull. Methuen's Monographs on Chemical Subjects. London: Methuen & Co., Ltd., 1934. (Pp. viii + 120, with 15 figures.) 2*s*. 6*d*. net.

The Fundamentals of Chemical Thermodynamics. Part II: Thermodynamical Functions and Their Applications By J. A. V. Butler, D.Sc. (Birmingham), Lecturer in Chemistry in the University of Edinburgh. London: Macmillan & Co., Ltd , 1934. (Pp. x + 271, with 43 figures.) 8*s*. 6*d*.

The Electronic Structure and Properties of Matter. Being Vol. I of A Comprehensive Treatise of Atomic and Molecular Structure. By C. H. Douglas Clark, M.Sc., A.R.C.S., D.I.C., Assistant Lecturer in Inorganic Chemistry in the University of Leeds. London: Chapman & Hall, Ltd., 1934. (Pp. xxvi + 374, with 33 figures.) 21*s*. net.

The Chemistry of Petroleum Derivatives. By Carleton Ellis, Industrial Research Chemist. New York: The Chemical Catalog Co., Inc., 1934. (Pp. 1285, with 48 figures.) $18.00.

The Natural Organic Tannins. History: Chemistry: Distribution. By M. Nierenstein, D.Sc., Reader in Bio-Chemistry in the University of Bristol. London: J. & A. Churchill, Ltd., 1934. (Pp. viii + 319.) 21*s*. net.

Introduction to Physiological Chemistry. By Meyer Bodansky, Ph.D., Director of Laboratories, John Sealy Hospital, Galveston, and Professor of Pathological Chemistry, University of Texas. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. xii + 662, with 39 figures.) 25*s*. net.

Annual Review of Biochemistry. Vol. III. Edited by James Murray Luck, Stanford University. Stanford University, California: Stanford University Press; London: H. K. Lewis & Co., Ltd., 1934. (Pp. viii + 558.) $5.00.

The Hardness of Metals and Its Measurement. By Hugh O'Neill, D.Sc., M.Met., Senior Lecturer in Metallurgy in the Victoria University of Manchester. London: Chapman & Hall, Ltd., 1934. (Pp. xiv + 292, with 119 figures, including 24 plates.) 25*s*. net.

A Practical Chemistry. For Higher School Certificate and Intermediate Students. By G. P. McHugh, M.Sc., Ph.D. (Lond.), M.Ed. (Dunelm), Head of Aristotle Road L.C.C. Commercial and Technical Institute. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. viii + 117, with 17 figures.) 2*s*. 9*d*.

Outlines of Physical Geology. By Chester R. Longwell, Professor of Geology, Adolph Knopf, Professor of Physical Geology, and Richard F. Flint, Assistant Professor of Geology, Yale University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. vi + 356, with frontispiece and 297 figures.) 18*s*. 6*d*. net.

Tertiary Faunas. A Text-book for Oilfield Palæontologists and Students of Geology. Vol. II: The Sequence of Tertiary Faunas. By A. Morley Davies, D.Sc., A.R.C.S., F.G.S., Hon.F.R.G.S., Reader in Palæontology in the University of London (Imperial College of Science and Technology). London: Thomas Murby & Co., 1934. (Pp. x + 252, with frontispiece and 28 figures.) 15*s*. net.

A Textbook of General Botany for Colleges and Universities. By Richard M. Holman, Associate Professor of Botany in the College of Letters and Science of the University of California, and Wilfred W. Robbins, Professor of Botany in the College of Agriculture of the University of California. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. xvi + 626, with frontispiece and 463 figures.) 25*s*. net.

Plant Chimæras and Graft Hybrids. By W. Neilson Jones, Hildred Carlile Professor, University of London. Methuen's Monographs on Biological Subjects. London: Methuen & Co., Ltd., 1934. (Pp. viii + 136, with 21 figures.) 3*s*. 6*d*. net.

The Families of Flowering Plants. II. Monocotyledons. Arranged according to a new system based on their probable phylogeny. By J. Hutchinson, F.L.S., Assistant in the Herbarium, Royal Botanic Gardens, Kew. London: Macmillan & Co., Ltd., 1934. (Pp. xiv + 243, with 107 figures.) 20*s*. net.

Identification of the Timbers of Temperate North America. Including anatomy and certain physical properties of wood. By Samuel J. Record, M.A., M.F., Sc.D., Professor of Forest Products, Yale University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. x + 196, with frontispiece, 6 plates and 47 figures.) 18*s*. 6*d*. net.

Principles of Animal Biology. By A. Franklin Shull, Professor of Zoology in the University of Michigan. With the collaboration of George R. Larue and Alexander G. Ruthven. Fourth edition. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xiv + 400, with frontispiece and 290 figures.) 21*s*. net.

Electrokinetic Phenomena and their Application to Biology and Medicine. By Harold A. Abramson, M.D. American Chemical Society Monograph Series. New York: The Chemical Catalog Company, Inc., 1934. (Pp. 331, with frontispiece and 106 figures.) $7.50.

Cellular Respiration. By Norman U. Meldrum, M.A. (Edin.), Ph.D. (Cantab.), Beit Memorial Research Fellow, the Biochemical Laboratory, Cambridge. Methuen's Monographs on Biological Subjects. London: Methuen & Co., Ltd., 1934. (Pp. xii + 116, with 17 figures.) 3*s*. 6*d*. net.

The Buckland Lectures. Third Series for 1932. The Natural History of the Herring in Scottish Waters. By Henry Wood, M.A., Ph.D., Senior Naturalist, Marine Laboratory of the Fishery Board for Scotland. Aberdeen: The Fishing News, 1934. (Pp. 36, with 7 figures and 2 charts.) 6*d*.

Modes of Behavioral Adaptation in Chimpanzee to Multiple-choice Problems. By Robert M. Yerkes, Yale University. Comparative Psychology Monographs No. 47. Baltimore: The Johns Hopkins Press, 1934. (Pp. 108, with 8 figures.) $1.50.

A Field Study of the Behavior and Social Relations of Howling Monkeys. By C. R. Carpenter, Yale University. Comparative Psychology Monographs No. 48. Baltimore: The Johns Hopkins Press, 1934. (Pp. 168, with 16 plates and 3 figures.) $2.25.

Essentials of Histology. Descriptive and practical. For the use of students. By Sir E. Sharpey-Schafer, F.R.S., formerly Professor of Physiology in the University of Edinburgh. Thirteenth edition. Edited by H. M. Carleton, M.A., B.Sc., D.Phil., Lecturer on Histology in the University of Oxford. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. x + 618, with 721 figures and 2 plates.) 15*s*. net.

The Doctor and His Patients. By Albert Krecke. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1934. (Pp. vi + 301, with 1 plate.) 10*s*. 6*d*. net.

Whither Medicine? By Josef Loebel, Dr. Med. Translated by L. Marie Sieveking and Ian Morrow. London: Sidgwick & Jackson, Ltd., 1934. (Pp. xii + 296.) 7*s*. 6*d*. net.

Mind and Nature. By Hermann Weyl, Institute for Advanced Study, Princeton. The William J. Cooper Foundation Lectures 1933, Swarthmore College. Philadelphia: University of Pennsylvania Press; London: Humphrey Milford, 1934. (Pp. viii + 100, with 12 figures.) 6*s*. 6*d*. net.

The Endless Quest. Three Thousand Years of Science. By F. W. Westaway. London and Glasgow: Blackie & Son, Ltd., 1934. (Pp. xx + 1080, with 48 plates and 193 figures.) 21*s*. net.

Faraday. By Thomas Martin. Great Lives Series. London: Gerald Duckworth & Co., Ltd., 1934. (Pp. 144.) 2*s*. net.

Africa. A Social, Economic and Political Geography of its Major Regions. By Walter Fitzgerald, M.A., Senior Lecturer in Geography in the Victoria University of Manchester. London: Methuen & Co., Ltd., 1934. (Pp. xvi + 462, with 90 maps.) 16*s*. net.

The Unity of Science. By Rudolf Carnap, Professor of Philosophy in the German University of Prague. Translated by M. Black. Psyche Miniatures, General Series No. 63. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1934. (Pp. 101.) 2*s*. 6*d*. net.

General Sociology. By Henry Pratt Fairchild, Professor of Sociology, New York University. Wiley Social Science Series. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. x + 634, with 8 charts.) 23*s*. net.

Habitat, Economy and Society. A Geographical Introduction to Ethnology. By C. Daryll Forde, Professor of Geography and Anthropology in the University of Wales. London: Methuen & Co., Ltd., 1934. (Pp. xvi + 500, with frontispiece and 108 figures.) 15*s*. net.

Russian Sociology. A Contribution to the History of Sociological Thought and Theory. By Julius F. Hecker, Ph.D. With a Foreword by Sidney Webb, P.C., LL.B. London: Chapman & Hall, Ltd., 1934. (Pp. xvi + 313.) 8*s*. 6*d*. net.

Three Essays on Sex and Marriage. By Edward Westermarck, Ph.D., Hon.LL.D. (Glasgow and Aberdeen), Late Martin White Professor of Sociology in the University of London. London: Macmillan & Co., Ltd., 1934. (Pp. x + 353.) 12*s*. 6*d*. net.

Human Sterilisation To-day. A Survey of the Present Position. By Cora B. S. Hodson, F.L.S. The Forum Series, No 19. London: Watts & Co., 1934. (Pp. viii + 56.) Cloth 1*s*. net; Paper 7*d*. net.

Birth Control To-day. By Marie Carmichael Stopes, D.Sc. (London), D.Phil. (Munich). London: John Bale, Sons & Danielsson, Ltd., 1934. (Pp. 237.) 5*s*. net.

The Ancestry of the Long-Lived. By Raymond Pearl and Ruth DeWitt Pearl, The Johns Hopkins University. Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. (Pp. xiv + 168, with frontispiece and 20 figures.) 13*s*. 6*d*. net.

Rebel Destiny. Among the Bush Negroes of Dutch Guiana. By Melville J. Herskovits and Frances S. Herskovits. New York and London: Whittlesey House, McGraw-Hill Book Co., Inc., 1934. (Pp. xviii + 366, with 15 plates.) 12*s*. 6*d*. net.

Modern Miniature Cameras. By Robert M. Fanstone, A.R.P.S. London and Glasgow: Blackie & Son, Ltd., 1934. (Pp. x + 70, with 12 plates.) 3*s*. 6*d*. net.

The Scientific Journal of the Royal College of Science. Vol. IV. London: Edward Arnold & Co., 1934. (Pp. 172, with illustrations.) 7*s*. 6*d*.

Transactions of the Bose Research Institute, Calcutta. Vol. VIII, 1932–33. Biological and Physical Researches. Edited by Sir Jagadis Chunder Bose, M.A., D.Sc., LL.D., F.R.S., C.S.I., C.I.E. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. vi + 266, with 131 figures.) 21*s*. net.

Abstracts of Dissertations for the Degree of Doctor of Philosophy. Vol. VI (October 1932–December 1933). Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. (Pp. vi + 303.) 3*s*. 6*d*. net.

Uncle Joe's Nonsense. For Young and Old Children. A Medley of Fun and Philosophy Reported by J. W. Mellor, Honorary Secretary of the Ceramic Society. London, New York, Toronto: Longmans, Green & Co., for the Ceramic Society, 1934. (Pp. xii + 231, with 127 illustrations.) 12*s*. 6*d*. net.

What is the Fourth Dimension? By Charles W. R. Hooker, O.B.E., M.A., B.Sc. London: A. & C. Black, Ltd., 1934. (Pp. viii + 110, with frontispiece and 19 figures.) 5*s*. net.

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CLOUDS IN THE SKY AND IN THE LABORATORY

BY SIR GILBERT WALKER, C.S.I., Sc.D., F.R.S.

THE feeling aroused by beautiful sunsets is so satisfying that some may dislike the idea of trying to analyse their physical meaning; and the pattern may be so complex as to make the task appear impossible. But at times the sky is covered with clouds of a uniform type that throws out an emphatic challenge. If we respond to it the conditions do not as a rule lend themselves readily to adequate mathematical analysis, and laboratory methods appear to offer the greatest promise of affording the clue.

As far back as 1855 E. H. Weber noticed a polygonal pattern in a thin layer of a solution of gamboge in spirit; surface evaporation chilled the liquid so that the upper layers became heavier than the lower; the equilibrium became unstable and some form of motion was inevitable. Subsequent experimenters, notably Bénard, have shown that when this motion has become steady it occurs in cells, usually pentagons and hexagons, with liquid rising in the centre of each and descending at the interfaces between the cells. In 1920 Idrac published observations of the motion of unstable liquids along a slightly sloping trough. Owing to viscosity the liquid flowed more slowly near the bottom of the trough than at the free surface, so that there was shearing motion between the upper and lower layers. The combination of the instability with the shear produced long straight vortices, easily seen if there is aluminium powder in the volatile liquid employed: these are alternately right- and left-handed with their axes parallel to the direction of the trough and so to the direction of the shear. The general scheme will be seen in Fig. 1.

It was inferred by both these investigators that their results would have application to cloud forms; but there was little direct evidence, and there was the alternative analogy of ordinary sea waves with their fronts at right angles to the velocity of the wind relative to the water. Accordingly current meteorological text-books generally accept the interpretation of long straight roll-clouds (Fig. 2) as forming in the horizontal interface between two air masses moving with different velocities, and the direction of the rolls is supposed to be perpendicular to the relative motion, or " transverse " as it may be called.

Now from time to time a cloud-gazer will see a series of parallel rolls weakening by degrees and gradually being replaced by a series of rolls at right angles to them ; when the transformation is half-way towards completion there is a crossed pattern. The change takes about five minutes and in the next five minutes there may be a similar change back to the original system of rolls. Six years ago an observation of this kind and the frequent occurrence of rectangular patterns convinced me that systems of parallel rolls are not due to Helmholtz waves ; for this would demand that the direction of relative motion above and below the cloud should always jump through a right angle and this is inconceivable. A further laboratory investigation was needed, using air instead of a liquid and trying various rates of shear. Three pupils in succession have carried on the work. The first, S. Mal, only got definite results with liquids. But A. C. Phillips and I met with some success when using a shallow wind channel, with a heated steel bottom and a glass top, through which air could be drawn by a fan. The length was 20 cm., the width 15 cm. and the height 6 mm. ; and the motion was made visible by titanium tetrachloride. If the air was allowed to come to rest the polygonal pattern was that shown in Fig. 3. When the fan was rotating, however. although results of interest were obtained, the conditions were scarcely comparable with those in the sky, where there is in general only one layer of shear between two air-masses travelling with different velocities, whilst we had two layers of shear in opposite

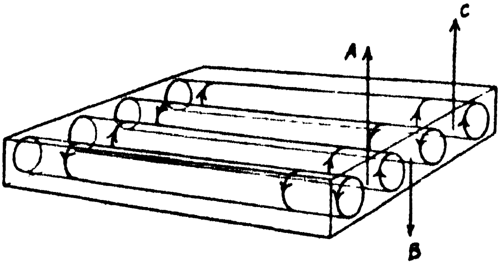


FIG. 1.

directions. A change of plan was necessary and A. Graham used a stationary heated iron bottom with a top formed by a long glass plate which was drawn along at a constant rate. When conditions had become steady and the speed of the glass plate was over 2·5 cm. a second, the space was filled with pairs of straight vortices, with alternately right- and left-handed rotations, and their axes parallel to the motion, or " longitudinal." (See Fig. 4, in which, as in Figs. 5 and 6, the plate moves from left to right.) The combination of forward and rotational motion in a vortex means that the path of a particle is spiral, and when the tobacco smoke used was unevenly distributed the stream lines of smoke could be seen in spiral patterns.

On giving the glass plate a somewhat slower motion a rectangular pattern (Fig. 5) was produced, the lines of the cloudlets being at times parallel and perpendicular to the shear, and at times inclined at 45 degrees to it.

If the velocity of the glass plate was reduced still further, there formed a system of vortices at right angles to the motion, or " transverse " (Fig. 6) ; and in these again the motion was alternately in opposite directions. Thus while half of them behaved like rollers between the upper and lower surfaces of the channel, half of them were rotating against the relative motion. With these transverse vortices there was no motion of translation along the axis, so that spirals did not appear.

It is important to notice that in Figs. 4 and 6 the vortices occur in pairs, each pair being separated by a distinctive line of demarcation from other pairs : we are in fact dealing with units formed of two oppositely rotating vortices, and these units may be called " cells." Even in Fig. 3 these can be made out ; and as we watch the development of the polygons from the pattern of fairly straight cells produced when smoke is drawn into the trough, we see that, as the velocity dwindles to zero, kinks appear here and there convulsively until the linear cells have all been replaced by polygons.

Let us now see what light these laboratory experiments throw on phenomena in the sky, and let us refer again to Fig. 1. In the laboratory we limit the motion vertically as in the slab there sketched, and introduce the smoke half-way up, so that it scarcely reaches the top and bottom boundaries. Accordingly in each cell we see two sharply marked white rolls. But in the sky there is nothing to guarantee that the condensation into cloud, which corresponds to the smoke, will occur in the plane through the axes of the vortices. The regions of ascending air are at A and C while descent occurs at B; so we usually see a long strip of cloud above the central lines of the cells at A and C, while above B, between the cells, the air is descending and we have blue sky. Similarly in rectangular, transverse and polygonal cells we usually have the interfaces between the cells indicated by blue sky, and the area within covered by cloud which is frequently ill defined at its margin: accordingly in Fig. 2 each roll lies above the axis of a cell. On some days we have the situation illustrated in Fig. 7, in which the newly formed clouds have clean outlines and are uniformly white; but those evaporating show their structure and the innate spiral texture appears; an example of the latter will be found in the cloud half-way up the photograph on the right side.

The range between the maximum and minimum rates of shear for the formation of rectangular patterns seems to be rather small, for it is not very often that these are seen well developed at low and medium cloud heights: but in "mackerel" skies they are not rare. An example at a moderate height is given in Fig. 8 as seen from below, and one in Fig. 9 as seen from an aeroplane. The latter shows the dome-shaped upper surface that we should expect in condensation above an ascending current, and this explains a feature characteristic of all clouds formed by instability within a layer—that the upper edge appears more sharply defined than the lower. When seen from below obliquely, a ray of light from the upper margin runs tangential to the cloud's rounded surface, so that a ray just below it has to travel through some thickness; but one from near the lower margin has but little thickness to penetrate and this edge appears woolly. The difference between the edges is well shown in Fig. 2.

Transverse clouds are common at all heights and are illustrated in Fig. 15. They can as a rule be distinguished from longitudinal by three features:

(*a*) Transverse cells arise when the shear is very slight, and longitudinal when it is considerable. So any turbulence superposed on the motion due to shear will produce large effects on the direction of a transverse cell, but small effects on that of a longitudinal cell. Accordingly transverse cells are often sinuous and resemble sand-ripples, while longitudinal are in general nearly straight. Another effect of the sluggish shear is that the rotation in the vortices may die out locally. Let us call *d*, *d'* the vortices of a cell D whose axis is marked by a strip of cloud, and *e*, *e'* the vortices of the next cell E, so that *d*, *e* rotate in the same direction, and *d'* lies between them. It may happen that in a nearly stagnant area *d*, *d′* and *e* cease to be continuous with their prolongations beyond the area. Then *d′* and *e* may join up to form a continuous vortex, the rotation being unbroken because either *d′* or *e* must turn through 180° to form a continuous curve. So *d* is free to run on into the prolongation of *e*, and the effect on the cloud strips above the ascending lines is that the strips D, E have joined and become continuous with the prolongation of D. The prolongations of *d* and *d′* may join up as *d′* and *e* did, or they may become continuous with *c* and *c′*, the vortices of the cell C adjacent to D on its other side. In other words the feebleness of the shear facilitates the joining up of adjacent cloud lines.

(*b*) The surface markings on the vortices tend to be distinctive. While those on longitudinal rolls are spiral, those on transverse rolls will be, as we have seen, perpendicular to the axis of the cell.

(*c*) The length of a longitudinal cloud-roll, and the length of a longitudinal line bounding a cloud pattern, is much greater than that of a transverse roll. Consider a horizontal area in a steady current of air: a difference of humidity between two points P, Q in that area will, when the local conditions below are tolerably uniform, be determined mainly by past history or by the origin of the air. So points with the same humidity will tend to lie along the lines of flow. If then we have one steady current flowing above a feebler current roughly in the same direction, the lines of constant humidity will tend to lie along the lines of shear between the currents; and so the conditions are favourable for length in longitudinal rolls, or longitudinal boundaries, and unfavourable for great length in transverse rolls unless the distribution of humidity is abnormally widespread. Accordingly, while the length of longitudinal clouds may run into scores of kilometres that of transverse clouds is usually limited to one or two kilometres.

In transverse, as in longitudinal clouds, the level at which condensation occurs is generally higher than the plane through the axes of the vortices. But occasionally we have rolls occurring in pairs as sharply defined as the laboratory products of Fig. 6: there will be seen on the right-hand side of Fig. 14 a good example of such a pair, and a second pair of vaguer outline.

Corresponding to the polygonal pattern which we have in the laboratory when there is no shear, the clouds are usually hexagonal or pentagonal, and well or ill defined according to the height of the condensation, the latter being the more frequent. Fig. 10 is interesting not only for the good definition of the margins of the cells, but also because it suggests that wrinkles were left in the vortices when they were bent up into polygonal form.

A curious fact may now be mentioned. In the laboratory when working with unstable liquids—either oil in a dish of which the bottom is heated or a volatile liquid of which the surface evaporates—it is found that the liquid *rises* in the centre of the cells whatever form they assume: but in air there is always *descent*.

Now from the hydrodynamical standpoint, assuming that the boundary conditions are the same at the top and bottom surfaces of the unstable fluid, a solution with ascent in the axes of the cells is the same, apart from sign, as a solution with descent; for the lower layers are trying to rise just as much as the upper layers are trying to fall. So both solutions should be possible; and we should expect that heating a liquid at the bottom and thereby setting up slight irregularities of temperature there would produce ascending streams above the hotter places, and so up-currents in the centres of the cells; conversely, cooling the liquid at the top should produce down-currents in the centres. In point of fact, however, if we blow locally on to a small area in the centre of a circular dish of spirit, the steady motion set up is of ascent there.

The paradox has been studied by Graham, and his solution turns on the fact there is an initial motion which may not be identical with the steady motion which rapidly develops. The initial motion is of the nature that might be expected. Thus if hot water is poured into a dish and cold milk introduced over it, the milk descends initially in streams which show as circular dark patches, and I have seen circular holes in a sheet of cloud in very rough weather when a strong wind was blowing to it over a high ridge. These holes were as sharply defined as those in the milk.

Graham's conclusion is that when the motion due to instability has become steady the vertical temperature gradient in the fluid is adiabatic, and hence that there will be a fairly sudden change of temperature either at the top or bottom. The question where will be settled by the boundary conditions, at which viscosity will play an important part. But while the viscosity of a liquid decreases with temperature, that of a gas increases: so the viscosity of the air will be greater at the bottom of the layer and that of the liquid at the top. Now the rate at which energy is dissipated by friction per unit volume will be greatest in the layer of rapid transition where the temperature gradient exceeds the adiabatic, and if the fluid is a gas will be greater if the sudden change occurs at the bottom than at the top. But in steady viscous motion the configuration is such as to make the rate of dissipation a minimum. Thus in air the change will occur at the top and minute patches of excess cold there will set up cold downward currents in the centres of cells: conversely in liquids upward currents will start from the lower boundary.

In the sky we know too little of the bounding conditions to be able to dogmatise; but we should certainly expect steady down-currents to be produced occasionally in the centre of cells when the motion is steady. When these cells are longitudinal or transverse the reversal of the directions of rotation in Fig. 1 will not make much difference, except that cloud will appear above B and blue sky will be visible at A and C. In the case of square or polygonal cells, however, the cooling due to ascent at the margin will produce a strip of cloud there while the descending current in the centre will permit the blue sky to be seen. Thus we should expect these "reverse" clouds to show a "reticular" pattern, like that of netting; and such clouds are of tolerably frequent occurrence, especially, I am told, in higher latitudes. In this country they may often be seen at cirrocumulus level, especially in the morning and evening: but they also form at ordinary altitudes. Clouds of this type are shown in Fig. 11.

When we consider the bounding surfaces of an unstable layer we naturally suppose that the upper one will be an inversion, above which the temperature rises with altitude, and the air below being too cold, cannot ascend through it. Any turbulence in the region below will tend to equalise the ratio of water vapour to air in that region, and so to increase the relative humidity at the top of the region where the temperature is lowest; there is therefore a tendency to form cloud there if anywhere. Now a layer of cloud will cool above by radiation into space, and will be heated below by radiation from the earth; and these are the effects that will produce instability. This interpretation is confirmed by my own experience that well-defined polygonal patterns are more frequent in the morning and evening than at midday. Durst also found in the records more instability clouds on moonlit nights than in the daytime. In such cases it may be that the heating of the lower surface of the cloud would form an inversion there and act as a barrier.

However, this explanation in terms of radiation is not free from difficulty. For it would lead us to expect in general the formation of a dense continuous cloud sheet before the broken-up pattern appears, whereas the latter usually come into being when there is nothing more than the thinnest of films. Also we cannot appeal to the radiation from and to a layer of air in which the water vapour, though abundant, has not condensed; for that radiation would scarcely be large enough. So it may be that as soon as the thin film forms it becomes unstable and breaks up, and that the subsequent growth takes place on this as basis. This may explain the observed fact that a patch of polygonal cloud often fades out at its edges, and that the size of the polygons, being roughly proportional to the thickness of the unstable layer, there diminishes accordingly. This may be seen in Fig. 10.

A conceivable auxiliary hypothesis is that the formation is aided by the radiation from and to dust-layers, which according to some authorities, play an important part in the formation of clouds. The only example that I have seen was in an aeroplane ascent over Berlin in the course of which at a height of 3 or 4 kilometres we suddenly emerged into clear air through which the sun shone with surprising brilliance; the existence of the dust was imperceptible from below, but its upper margin was fairly sharp and, seen from just above, was light brown in colour.

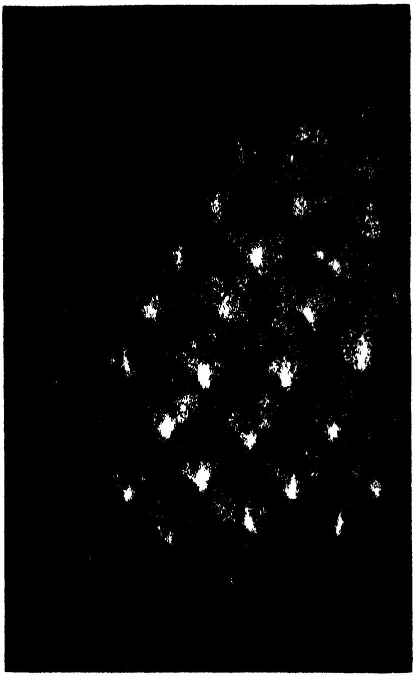
It may be that the lower boundary of the cells is at times not definite but of gradual transition, and also that at times the cells descend to the ground. For over tropical deserts we have daily a mass of unstable air extending upwards to a considerable height. Violent up-currents are produced and, in the absence of sufficient humidity to form cloud, show themselves by the dust that they collect. These dust-devils may be seen in large numbers from a hill, travelling over the plain, but I do not know whether they tend to occur in the regular pattern that we should expect if conditions were uniform over a wide expanse.

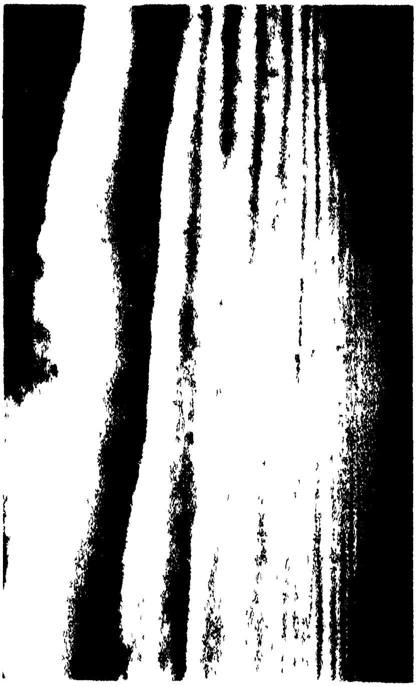
Analogous cloud formations, with heights greater than their diameters, do however present themselves at times in patterns that are fairly regular. Thus in Fig. 12 we have a layer in which condensation makes the ascending columns visible, and at their summits, presumably at an inversion, there are denser masses of cloud. As a rule the columns though parallel are inclined.

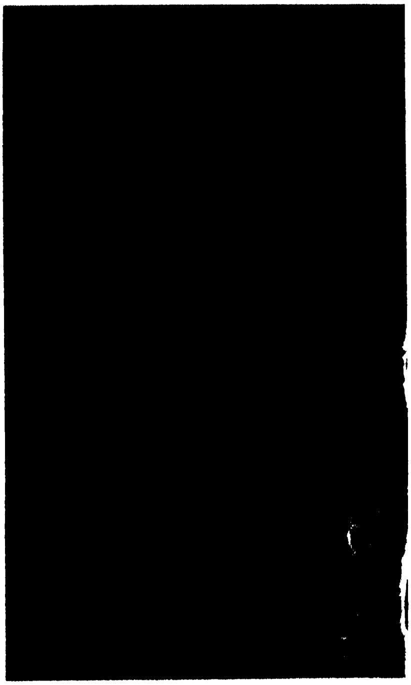
The question now arises whether cells of this type occur not in a skeleton but in a complete form, just as longitudinal cells may appear either as opaque rolls or as spirals. We are at once reminded of the type that we often see when moderately strong winds are blowing; these clouds are as dense as ordinary convection clouds but have their axes sloping forwards with the wind at a uniform inclination of something like 40°. The first thought

PLATE I

Fig. 3







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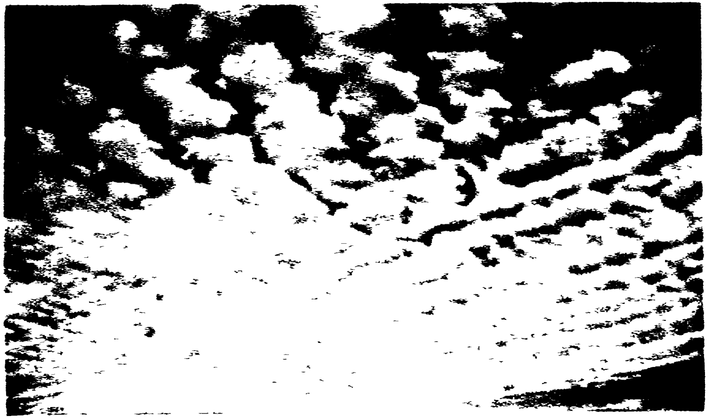

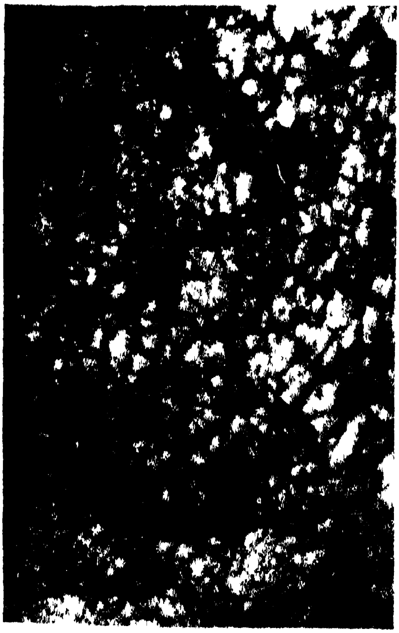

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PLATE IV

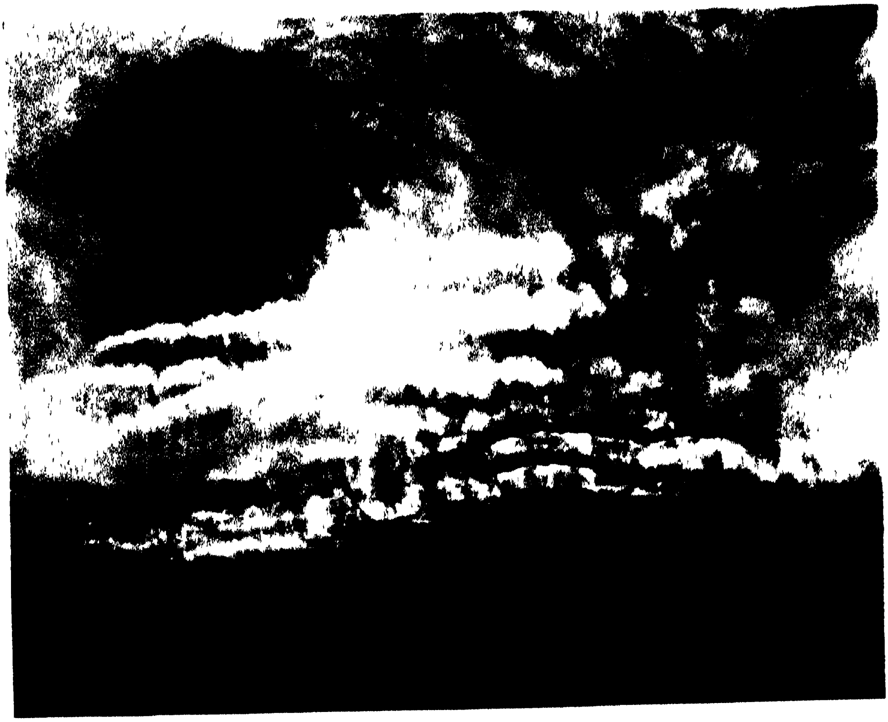

Fig. 14

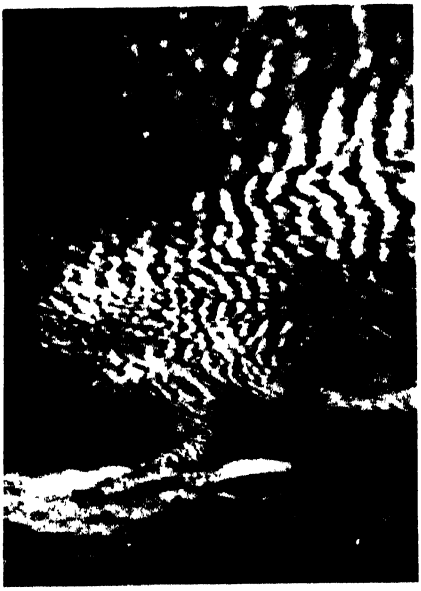

Fig. 16

may be that this slope is the direct consequence of conveyance of different levels with the velocity of the air at these levels, the wind increasing with height. But such conveyance would imply a continually increasing angle between the axes and the vertical, and rapid disintegration: the permanence of the slope and of the outline seems to indicate a definite velocity pattern, *i.e.* a cell formation.

When we discuss structures with a thin fibrous axial portion surmounted by a dense cloudlet, we are entering on controversial ground; for clouds of this type occur at all heights, and when at high or medium levels the axial portions are very often called "Fallstreifen," being interpreted as precipitation from the clouds above them. Wegener maintained that practically all thread-like cirrus was falling as precipitation: but to this view there are two objections. First that, as we see in Fig. 13, it often happens that there is no cloud from which the ice particles can fall, while in our view such a situation merely means lack of relative humidity or of condensation nuclei at the top of the ascending column. The second objection is that Wegener applies the term to the cirrus that forms the "anvil" above a thundercloud. Now its shape of an inverted cone is quite naturally explained as due to the air rising above the cloud and spreading out because its upward motion is checked by an inversion above the anvil. But if these cirrus threads are falling from a cloud it is hard to see why they should converge so rapidly as they fall. Also in general there is no cloud from which they can be dropped.

The forms of high clouds, being composed of ice particles, differ in important respects from those of lower clouds consisting of minute water drops. The threads of which many of them are made were perhaps originally stream lines, but they last for a considerable time, so that they get twisted and bent by turbulent motion, as in Fig. 13. Thus at cirrus level a series of longitudinal cells has the appearance not of solid rolls, but of a number of empty cells from a honeycomb; and a sheet made visible by the lines of ice particles may at times persist until it is rolled up into a conical form like the bell of a flower. Attractive effects are also produced when the axes of cells in lines at regular intervals undergo similar distortions: an example of flower-like products is given in Fig. 16.

One of the commonest of patterns at cirrus level is a narrow strip of cloud with thin lines projecting on each side almost at right angles to it and often slightly bent downwards. The strips may perhaps be due to condensation in such lines as A, C of

Fig. 1, combined with stream lines of condensation in the lines of outflow in the rotating cylindrical shells right and left of them.

But there are many points still needing elucidation. Thus apparently it happens too often that in longitudinal cells showing spirals, instead of right- and left-handed screws occurring in equal numbers, there is an excess of one sign. It may be that conditions of visibility are responsible, but this scarcely seems an adequate cause in some cases.

It will be seen that the study of cloud patterns has definite advantages. Although it is inevitably classified as meteorological, its demands for specialist knowledge are not severe. Most lovers of nature have qualifications enough; and though their country rambles will offer the best opportunities, they will find ample subjects for study in the cloudscapes visible from an open square or a house roof, even in a large city. The only equipment necessary, in view of the extreme rapidity of the changes, is a camera with a colour filter so that the photographic records may be examined at leisure.

In conclusion I would express my obligations to J. Evershed for much help and for many hours of work in the dark-room, and to C. J. P. Cave and G. A. Clarke for allowing me to use their collections of photographs. I am also indebted to Harold Floreen of Cheyenne, Wyo., U.S.A., for Fig. 14.

THE CORROSION OF METALS

By U. R. EVANS, M A., Sc.D.

A STRANGE feature of the teaching of chemistry in this country is the present neglect of its metallic province. Of the ninety-two elements, some seventy-two may be claimed as metals; yet these seventy-two metals and their numerous compounds probably receive less attention than is bestowed on the compounds of one single non-metal, carbon. It may be that those responsible for the allotment of time in the curriculum are hardly aware of the new developments in metallic chemistry, possibly because—in the admirably executed abstracts issued by the Bureau of Chemical Abstracts—much of the new research on metals is assigned to the "B" series, which is rarely read by the pure scientist. Whether this is the reason or not, it is likely that the asymmetric allotment of time is producing misconceptions among the rising generation of chemists, who come to believe that the laws and principles dominant in the part of the subject actually studied are general over the whole range of elements. Yet it may be doubted whether the doctrine of valency, as it is sometimes taught to chemists to-day, is very helpful in explaining the occurrence of such compounds as $CuAl_2$, Mg_2Cu or $MgCu_2$; and it is difficult to avoid misgivings about the teaching of the Law of Definite Proportions if the student is not warned of the numerous apparent exceptions; if, for instance, he is left in ignorance of the fact established by Pfeil [1] regarding the substance known as "ferrous oxide" and usually written as FeO, which is found to contain anything between 75·0 and 76·9 per cent. of iron, but to become unstable before it reaches the iron content (77·7 per cent.) indicated by the formula FeO.

The cases cited should not be taken to mean that metallic chemistry is an anarchical region, ungoverned by laws, but rather that its laws are different from those in force in that portion of chemistry which receives preferential treatment in the lecture-room. Many similar examples are met throughout metallic chemistry, and nowhere so frequently as in the study of Corrosion—a term which may be taken to cover all changes from the elementary condition into the combined state. Here we have a vast subject, the importance of which is impressed upon every user of metals by practical and financial considerations; it has indeed been calculated that the annual cost to the world of metallic corrosion exceeds the annual cost of armaments. That being the case, it is not surprising that the industrial chemist pays much attention to changes of this character. Yet the pure chemist regards them as matters of no interest to himself. This appears to be regretable, since, just because they are largely governed by factors different from those commonly operative in non-metallic chemistry, a short study of the processes involved in the destruction of metals would correct to some extent the lack of balance in the curriculum to which I have referred.

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[1] L. B. Pfeil, *J. Iron Steel Inst.*, 1931, **123**, 248.

In the teaching of physical chemistry, there has been, until recently, a tendency to approach the subject of chemical change from the standpoint of energy rather than of geometry. If a change would result in a decrease of free energy, that change, it was argued, is capable of occurring spontaneously; in calculating the velocity, the idea of critical energy increments was introduced and usually led to a rational and self-consistent interpretation of the measurements made in the laboratory. There is little doubt that, fundamentally, these conceptions apply equally well to the corrosion of metals. In some cases, energy considerations become determinative; it may be doubted, for instance, whether the velocity of corrosion of the "hydrogen evolution type" can be calculated from basic principles without the use of quantum mechanics, on the lines suggested by Gurney.[1] But in many other types of attack, the factor which actually limits the velocity of attack, or even decides between *corrosion* on the one hand and *passivity* on the other, is not any factor involving energy; more often these questions are decided by purely geometrical considerations, which determine the access of some substance to the metallic surface.

To give an example. In the industrial process for recovering tin from tin plate scrap by means of chlorine gas, the chlorine removes the tin quite readily as tin chloride, but when the iron basis is reached, the attack very conveniently ceases. This is not because chlorine has less "affinity" for iron than for tin; it is merely because tin chloride is a liquid or a gas, whereas chlorides of iron are, at the temperatures in question, solids. Consequently as soon as the tin has been removed, the iron surface is converted in situ to a layer of solid chloride, which protects the underlying metal from further attack. The truth of this explanation is seen when the material has been insufficiently dried ; in such a case, the iron also forms a liquid as the corrosion product, the chlorides of iron being extremely hygroscopic, and becomes seriously attacked.

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[1] R. W. Gurney, *Proc. Roy. Soc.* (A), 1931, **134**, 137; 1932, **136**, 378.

Other simple illustrations are to be found in a research by Parsons.[1] This American experimenter tested the action of solutions of iodine in numerous organic liquids upon a series of metals. Whenever the iodide of the metal was capable of being dissolved (or peptised) by the liquid in question, attack proceeded freely ; whenever the iodide remained on the metal as an undissolved solid, the attack was caused to slow down. But the arrest in the corrosion by the formation of a solid corrosion-product will not, in general, be instantaneous ; studies of the attack upon silver by a solution of iodine in chloroform or other organic liquid—carried out by L. C. Bannister at Cambridge[2]—showed that the rate of attack slowed down gradually as the film thickened, and under many conditions the velocity at any moment was inversely proportional to the film thickness which had been reached. A series of experiments in which the time, temperature, solvent, iodine-concentration and surface condition of the metal were varied in turn gave considerable information as to the manner in which iodine passes through the film of iodide.

Somewhat similar principles appear to govern the oxidation of metals at high temperatures. Pilling and Bedworth,[3] in a well-known paper, published eleven years ago, classified the metals according to their behaviour to oxygen at high temperatures. If we neglect the noble metals, whose oxides decompose at the temperatures in question, and likewise metals like molybdenum, which possess volatile oxides, we are left with two main classes :

(a) the *ultra-light metals* forming *porous oxides* ;

(b) the *heavier metals* forming *non-porous oxides*.

Simple calculations from the specific gravities shows that the oxide formed upon very light metals, such as magnesium or calcium, cannot occupy the whole volume of the metal destroyed and must therefore be porous or fluffy ; such a product can hardly obstruct the access of further oxygen to the metal below, and the oxidation should suffer no retardation with time ; in other words the curve connecting oxidation with time should be a straight line, and this Pilling and Bedworth's work on calcium showed to be the case.

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[1] L. B. Parsons, *J. Amer. Chem. Soc.*, 1925, **47**, 1830.

[2] U. R. Evans and L. C. Bannister, *Proc. Roy. Soc.* (A), 1929, **125**, 370.

[3] N. B. Pilling and R. E. Bedworth, *J. Inst. Met.*, 1923, **29**, 529.

The heavier metals (and it should be noted that aluminium stands in this class) form oxides which if uncompressed would occupy a volume greater than the metal destroyed during their production; a metal of this class, heated in air, gives a non-porous oxide-film in a state of lateral compression and the rate of oxidation will fall off as the layer thickens; in the simplest cases, the velocity of thickening is found to be inversely proportional to the thickness.[1] Complications naturally arise if the film develops cracks, or if it "sinters" to give a material of smaller permeability. The subject becomes still more involved where several layers consisting of different oxides appear on the surface; as shown by Pfeil,[2] iron forms three such layers.

Evidence that the lateral compression in the oxide-film demanded by the Pilling-Bedworth principle really does exist, and that it plays a part in affording protection, is given by the recent experimental work of Finch and Quarrell,[3] who found by an electron-diffraction method that the oxide-film formed on zinc is pseudomorphic after the metal and has a structure slightly different from that of massive zinc oxide, being in lateral compression in the direction parallel to the metal and correspondingly in extension at rightangles. At elevated temperatures, the film assumes the structure of stable unstrained zinc oxide, becoming unprotective and permitting the formation of fresh pseudomorphic zinc oxide below.

Pilling and Bedworth's principle has been extended by Fischbeck [4] to the action of halogens at ordinary temperatures; the attack by dry halogens is only violent in those cases where the ratio of the volume of the compound to that of the metal destroyed in producing it is less than unity; such a condition is clearly necessary for the formation of a porous non-obstructive product. The condition is, however, rarely fulfilled; in most cases calculation shows that the ratio exceeds unity. For sodium bromide the ratio is 1·36, and sodium has been preserved in bromine for a year without sensible change; but for potassium bromide the ratio is only 0·95, and potassium explodes when introduced into liquid bromine.

It is impossible here to discuss the vast amount of work on high temperature oxidation, but passing reference may be made to the work of Dunn [1] and of Vernon [2] on the effect of temperature, and that of Wilkins and Rideal [3] on oxidation at low pressures. Of great practical importance is the work of Murphy and Jominy,[4] of Heindlhofer and Larsen [5] and of Cobb [6] on the effect of gaseous impurities, such as sulphur dioxide; these accelerate attack, possibly by reducing the obstructive character of the oxidised layers. Special attention should be paid to the papers of Hatfield,[7] of Portevin [8] and of Thyssen [9] on the effect of alloying elements, like aluminium or chromium, which reduce attack by increasing the protective character of the scale.

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[1] If y be the thickness at time t, $dy/dt = k_1/y$, or $y^2 = 2k_1t + k_2$ where k_1 and k_2 are constants.

[2] L. B. Pfeil. *J. Iron Steel Inst.*, 1929, **119**, 501; 1931, **123**, 237.

[3] G. I. Finch and A. G. Quarrell, *Proc. Phys. Soc.*, 1934, **46**, 148.

[4] K. Fischbeck, *Zeitsch. Elektrochem.*, 1933, **39**, 324.

At ordinary temperatures in dried air or oxygen, metals appear capable of existing for long periods without visible change, simply because the oxide-film, which is formed with the utmost rapidity, prevents the access of oxygen molecules to the metal below, and thus ceases to thicken appreciably before ever it reaches visible thickness. Such films, invisible whilst on the metal, become visible when removed, and numerous methods have been worked out for stripping these " invisible " films and studying their properties.[10] But changes may occur, at ordinary temperatures, if the air is not pure and particularly if it contains both moisture and certain atmospheric impurities, such as sulphur dioxide. The extensive work of Vernon [11] and J. C. Hudson [12] on atmospheric corrosion brings out the effect of the physical character of the corrosion product in determining the course of attack. In some cases, such as the tarnishing of copper exposed indoors to an unsaturated London atmosphere containing traces of sulphur compounds, Vernon found the velocity of attack to fall off as the film thickened; in other cases, such as the attack upon zinc by a similar atmosphere, the product formed after an initial period appeared to cause very little additional obstruction, and the curves connecting weight-increment and time became straight lines. He also described instances where the velocity actually rose with the time; this was true of iron in an humid atmosphere sometimes reaching saturation. Particularly interesting was the behaviour of aluminium in an unsaturated atmosphere, where the action was practically brought to rest by the film of corrosion-product, and then suddenly recommenced, owing apparently to the cracking of the film. Here it is interesting to compare Vernon's microgravimetric curves, with the curves obtained optically by Steinheil[1] on aluminium films obtained by condensation on mica—a method which gives a surface quite free from oxide at the commencement of the experiment; the curves, like those of Vernon, show how the rate of oxidation comes almost to rest when the film reaches a certain stage.

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[1] J. S. Dunn, *Proc. Roy. Soc.* (A), 1926, **111**, 203, 210; *J. Inst. Met.*, 1931, **46**, 25.

[2] W. H. J. Vernon, *J. Chem. Soc.*, 1926, p. 2273.

[3] F. J. Wilkins and E. K. Rideal, *Proc. Roy. Soc.* (A), 1930, **128**, 394, 407: F. J. Wilkins, *J. Chem. Soc.*, 1931, p. 330.

[4] D. W. Murphy and W. E. Jominy, *Univ. of Michigan. Eng. Res. Bull.*, No. 21 (1931).

[5] K. Heindlhofer and B. M. Larsen, *Trans. Amer. Soc. Steel Treatment*, 1933, **21**, 865.

[6] J. W. Cobb. Unpublished work.

[7] W. H. Hatfield, *J. Iron Steel Inst.*, 1927, **115**, 483.

[8] A. Portevin, E. Prétét and H. Jolivet, *Rev. Met.*, 1934, **31**, 101, 186, 219.

[9] M. H. Thyssen, *J. Iron Steel Inst.*, 1934.

[10] U. R. Evans, *J. Chem. Soc.*, 1927, p. 1020; U. R. Evans and J. Stockdale, *J. Chem. Soc.*, 1929, p. 2651.

[11] W. H. J. Vernon, *Trans. Faraday Soc.*, 1924, **19**, 839; 1927, **23**, 113; 1931, **27**, 255, 582; *J. Inst. Met.*, 1932, **48**, 121; *Trans. Electrochem. Soc.*, 1933, **64**, 309.

[12] J. C. Hudson, *Trans. Faraday Soc.*, 1929, **25**, 177; *J. Inst. Met.*, 1930, **44**, 409; *J. Birmingham Metallurgical Soc.*, 1934, **14**, 331.

Special reference should be made to Vernon's studies of the behaviour of copper towards air containing sulphur dioxide and moisture in carefully regulated amounts. It was found that sulphur dioxide without moisture produced no effect, and moisture without sulphur dioxide very little effect; but that very rapid attack occurred when both were present in the right proportions. At fixed humidity, the rate of attack increases with the sulphur dioxide concentration to a maximum, then sinks again, reaches a minimum at about 1 per cent. sulphur dioxide, and then rises once more; in atmospheres of 1 per cent. concentration the reaction product is normal copper sulphate, and it would appear that this is more protective than the products formed at lower or higher sulphur dioxide concentrations, which contain excess of base or acid. The effect of varying the humidity is remarkable; up to about 50 per cent. humidity the attack is slow, even in atmospheres rich in sulphur dioxide, but at 75 per cent. relative humidity and above it becomes very rapid. This illustrates an important principle previously brought out in the earlier experiments of Vernon and Hudson and further elucidated by Patterson and Hebbs,[2] namely the *Principle of Critical Humidity*. When the relative humidity of the atmosphere rises above a certain value (probably the value at which the corrosion-product can absorb moisture readily from the atmosphere), the corrosion velocity increases quite suddenly; the critical humidity may fall far below 100 per cent., since many corrosion products are distinctly hygroscopic. Often the corrosion product does not "look wet," although some experiments of the author,[1] in which about ten metals were tested in closed vessels containing moist air contaminated with different acid or alkaline gases (sulphur dioxide, hydrogen chloride, ammonia, etc.), indicated that corrosion was exceptionally rapid in those cases where the corrosion product was sufficiently hygroscopic to make the specimen appear conspicuously wet to the eye.

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[1] A. Steinheil, *Ann. Physik*, 1934, **19**, 475.

[2] W. S. Patterson and L. Hebbs, *Trans. Faraday Soc.*, 1931, **27**, 277.

In late years, very great interest has been taken in the copper steels, which exposure tests in different countries indicate as more resistant than ordinary steels to the acid-rich atmospheres of industrial districts; the extensive series of tests which is being carried out by Dr. J. C. Hudson[2] for the Iron and Steel Institute appears to be bearing out this view. The corrosion-product formed on the copper steels is generally thought to be more clinging and uniform than that on ordinary steels, and apparently has more protective power—to judge from Passano's analysis[3] of some tests at Bucktown. German researches[4] have shown that metallic copper is deposited on the steel, doubtless through the reduction of copper-compounds in the first-formed rust by iron. This deposition of copper at the point where corrosion first happens to start, may well divert attack elsewhere, and thus lead to a uniform sheet of rust.

Other examples of the control of corrosion by a supply of some necessary product are provided in the work of Bengough, Stuart, Lee and Wormwell[5] on zinc and iron. A disc of zinc placed some way below the surface of very dilute solution of potassium chloride undergoes corrosion at a rate which falls off with the time. Experiments showed that chlorine ions were removed by the corrosion product and, since chlorine ions are needed for the attack upon the metal, the corrosion-rate fell off with the time, being at any moment proportional to the chlorine ion concentration remaining. This assumption leads immediately to an equation connecting corrosion velocity and time.[1] But oxygen, as well as chlorine ions, is needed for rapid corrosion, and if the chlorine ion concentration at the commencement of the experiment is high, the maximum velocity permitted by the chlorine ion concentration cannot be achieved, because the rate of supply of oxygen is insufficient. Thus at the higher concentrations of salt, it is the rate of oxygen supply which controls the velocity of attack, and since oxygen is less soluble in concentrated solutions than in dilute solutions, it is found that, under the conditions employed by Bengough, concentrated salt solutions corrode less fast than more dilute solutions.

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[1] U. R. Evans, *Trans. Faraday Soc.*, 1923, **19**, 201.

[2] J. C. Hudson, *Second Report of the Iron and Steel Institute Corrosion Committee* (1934).

[3] R. F. Passano, *American Society for Testing Materials*, Symposium on outdoor weathering of Metals and Metallic Coverings (1934).

[4] C. Carius and E. H. Schulz, *Mitteilungen aus dem Forschungsinstitut der Vereinigten Stahlwerke A.G.*, 1929, **1**, 177.

[5] G. D. Bengough, J. M. Stuart, A. R. Lee and F. Wormwell, *Proc. Roy. Soc.* (A), 1927, **116**, 425; 1928, **121**, 88; 1930, **127**, 42; 1931, **131**, 494; 1931, **134**, 308; 1933, **140**, 399.

This refers to totally immersed horizontal specimens. In the case of vertical specimens of zinc sheet half immersed in potassium chloride solution, Borgmann's experiments at Cambridge indicate that the corrosion increases steadily as the potassium chloride concentration is increased.[2] With rod specimens, where the rounded form is, for geometrical reasons, less adapted for the renewal of the oxygen, there is, under half-immersed conditions, a falling off of the corrosion velocity at high concentrations. With iron, there is a falling off of the velocity at high salt concentration even with half-immersed sheet specimens, and the same is true of immersion in solutions of other salts, such as sodium chloride.[3]

These statements of laboratory results may appear surprising to those familiar with practical cases of corrosion, since it is generally thought that salt solutions become more dangerous as they become stronger. But many of these apparent conflicts between laboratory work and practical experience rest upon a failure to distinguish clearly between (*a*) the *danger* of corrosion setting in at all, and (*b*) the *velocity* with which that corrosion will proceed, supposing that it has once begun to develop.[4] Recent work at Cambridge by Mears[5] on the action of small drops of potassium chloride solution on iron has shown that the *velocity* of attack (in those drops which produce corrosion at all) first increases with the salt concentration to a maximum and then decreases again; but the *proportion* of drops which develop corrosion at all increases steadily as the concentration is increased, until they all corrode; no decline in the proportion corroding has been detected in concentrated solutions. It should be added, however, that there are other salts, such as sodium bicarbonate, where the proportion of drops developing corrosion is *less* at high concentrations than at low ones.

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[1] If the chlorine removed by the corrosion-product is proportional to the corrosion (y), and the velocity (dy/dt) is proportional to the chlorine remaining, then $dy/dt = k_4(C_0 - k_3 y)$, where C_0 is the original concentration and k_3 and k_4 are constants.

[2] C. W. Borgmann and U. R. Evans, *Trans. Amer. Elektrochem. Soc.*, 1934, **65**, 256.

[3] U. R. Evans and T. P. Hoar, *Proc. Roy. Soc.* (A), 1932, **137**, 350.

[4] U. R. Evans, R. B. Mears and P. E. Queneau, *Engineering*, 1933, **136**, 689.

[5] R. B. Mears, unpublished work.

It is a general principle that corrosion tends to stifle itself when it leads to a compact solid product in physical contact with the surface, that is to say when the primary product of the attack is sparingly soluble. This is the reason why the corrosion of metals immersed in liquid so often takes an *electrochemical course*. Let us consider the case of zinc in water containing oxygen. Any direct form of chemical change which converted the metal *in situ* into sparingly soluble zinc hydroxide might be expected to stifle further attack; and indeed Bengough's time-corrosion curves for zinc in distilled water below oxygen show that the corrosion-rate does fall off with the time. But now let us suppose that the liquid also contains sodium chloride, and that for some reason an electric current flows from one part of the surface to another. Familiar electrochemical reactions will then produce at the anodic points soluble zinc chloride, and at the cathodic points soluble sodium hydroxide; where they meet, they may give sparingly soluble zinc hydroxide (or perhaps a basic chloride), but this solid matter will be produced at a perceptible distance from the point of corrosion and will not slow down the attack perceptibly. The E.M.F. producing such an electric current may be set up in numerous ways; it may arise through chemical or physical differences between different parts of the metallic surface, or through differences in the state of the invisible oxide film, or through differences in the liquid (for instance, through unequal supplies of oxygen to different parts). But whatever the cause of the potential-difference—and in practice an absolutely equipotential surface is extremely unlikely to exist—the corrosion is able to continue without stifling itself, *provided that* the primary products of attack are *freely soluble bodies*. If the primary corrosion product at the anodic parts were to be a sparingly soluble body, for instance lead sulphate, electrochemical attack would stifle itself just as easily as direct chemical attack. Thus lead immersed in a solution of potassium sulphate is only very feebly corroded. More often, however, electrochemical action produces soluble bodies at anode and cathode, and this is the only reason why wet corrosion at low temperatures more often takes an electrochemical than a direct chemical course. Where the product of direct union is freely soluble (*e.g.* in the action of iodine on iron), there is probably a direct attack, without any passage of currents between distinct anodic and cathodic areas, unless the iodine has access only to one part of the surface.

The distribution of attack by a salt solution under simple conditions is easily understood when the electro-chemical mechanism is considered. Much may be learned from watching a drop of potassium chloride solution placed on the horizontal surface of iron which has been abraded and exposed for a short time to dry air. In order to show up the anodic and cathodic points, it is convenient to add to the salt solution a trace of phenol-phthalein and potassium ferri-cyanide; if the quantities of the two indicators added are very small, the distribution of attack is not modified by their presence. The phenol-phthalein serves to show the cathodic regions (where potassium hydroxide is produced) by a pink colour, whilst the ferricyanide marks the anodic regions (where the ferrous chloride is produced) by a blue colour.

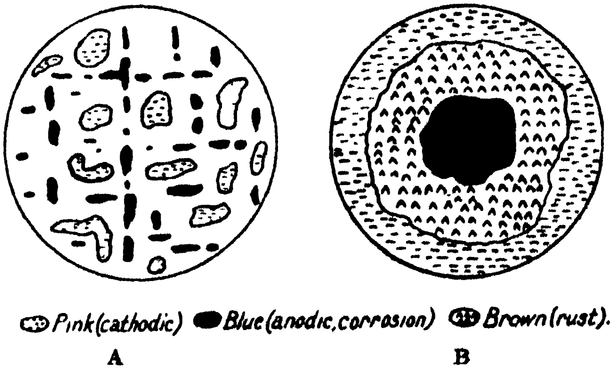

FIG. 1.

If the drop contains dissolved oxygen, blue anodic points soon appear, aligned along the abrasion marks; simultaneously pink patches appear in all the parts of the drop (Fig. 1 A). But soon, in the peripheral zone, the blue points disappear and the pink points join together to form a pink ring round the edge. Similarly in the central region, the pink points disappear, and the blue points spread out to form a blue core; then between the blue and pink regions, there appears a brown ring of rust (Fig. 1 B). The explanation is simple.[1] At the anodic points (*i.e.* the places where the invisible film of oxide is weak) the iron is attacked producing ferrous chloride; at the cathodic points, where the film is strong, potassium hydroxide is formed. But the cathodic reaction calls for oxygen as a depolariser, at least if it is to proceed rapidly. When in the centre of the drop the oxygen originally present has been used up, the production of potassium hydroxide ceases. The formation of ferrous chloride in the centre can, of course, still continue, but the corresponding production of potassium hydroxide must now take place in the peripheral zone, where the oxygen of the air has access. Thus ferrous chloride is formed in excess in the interior of the drops, whilst potassium hydroxide is formed in excess in the exterior. Any ferrous chloride formed at susceptible points in the peripheral zone is at once precipitated by the potassium hydroxide and oxygen there present, and the hydrated ferric oxide formed in physical contact with the metal stifles the attack. Thus the peripheral zone becomes immune from corrosion, whilst the attack continues at the centre of the drop, where the original surface of the metal soon becomes eaten away. Where the ferrous chloride and the potassium hydroxide meet one another ferrous hydroxide is formed, which soon combines with further oxygen to form hydrated ferric oxide (brown rust). The transition from the primary distribution of attack (Fig. 1 A) to the secondary distribution (Fig. 1 B) is clearly caused by the disappearance of the original oxygen in the central part of the drop. For, if the liquid is carefully *freed from oxygen* before being placed on the metallic surface, the secondary distribution appears *immediately*; but if the liquid is *super-saturated* with oxygen before the drop is placed in position, the primary distribution survives for an abnormal length of time.

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[1] U. R. Evans, *Korrosion und Metallschutz*, 1930, 6, 77.

The state of corrosion of an iron plate half-immersed in a vertical position in a solution of a sodium or potassium salt gives analogous results. Here again the alkaline hydroxide formed along the water-line—the only part to which oxygen has free access—protects the metal locally against attack; thus, although corrosion needs oxygen for rapid attack, the particular part of the specimen most accessible to oxygen remains immune, whilst at places lower down where anodic attack proceeds quite readily, no oxygen can be detected at all.

This case has been examined quantitatively by Hoar in the Cambridge Laboratory,[1] and it has been found that the electrical currents flowing between the cathodic zone along the water-line and the anodic parts lower down where attack is proceeding explain accurately the velocity of corrosion actually observed. It is difficult to measure directly the current flowing through the metal, but by placing next the cathodic part of the surface, a narrow tubulus leading to a calomel electrode, it is possible to measure the local potential difference between metal and liquid in the cathodic zone; now under any given conditions the potential at a cathode will vary by polarisation with the current passing, and, by a preliminary study of the relation between current and potential, it is possible to use the measurement of the potential at the cathodic portions of any corroding surface to obtain a knowledge of the current passing between anodic and cathodic zones. In this way, an electrical measurement of the corrosion velocity can be obtained without any assumption of arbitrary constants, and the agreement with the directly measured values, as indicated on Fig. 2, is very satisfactory.

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[1] U. R. Evans and T. P. Hoar, *Proc. Roy. Soc.* (A), 1932, **137**, 343.

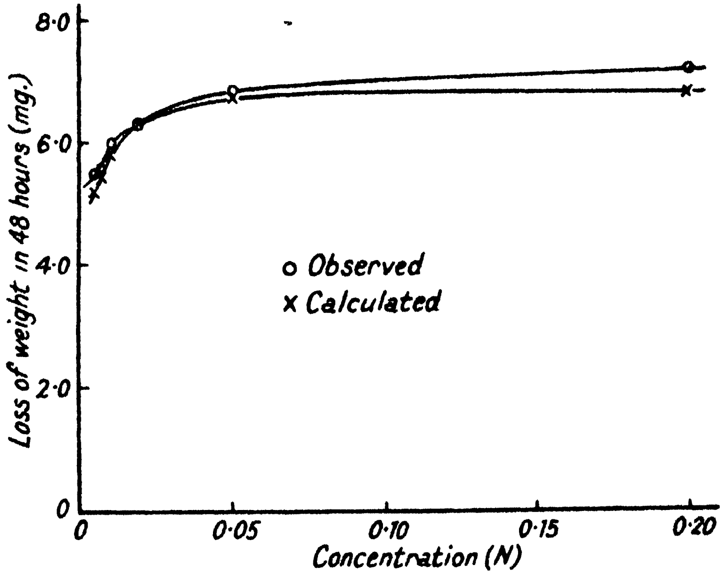

Fig. 2.

It is impossible to deal here with the work of W. J. Müller[1] and Hedges[2] on Anodic Corrosion and Passivity, or with the work of Palmaer[3] on corrosion of the Hydrogen Evolution Type. The reader who has time to study the books of those three investigators may be surprised to find the extent to which they have cleared up these difficult subjects.

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[1] W. J. Müller, *Bedeckungstheorie der Passivität* (Verlag Chemie).

[2] E. S. Hedges, *Protective Films on Metals* (Chapman & Hall).

[3] W. Palmaer, *The Corrosion of Metals* (Svenska Bokhandelscentralen, A. B.).

To the technical chemist, the causes of corrosion may seem less important than its prevention. Space does not permit the discussion of protection processes, but it may be mentioned that these also depend, in many instances, on the principles already enunciated. Thus recent work at Cambridge by Britton [1] and Lewis [2] indicate that the well-known anti-corrosive paint, red lead, is capable of preventing corrosion even when there is definite access of water and oxygen to the metal through a scratch or pore in the paint, and it does this by causing the corrosion-reaction to stifle itself through rapid precipitation of the incipient corrosion-product in physical contact with the metal. In this way, red lead paint differs from iron oxide paint, which only prevents corrosion in so far as it excludes the corrosive agents mechanically from the metal. Potassium or sodium chromates, which are often used to render water non-corrosive, act by reinforcing with a mixture of iron and chromium oxides the weak spots in the air-formed oxide-film (which is in itself quite incapable of withstanding the ions present in any ordinary water). The film, normally invisible, becomes—as usual—visible when separated from the metallic basis. On stainless steel containing chromium, this protective admixture is probably formed over the whole surface. The work of Forrest, Roetheli and Brown [3] shows that stainless steel, tested in the film-free condition, commences to corrode as quickly as ordinary steel, but that, in contrast with ordinary steel, the corrosion-rate falls off almost at once, as a new skin, usually invisible, forms over the surface.

It is evident from what has been written that in the Corrosion of Metals, *Geometry is often more important than Thermodynamics*, and that it is often less imperative to know *what* has been produced than *where* it has been produced. The topochemical principles long emphasized by Kohlschütter have a special significance in corrosion—although their importance extends to many branches of chemical science; on this point the reader may be referred to the "Festschrift" recently published in celebration of Professor Kohlschütter's jubilee.[4]

One other matter deserves mention. It is commonly considered that relentless controversy exists on the subject of corrosion, and indeed this supposed state of affairs has often been quoted as a pretext for refusing to study the subject at all. It is true that twelve years ago, there was little agreement between the advocates of rival theories, and any chemist who may then have sworn to waste no further time in reading the polemics will doubtless remain unaware that the position has changed. As a matter of fact, the majority of laboratories where corrosion research is proceeding, are to-day in essential agreement on the basic principles, although discussion still continues on numerous important points of detail. There are, of course, one or two Laboratories which differ more fundamentally from the generally accepted opinion ; but even here the controversy does not apply to the whole field of the subject, although there has sometimes been a tendency to lay more stress on points of disagreement than on points of accord. It is sometimes thought that between the Research Laboratories at Teddington and Cambridge there is no agreement ; but of the nine basic points [1] enunciated in the last paper from the Teddington Laboratory, seven are in essential agreement with points to be found in contemporary Cambridge papers. There is no need for a dispute regarding priority ; it is unimportant which laboratory, if either, reached these conclusions first. The important thing is that the views, although expressed differently, are very much the same, and, although the agreement is obscured by lengthy discussions about other points where genuine differences persist, it would be a pity if on this account the large area of common ground were overlooked.

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[1] S. C. Britton and U. R. Evans, *Trans. Electrochem. Soc.*, 1933, **64**, 43.

[2] K. G. Lewis and U. R. Evans, *J. Soc. Chem. Ind.*, 1934, **53**, 25T.

[3] H. O. Forrest, B. E. Roetheli and R. H. Brown, *Ind. Eng. Chem.*, 1930, **22**, 1197.

[4] *Kolloid-Zeitschrift*, 1934, **68**.

I would like to thank Professor R. S. Hutton, Dr. G. D. Bengough, Dr. W. H. J. Vernon, Dr. T. P. Hoar and others, who have kindly read through this paper and offered helpful suggestions.

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[1] G. D. Bengough and F. Wormwell, *Proc. Roy. Soc.* (A), 1932, **140**, 407.

SOIL STRUCTURE IN RELATION TO VEGETATION

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THE term "soil" so readily brings to our mind the cultivated ground of our gardens and arable fields that we are too apt to think of it in terms of these artificial approximations to homogeneity. It is, indeed, true that by far the greater part of the data we possess respecting the composition and characteristics of soils have been obtained from cultivated areas. Nevertheless, valuable as these data are, they tend to minimise those features of soil structure which are of considerable importance in the life of higher plants, but which, in cultivated ground, are insignificant as compared with their import under natural conditions. The soil of a primitive woodland, an ancient chalk down, or a stretch of heath displays an organisation of a definite character, often resulting in such abrupt changes as we pass downwards as to constitute a marked stratification, which permits us to distinguish definite soil horizons.

The extensive work carried out during recent years upon the climatic differentiation of soil types has shown how large a part is played by meteorological conditions in determining the misnamed soil profile. At the one extreme we have the soil differentiation under conditions of low temperature and high precipitation, in which the downward trend of water movement in the soil is greatly in excess of the upward movement, due to evaporation from the soil surface. Such conditions inevitably lead to the removal of soluble substances from the upper layers so that a gradient of leaching is established which may be so complete as to produce a podsol or podsolic type in which the surface soil, whether covered or not by a layer of humus, is practically devoid of soluble plant foods, whilst below there may even be an enrichment through a transport of substances from above. Such soils, as is well known, are characteristic of the great Northern coniferous forest belt. At the other extreme, where soil differentiation occurs under conditions of low humidity and high temperature, upward movement of soil water usually preponderates, and results in the accumulation and concentration of salts at the surface, producing alkaline soils, such as the bad lands of America or the salt steppes of Trans-Caspia, where halophytic vegetation can alone flourish. Under less extreme conditions the direct effect of climate is more profoundly modified by other factors.

It must be emphasised that though soil types do tend to correspond in general with climatic regions, in the sense that for any given climatic complex there is probably a prevailing soil type, yet the geological character of the substratum and the constitution of the plant covering can appreciably modify the nature of the soil, subjected to a particular complex of meteorological conditions. Especially is this the case where the climatic factors are not extreme. Even in the more pronounced climatic conditions, a variety of sub-types of soil profile may be encountered, such as, for example, are associated with the divers types of spruce forest in Finland, characterised by different dominants in the ground flora. Whortleberry, for example, accelerates the leaching process, which is much slower under a carpet of vegetation dominated by lichens.

Some plants exploit a soil extensively, with a widespread and deeply penetrating root system, which is, however, not copiously branched, whereas other kinds of plants exploit the soil intensively with a richly branched root system that ramifies throughout the volume of soil occupied. Such differences, when associated with species that are social in habit, must inevitably affect the depth and degree of incorporation of organic material into the mineral matrix, both directly, by the death and decay of the roots, and indirectly, by their effect upon the pore system. Further, the distribution of the root system has a direct influence upon the process of leaching, owing to the respiration of the roots themselves. The carbon dioxide which is thus liberated causes more rapid leaching around the roots, as can often be seen in chalky soils, where the clayey residue from the parent rock commonly forms an investment around the larger root system. So, too, the nature of more secular changes is affected by the degree of saturation or unsaturation in respect to bases of the organic plant residues.

Evidence of the progress of leaching is especially well shown in calcareous soils, where, as brought out in the accompanying data from chalk downs at Tring, the surface soil is considerably depleted of carbonate, a process which is markedly affected by local topography, as can be seen by comparison of the carbonate gradient on the level part of a down with that exhibited by a slope.

TABLE I

PERCENTAGE CALCIUM CARBONATE

Depth in inches.	Slope Loc. A	Slope Loc B.	Gentle Slope.	Level Down Loc. A.	Level Down Loc. B.
0–1	46·2	41·0	10·3	3·0	0·07
1–2	52·0	48·0	10·0	6·7	0·01
2–3	56·2	60·5	11·9	7·8	0·04
3–4	61·5	64·0	11·1	9·9	0·04
4–6	—	—	11·2	23·0	0·04
6–8	—	—	17·0	44·0	0·9

But after the carbonates have been completely removed the leaching process continues, involving the replacement of the exchangeable bases until the colloidal fraction is completely unsaturated. Since, however, the calcium carbonate is most readily removed it follows that, in highly calcareous soils, there may, as a consequence of differential leaching, be an actual increase of some substances as compared with the parent rock, so that a reverse gradient in respect to potassium or phosphorus may be temporarily established, to which reverse gradient the salts liberated from surface litter doubtless in part contribute.

Experiments carried out by Powers, using artificial composts of clay and organic colloids, indicate that such mixtures have a higher base exchange capacity than either constituent possesses alone. Evidence was obtained that one-fifth to two-fifths of clay and three- to four-fifths of organic colloidal material afforded the maximum exchange capacity (*Soil Science*, XXXIV, 1–10, 1932). These results suggest that in woodland soils in this country the horizon a short distance below the raw humus is probably the most favourable in respect to food substances, and this is in fact a region of extensive root development.

In natural soils two factors mainly operate to check the decrease in gradient of soluble constituents from below upwards, which is the natural outcome of the climatic leaching process, both of which tend towards, and may even produce to a slight degree, a gradient in the reverse direction. These two factors are the action of earthworms and the surface dressing of plant débris. They are probably only operative when the under-lying soil, at no appreciable depth from the surface, is rich in soluble salts. Thus, earthworms are only frequent in soils where the subsoil at least is well supplied with calcium carbonate. Under these conditions the earthworms feed in the surface layer, rich in organic material, and often appreciably acid in reaction. The worm casts they deposit on the surface are, however, only mildly acid or even neutral. Worm casts examined by the writer were found to have a reaction of *p*H 5·7 when the soil on which the worms had been feeding had a reaction as acid as *p*H 5·1 (Salisbury, *Journ. Linn. Soc.* (*Bot.*), Vol. 46, p. 415, 1924). Similarly, the leaves of *Merculialis perennis* shed on to the floor of a beechwood may have a definitely alkaline reaction (*p*H 7·4) and contain about 5 per cent. of calcium salts, and thus tend to ameliorate the acidty of the surface on which they fall, whereas the dead leaves of other species, such as the Hair Grass (*Deschampsia flexuosa*) have an acid reaction and contain less than one-fifth of 1 per cent. of calcium salts.

Thus the soil gradients, as conditioned by the microclimate of an area, may be profoundly modified by the character of the plant covering and by the nature of the soil fauna.

It is not our purpose here to consider the different categories of soil profile, of which admirable summaries have been furnished elsewhere (cf. Russell, *Soil Conditions and Plant Growth*, Edit. 6, Chap. 4, 1932), our aim being to consider how this soil structure, evolved as a resultant of the interaction of climate, vegetation, soil flora, soil fauna, and geological origin, affects the life of the green plants that are rooted in it.

Any true soil is composed of five constituent parts, namely (1) the mineral particles of various sizes and in various states of chemical decomposition, (2) the organic material, similarly in varying degrees of chemical change, from the raw litter to humus and more or less complete disintegration, with the consequent liberation of the erstwhile combined bases, (3) the soil solution consisting of various inorganic salts in a very dilute state, and in proportions changing with the water content and other factors, but, in normal conditions, usually totalling between one-fifth of one atmosphere, and one atmosphere of osmotic pressure. (4) The soil atmosphere occupying that part of the pore space not taken up by the soil solution, and consisting of the same gases as are present in the atmospheric air, but in differing proportions, particularly as regards the partial pressure of carbon dioxide, which is often high. (5) The micro-organic population of plants and animals, of which the bacterial and fungal flora and the protozoon fauna are perhaps the most important. The total volume occupied by the micro-flora and -fauna is probably in natural soils well under 1 per cent. of that of the soil as a whole.

In attempting to visualise the three-dimensional structure of the soil in its relation to plant life, the changes of each of these soil constituents with change in depth must be considered.

Although the technique for estimation of numbers of soil organisms still leaves something to be desired, it is nevertheless evident that their numbers decrease rapidly with increasing depth. Thus the carbon dioxide produced copiously by their respiratory activity will be most abundant near the surface, so that the percolating rain water tends to become saturated and its solvent action enhanced at the outset. Further, if ready upward diffusion is hindered by the presence of free water at the surface, or even by a blanket of raw humus, as in the tile-like leaf-litter of a beechwood, this heavy gas will not establish equilibrium with the air above, but will tend to pass down by gravity into the soil interstices, which may thus present very high partial pressures of carbon dioxide. Romell found as much as 2 per cent. of this gas at a depth of 45 cm. in a fen soil, and at only 15 cm. below the surface the amount was still nearly seven times that of atmospheric air (*Medd. Statens Skogsforsoksanstalt*, 124–359, 1922). More recently Godwin, Mobbs and Barucha (*Nat. Hist. of Wicken Fen.*, p. 610, Bowes and Bowes, 1932) have stated that 3–4 per cent. CO_2 is quite typical of the soil atmosphere at a depth of only 20 cm. in Wicken Fen. In beech forests the humus layer may contain about five times the carbon dioxide content of atmospheric air, so that in sheltered woods the air is enriched in this respect by upward diffusion, to the benefit of the assimilation of the herbaceous vegetation. On the other hand, a high carbon dioxide content is inimical to both root and root hair development, as also to their functional activity. The metabolism of the soil organism also produces nitrate, sulphate, and bicarbonate anions, which bring into solution equivalent amounts of calcium and magnesium cations, so renewing the soil solution depleted by absorption of the higher plants. Since the changes in temperature during the growing season are mainly the outcome of heating of the surface by the sun's rays from above, convection currents, whether in the water films or the soil atmosphere, can have little influence in accelerating diffusion either of liquids or gases. Indeed, it is probable that, except when the surface is appreciably hotter than the lower layers of the air, the main impetus to diffusion is afforded by changes in volume, accompanying fluctuations of temperature and barometric pressure. Upward diffusion against gravity through the fine soil capillaries must be so slow when, as in this country, precipitation is in excess of evaporation, as to be almost negligible as a factor influencing the supply of food material for the plants.

In periods of precipitation the extremely dilute solution almost amounting to pure water which the rainfall provides is superimposed upon the soil solution, and thus the difference in density presents a spatial relation that tends to accentuate depletion of solutes from the upper layers, and thus to impoverish the food supply of surface rooting species. Hence the presence of a soil population that is at a maximum close to the surface and which, by its production of anions, enriches the cation supply mainly from above, has a very important significance in the economy of the soil The elements retained near the surface in the bodies of the soil organisms are naturally not subject to leaching until they become liberated as these organisms die. Hence, where the soil population is abundant this source of nutrient supply may be important for higher plants both in respect to its amount and position.

In woodlands the importance of the annual leaf fall, for the maintenance of surface food supply, is very considerable. It has been estimated that in a beech forest this annual supply amounts to between 200 and 300 grams dry weight per square metre. The calcium content of this would be approximately 2¼ per cent. In other words, every square metre of a beech wood receives an annual dressing of between 5 and 7 grams of calcium. Even in a pine wood the litter contains nearly 1 per cent. of calcium, and the annual fall of leaves is about 200 grams, so that there is a potential increment of 2 grams per square metre. The total lime content of the forest floor under deciduous (*Tilia* and *Acer*) and coniferous (*Pinus*) woodland has recently been determined by Alway, Methley, and Young (*Soil Science*, XXXVI, 387, 1933). Calculated as CaO they found over 2,000 lb. per acre under the deciduous trees, as compared with between 200 and 900 lb. per acre under coniferous woods dominated by different species of *Pinus*.

The rate of decay of pine needles is much slower than that of the leaves of deciduous trees, so that the contained bases are not so soon available in an exchangeable form. It should perhaps be emphasised that, since with the decay of the litter its bulk decreases, the amount of salts contained in a unit volume of organic material will increase as decomposition proceeds until the rate of such concentration is surpassed by that of removal through the action of plants or by leaching. Muller found that alike with Soy beans, straw and manure there was a progressive increase in the base exchange capacity from a low value at the outset throughout the period of nine months. The ultimate result is the same whether the process be rapid or slow, since there will always be in the natural habitat litter increments that are in the stage of decay that yields up its contained salts at the maximum rate. The significance of slow decay is perhaps that, as decomposition advances, the litter of a particular year has been buried beneath successive litter increments, so that the conditions of decay become more anaerobic, and, when the appropriate stage of decomposition has been attained, the litter of the particular year may no longer be in the region of maximum root development. The leaves of oak yield rather less than 2 per cent. of calcium, but it is evident that, even those trees which are poor in calcium, will still contribute an appreciable addition to the surface supply. In a similar way the superficial addition of other bases, such as potassium, is to some extent augmented. This annual addition of litter, so pronounced in the woodland, is a feature more or less marked of all natural soils, and in general the rate of decay in our climate does not keep pace with the rate of increment, so that the organic content of the soil tends to increase with the passage of time. This is strikingly illustrated by the dune soils of this country, which show a marked increase of organic content with increasing age, that is largely responsible for the augmenting flora as one passes from the younger dune ridges near the sea face to the oldest ridges on the landward side of the dune system (Salisbury, *Annals of Botany*, XXXVI, 391–431, 1922).

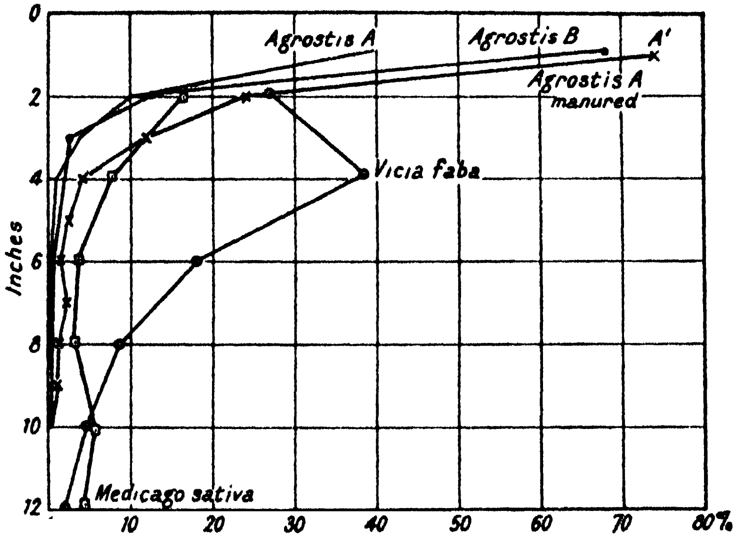


FIG. 1.—Relative Root Development.
(*After data from Sprague and West.*)

The organic matter in the soil has a twofold origin, that which is added superficially, and that which is added interstitially, by the death and decay of roots which have penetrated the soil. Downwash by rain-water and the action of earthworms contribute to the incorporating process. Recently obtained data respecting the vertical distribution of root systems have shown how rapidly the root content of the soil falls off with increasing depth. The data presented in Fig. 1 show this rapid diminution for two species of *Agrostis* investigated under natural conditions by Sprague. The addition of artificial manure to the species A has resulted in a shifting of the curve to the right (A^1). In other words, there is an increased volume of root development at all depths, but the form of the curve remains the same. On the other hand, species growing in cultivated soils, represented by *Vicia faba* and *Medicago sativa*, which have been investigated by West (*Jour. Council. Scientific Indust. Res.*, VII, 87–93, 1934), show a far less rapid diminution. Hence root distribution and the surface additions of litter both conspire to produce an organic gradient of decreasing amount from above downwards. The accompanying data from a few selected

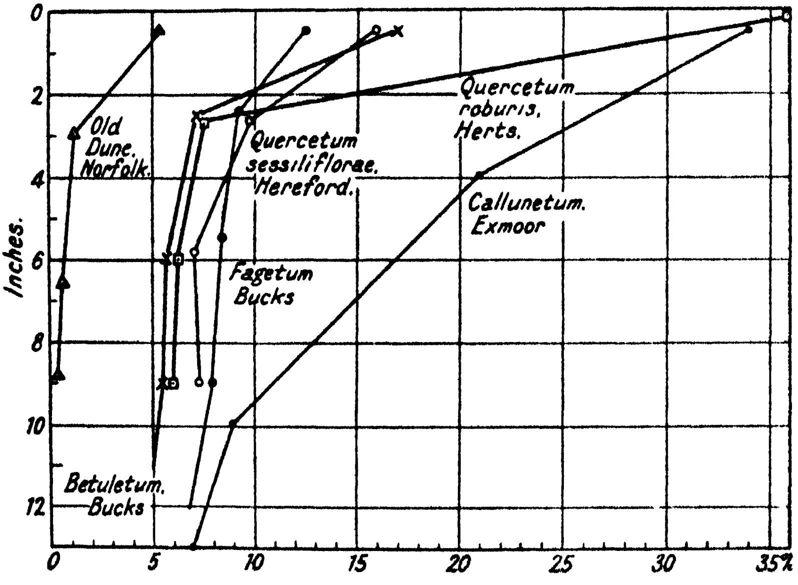


FIG. 2.—Vertical Distribution of Organic Matter.

natural plant communities not only indicate the wide range of organic content of the surface, but also show how general is the rapid fall in organic content as one passes downwards. This brings with it certain characteristics important to plant life. Most significant are those associated with the high water capacity and high base exchange capacity of organic matter.

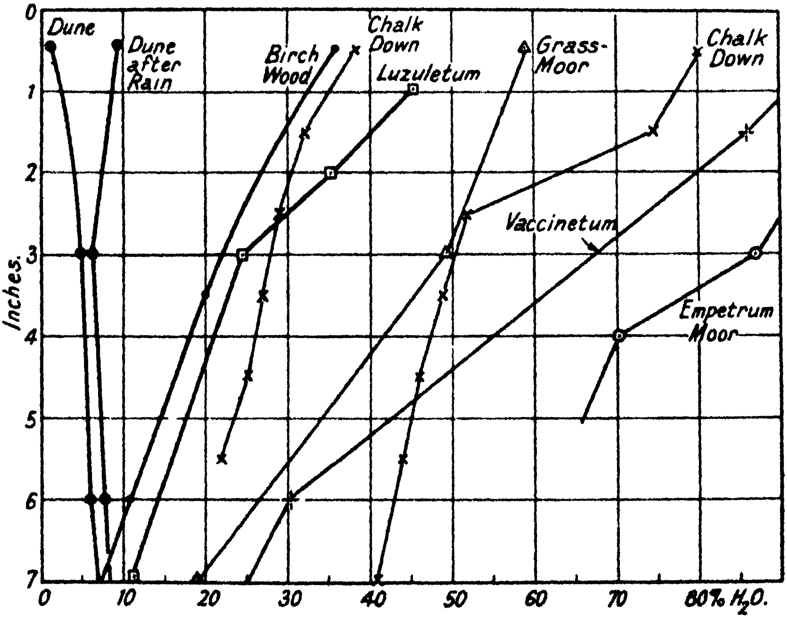
It is in general true that the presence of more than 5 per cent.

TABLE II

GRADIENT OF ORGANIC CONTENT IN VARIOUS PLANT COMMUNITIES

Depth in Inches.	Oakwood (*Quercus robur*).	Oakwood (*Quercus sessiliflora*).	Birchwood (*Betula alba*).	Heather Moor.
	per cent.	per cent.	per cent.	per cent.
0–1	10·5–40	17·5–32	6–17	86
2–3	5·8–11·1	8·1–9·8	3·3–7·4	25
5–6	5·1–7 8	4·8–7·2	2·5–5·8	14
9	4·9–6 5	4·6–6·1	3–5·5	18

organic material is sufficient in most soils to mask differences in the size of the associated mineral particles. Hence it follows that the water retaining capacity of the soil usually, except where evaporation is high, shows a decrease in the upper layers, from above downwards (*cf.* Fig. 3); and, indeed, so great is the water retaining capacity of humus that in woodland areas occupying regions of moderate rainfall the surface accumulation of organic matter may be sufficient to hold up most of the water that falls upon it. Thus a condition may be brought about in which, whilst surface rooting species in a woodland thrive on an adequate water supply, the deeper-rooted trees and shrubs may possibly be suffering from a



Variation of Water Content with Depth.

FIG. 3.—Natural Water Contents from Various Communities.

water deficiency. Old pastures may likewise suffer from drought caused through the surface retention of water conditioned by accumulated litter.

If capillary water be available from the subsoil the water gradient may exhibit an increase with depth, even near the surface, more commonly there is a horizon of minimum water content between surface and subsoil, so that for deeper rooted species increased depth of root brings a more copious water supply. But under any conditions of water supply the organic gradient inevitably has a profound influence on the distribution and availability of water for the plant.

For species with shallow root systems the water availability will in general decrease with depth, whilst for deep rooted species the reverse will commonly obtain, but for either type of root system increased distance from the evaporating soil surface will bring with it a more regular water supply, less liable to become very low, though it may never attain high values. How far marked fluctuations will be deleterious depends in great measure on the relation that obtains between the periodicity of precipitation and the periodicity of leaf production.

A further important effect of the influence of the organic content is upon the pore space. The following data show the average pore space gradient for the first few centimetres in an oakwood on clay and an old pasture on chalk.

TABLE III

Quercetum roburis.		Chalk Pasture.	
Depth	Pore Space.	Depth.	Pore Space.
cm.	per cent.	cm.	per cent.
0–5	10·6	2·5	19
5–8	5	2·5–5	16
8–13	4	7·5–10	11

It is clear from these two examples that the pore space falls off quickly in volume in a manner comparable to the diminution of the organic fraction. One striking effect of the changing pore space is its influence upon the ease with which the soil is penetrated by roots and rhizomes. As a root tip or a rhizome apex grows, the soil particles in its path must be pushed aside and thus be more tightly packed around it. Such displacement, except perhaps in the upward direction, is only possible by reduction of the pore space. Hence the penetrability of the soil is seen to be a function of its porosity. Data obtained by P. S. Gupta in the author's laboratory show the average penetrabilities at various depths in woodlands growing on three types of soil.

TABLE IV

Relative Penetrabilities of Soils

Depth in cm.	Boulder Clay.	Chalk.	Clay.
1–1·5	3·6	5	3·7
3–3·5	2·5	3·4	2·2
5–5·5	1·8	3·0	1·7
7–7·5	1·6	2·7	1·9
9	2·0	—	—

These figures represent the relative ease with which the soil is penetrated in the horizontal direction at the various levels by a narrow-pointed metal rod, and it can be assumed that the relative penetrabilities by a root would be of approximately the same order

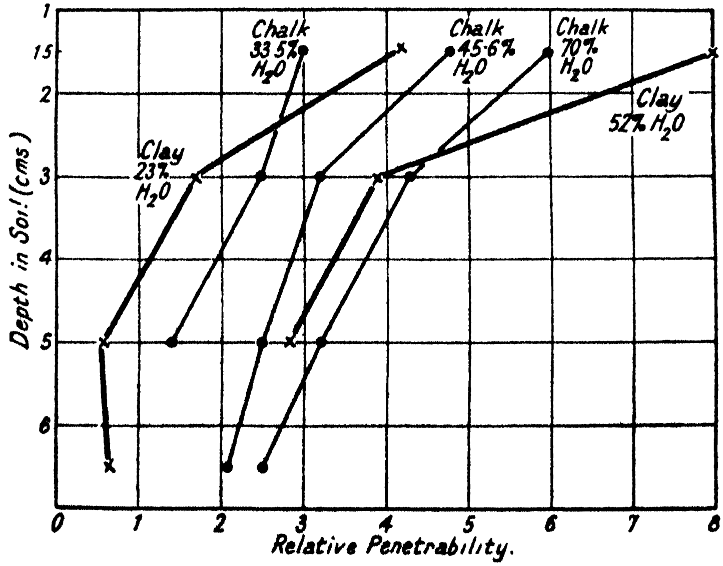


Fig. 4.—Penetrability in Chalk and Clay showing change with depth and water content.

(*Based on data obtained by P. S. Gupta.*)

of magnitude. It is evident from these determinations that the resistance to penetration at a depth of 7 cm. from the surface (about 3 inches) is nearly twice that at a depth of 1–1·5 cm. In Fig. 4 the relative penetrabilities are plotted for one locality on chalk and one on clay at different water contents, and it will be evident from these that the difficulty of penetration becomes greater and the decrease with depth becomes accentuated as the soil becomes drier. Hence the organic matter will not only produce an increased penetrability by its effect on soil porosity, but also by its capacity for water retention. Gupta found that the increase of penetrability with increase in water content obtains in clay soils, chalky soils, and sandy clay soils (*Jour. Ecology*, pp. 452–74, 1933). On the other hand, it is a familiar fact that a sandy foreshore is firmer to walk on when wet than when dry, provided the water content is not excessive. This is indicative of the fact that in sandy soils the penetrability decreases with increasing water content.

The data furnished in Table V show the relative penetrabilities of a dune sand of fine texture at various water contents expressed as percentages of the maximum penetrability when dry. It will be noted that for this particular dune sand the minimum penetrability is associated with a water content of about 5 per cent. of the dry weight. At this moisture content the penetrability is less than a quarter of that when the sand is dry.

TABLE V

PENETRABILITIES OF DUNE SAND AT VARIOUS WATER CONTENTS

	Dry Sand	5% W.C.	10% W.C	20% W C	30% W.C.
Range	100–70	12·5–38	15·6–40	11–40	31–62
Average	85	20	25	26	43

A very interesting feature of the biology of geophytic species may, I think, be correlated with this difference between heavy and sandy soils. The bulbs of *Scilla nutans* and *Allium ursinum*, the root of *Verbascum* and the corm-like rhizome of *Arum maculatum*, are but a few examples of underground perennating organs, which maintain their appropriate level in the soil by means of contractile roots. The longitudinal contraction of these specialised organs is dependent not upon a decrease but upon an increase in volume, which is associated with a change in shape of the cortical cells. These, whilst extending horizontally, diminish longitudinally, but this is accompanied by an intake of water, and so naturally occurs when the water content of the soil is high, *i.e.* when its penetrability is greatest. On the other hand, the geophytic species of *Oxalis*, characteristic of the sandy soils of South Africa, although also adjusting the level of their perennating organs in a similar manner, possess contractile roots whose mechanism is dependent not on a gain but on a loss of water, and so tends to operate in the dry season (*cf.* Thoday, *Ann. Bot.*, XL, pp. 571–83, 1926). Hence in both types of habitat the vertical movement of the bulbs or corms takes place by a mechanism that operates when resistance to the soil is at a minimum.

In addition to determining the penetrability, the pore space has a further significance in facilitating water transport. The plant depends for its water supply not alone on the soil immediately adjacent to its roots, but in very great measure upon the maintenance of a flow in the water films. If the soil capillaries were tubular and completely filled with liquid their efficiency to conduct water would be a function of the fourth power of their radius. The flow of soil solution is, in fact, dependent not only upon its amount, but upon the volume of pore space through which it flows and how this pore space is distributed, whether in the form of comparatively few but large channels, as in a coarse sand, or of innumerable but very narrow capillaries, as in a fine clay. The actual pore space of a clay soil may be as much as 50 per cent. of the total volume, whilst the total pore space of a coarse sand is about 30 per cent. Yet the rate of flow through a sandy soil is something approaching 200 times as rapid as that through a clay soil. It has been estimated that the total surface collectively presented by all the particles in a cubic foot of pure clay would be somewhere between 2½ and 4 acres. The total internal surface of a cubic foot of sandy soil is, however, only about ¼ acre. Actually the resistance to flow through a pure clay soil would be something of the order of 40,000 times that of a coarse sand. Most clay soils, however, are only in part made up of clay particles. Hence, their resistance to flow is not so great, but the large area of internal surface such soils present, though advantageous in retaining a large amount of water, offers great frictional resistance to its movements. The small internal surface of a sandy soil, though retaining far less water, presents but a slight obstacle to its flow towards the root.

It is to variations in the manner of distribution of pore space that the discrepancies between pore space gradients and penetrability gradients can mainly be attributed. In estimating the functional pore space in this respect the diminution due to swelling of the colloids by imbibitional water must be borne in mind, and this varies with the water content and the character of the soil solution.

A further significant effect of pore space is upon the conductivity of the soil for heat. Except in very moist soils, where the continuous water films ensure ready thermal conduction, the lack of intimate contact between the soil particles, due to the pore spaces, is the chief obstacle to the passage of heat. Thus it is that the dark-coloured litter in a wood on a sunny spring day may act in a manner similar to that of a black bulb thermometer and attain to such a temperature that the over-lying air in which the foliage of the prevernal species is situated is sufficiently warmed to enable these plants to assimilate when the air generally is below the critical temperature for that process. So, too, the writer has recently shown that on sand dunes the poor contact between the sand grains is sufficient to enable the surface layer to become heated in the sun, so that the air in which the leaves of the winter annuals are situated may be as much as 15° C. higher than that of the air generally, and thus here too assimilation is enabled to proceed under temperature conditions that elsewhere would inhibit the process (Salisbury, *Trans. Norfolk and Norwich Nat. Soc.*, XIII, pp. 336–55, 1933). The advantageous effect of hoeing or otherwise cultivating soil around plants in a period of drought is in no small measure due to the consequent maintenance of large pore spaces, which, whilst they hinder water loss, also materially diminish the heating up of the underlying soil. The following data, obtained during the drought of the past summer, giving the temperature gradients beneath adjacent cultivated and uncultivated surfaces show that the breaking up of the surface has, by checking the downward conduction of heat, rendered the cultivated soil hotter above, but cooler below.

TABLE VI

Depth.	Cultivated Surface.	Uncultivated Surface.	Average Difference. Cult. – Uncult.
cm.	° F	° F.	
1	115–125 Av 120	101–117 Av. 110	+ 10
3·5	105–110 Av. 107	92–106 Av. 100	+ 7
5	87–97 Av. 93	88–96 Av. 93	0
6·5	83·5–91 Av. 87	84·5–92 Av. 89	– 2
8·5	80–87·5 Av. 83	82–88 Av. 85	– 2

A further effect of organic material is its bearing upon soil reaction. Here two different influences must be recognised. Organic acids are formed during decay of the plant remains, so that the greater the quantity of these latter the higher the acidity is likely to be, and in general it is true that for a particular type of plant community the acidity increases with the organic content. Secondly, the early stages of decay are usually more acid than the later stages. Since the maximum organic content is, as we have seen, at the surface, and here is situated the least decomposed material, it is not surprising to find that the hydrogen ion concentration is normally greatest at the surface and decreases with increasing depth.

TABLE VII

VERTICAL DISTRIBUTION OF HYDROGEN ION CONCENTRATION EXPRESSED AS *p*H

Depth	Quercetum.	Betuletum.	Fagetum	Callunutem.	Vaccinetum.	Old Dune.
in 0–1 . .	4·9	4·7	6·3	4·8	3·9	6·8
3 . .	4·9	6 1	6	5	3 9	6·9
5 . .	5	6 1	6·4	5	4	6·9
9 . .	5·2	6 3	7·2	5·1	4·0	7
18 . .	—	—	—	5·2	5·1	—

Occasionally, where the subsoil is rich in bases, the actual surface is less acid than the subsurface, as in the Fagetum here cited, but even in Calcicole communities the carbonates have commonly been sufficiently leached from the surface layer not to permit of neutralisation of the organic acids produced.

The soil reaction is not merely important from its direct effects upon the plant growth and the widely diverse tolerance of different species for hydrogen ions, but also from its indirect action upon the solubilities of toxic substances, such as the salts of alumina, or of nutritive substances, such as the phosphates. In acid soils phosphates tend to occur in the form of phosphates of alumina and iron which are not nearly as readily available for plant nutrition as calcium phosphate formed during organic decomposition in more basic soils.

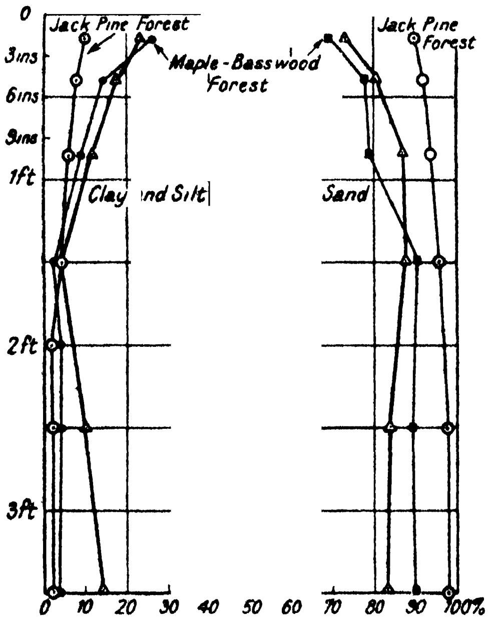


FIG. 5.—Gradient of Minimal Fractions. (*After data from Alway and McMiller.*)

As regards the mineral particles, the vertical distribution in natural soils is in general what might be anticipated from the fact that weathering is most severe at the surface. That is to say, the larger-sized particles tend to increase in amount with depth, whereas the clay fraction tends to be at a maximum near the surface (*cf.* for example Fig. 5 from data by Alway and McMiller, *Soil Science*, XXXVI, 281, 1933) despite the downwash to which the finest particles are subject. In so far, then, as the mineral particles affect the soil texture, their distribution tends to operate in the reverse direction to that of the organic matter.

We are now in a position to attempt to complete our three-dimensional picture, and to envisage the effect of the soil structure we have been considering upon the growth of the plant. As the root grows downwards it normally passes to conditions of increasing soil density, thus expending more and more energy on penetration. The partial pressure of oxygen decreases and the partial pressure of carbon dioxide increases, so that root respiration is rendered progressively more difficult. On the other hand, the degree of leaching decreases as the root descends, so that usually there is a rise in the amount of food material for a short distance with increasing depth. Also the acidity which may be very high at the surface tends to diminish. As regards the water supply, the gradient may be in either direction, according to the local topography and season, but the fluctuations in this respect are less marked at the lower levels.

It is evident, then, that the root system of the plant must effect a compromise between the contending advantages and disadvantages. Some plants, by the production of a root system, differentiated into superficial feeding roots and deep anchoring roots, may be said to make the best of both worlds, but for the majority the level of maximum development is determined by the special idiosyncrasies of the species. Willow roots are, for example, remarkably tolerant of almost anaerobic conditions, and in consequence are more or less indifferent to high concentrations of carbon dioxide. Even more striking are the differences with respect to the toleration of acidity. It is in general true that amongst woodland species the surface rooting types, such as *Oxalis acetosella*, are tolerant of high acidity, whilst deep rooted species, such as *Arum maculatum*, are less tolerant, but by their occupation of different soil horizons they can develop in the same area without serious detriment either from unsuitable soil reaction or from too severe mutual competition. So also in respect to penetration the species with slender rhizomes are superficial, whilst those occupying the lower and denser levels have rhizomes that are robust (*cf.* Salisbury, *Festschrift K. Schröter*, pp. 334–54, Zürich, 1925).

These few examples are sufficient to indicate that the architecture of the soil with which we have dealt has a very real significance in determining the character of the vegetation which it bears, whilst this in turn plays no small part in modifying the structure of the soil in which it is rooted. We recognise the complexity of the interactions which determine the suitability of a particular level in the soil for the root development of any one species in this system of living and non-living matter that, by the continuing action of temperature and precipitation and still more by the living occupants, both large and small, is subject to a fourth dimension, the changes wrought by the passage of time that constitutes the edaphic succession.

The colonisation of a bare soil surface by higher plants is not an event but a process which concerns changes in the organisation of the soil no less than in the vegetation which it bears. This process affects alike the water content, the soil population, the soil aeration, the penetrability of the soil, the soil reaction and the composition of the soil solution, though these in varying degrees. Hence the nature of the first species to become established on any area may be as much determined by tolerance for the initial conditions as by the facilities for their dispersal, whilst their continued presence will in great measure depend on the differential effects of the soil structure upon the germination and establishment of succeeding generations of the pioneers and their successors or supplanters.

ELECTRON OPTICS

BY L. C. MARTIN, D.I.C., A.R.C.S., D.Sc.

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IT has long been known that the path of a cathode-ray beam can be deflected by suitably disposed electric or magnetic fields, but it is only comparatively recently that it has been shown that a stream of electrons derived from a very small emitting source can be re-directed by such fields through a correspondingly small focus. We have, in fact, a close analogy with ordinary optics ; the radiation can be regarded in the first instance as of a corpuscular nature although the wave-directed characteristics of the corpuscles have ultimately to be considered. The pioneer theoretical work is due to H. Busch [1], but its implications were not fully realised until a year or two ago. Since then, the subject has developed rapidly, and electron optics has important applications in cathode-ray oscillographs and electron diffraction apparatus ; in addition, electron microscopes have been constructed which yield fairly sharp images under a magnification of more than ten thousand—an impossibility for an ordinary microscope owing to the finite wavelength of visible light. Objects suitable for study have to be mounted in a vacuum, and they are usually traversed by a beam of electrons just as light passes through the object in optical microscopy.

If an electron, starting from rest, moves over an unobstructed path element in which the electric potential increases positively by an amount V, the work done is eV where e is the numerical value of the electronic charge. This is manifest in the kinetic energy. (We shall only consider velocities small in comparison with that of light.)

$$\tfrac{1}{2}mv^2 = eV,$$

where m is the mass. The velocity is therefore

$$v = \sqrt{2eV/m}.$$

It is usual to indicate the energy in terms of the potential difference, expressed in volts, necessary to produce it. Thus we speak of " 1,000-volt " electrons, and so on.

Electrons are produced copiously from the cathodes of discharge tubes; in high tension tubes it is considered that their release is due to the fall of the positive ions of the residual gas into the cathode, but in thermionic emission free internal electrons acquire sufficient energy to pass the potential barrier of the surface; the discharge in tubes containing hot cathodes can be maintained with comparatively small potentials, even when all traces of residual gas have been removed. In either case the initial energy of emergence is thought not to exceed one or two volts; emergence may occur at any angle with the surface.

Let the field near a cathode surface be uniform, and the direction of the force be perpendicular to the surface over a distance *l* (Fig. 1).

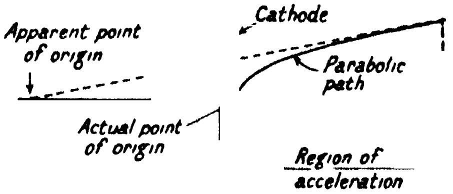

FIG. 1.—Path of Particle with grazing Emergence.

Consider an electron emerging, in a direction grazing the surface, with an energy V_t volts. It experiences a uniform acceleration perpendicular to the surface, and takes a parabolic path. Let the total increase of potential up to the distance *l* from the surface be V_n, then, since the transverse velocity does not change, the path will acquire an angle of $\sqrt{V_t/V_n}$ with the normal. A simple calculation will show that the apparent point of origin of such electrons will be at a distance *l* behind the cathode. In a 500-volt tube the maximum angle of such an electron from the normal would be about $\sqrt{2/500}$ radians, or 3¼°, but the majority of electrons would possess divergencies much smaller even than this. In a cold cathode tube the particles experience the major part of the potential change

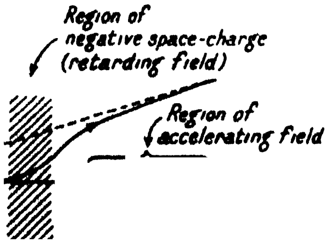
Region of negative space-charge (retarding field)
Region of accelerating field

FIG. 2.—Path of Particle with negative Space-Charge near Cathode.

in the close neighbourhood of the cathode; they therefore appear to radiate largely in straight lines normal to the cathode surface. On the other hand, with a hot cathode, the thermionic emission may become so intense that there is an accumulation of charged particles (a "space-charge") near the surface (Fig. 2) since the velocities near the surface are small. The effect is to produce a retarding field in this region which will affect the paths of the electrons in the sense suggested by the figure. Such effects have to be borne in mind when various technical arrangements are considered.

Refraction of Electron Ray by Potential Layer

Let an electron with energy V pass through a short region of path in which an increase of potential dV is encountered. The energy can be taken as constant inside the equipotential enclosures suggested in Fig. 3; these enclosures have very close plane wire gauze "nets" for their adjacent faces and they are at potentials V and $V + dV$ respectively. If these potentials represent the whole energy of the electrons traversing the enclosures—a close approximation in practice—the velocities before and after traversing the potential layer are v and v' respectively where $v = k\sqrt{V}$ and $v' = k\sqrt{V + dV}$. But since the velocity components parallel to the layer must be identical we have

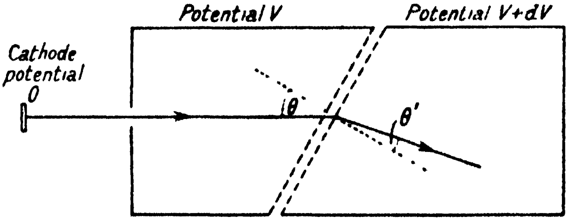

Fig 3.

$$v \sin \theta = v' \sin \theta',$$

where θ and θ' are the angles of incidence and refraction respectively. Thus

$$\sqrt{V} \sin \theta = \sqrt{V + dV} \sin \theta'.$$

It is clear that we can treat $\sqrt{V}$ (or a constant multiple of $\sqrt{V}$) as a refractive index for a space of potential V in analogy with the familiar optical equation (using n and n' for refractive indices)

$$n \sin \theta = n' \sin \theta'.$$

ELECTROSTATIC LENSES

Consider now two such enclosures separating a segment of a spherical potential layer of radius r (Fig. 4). Exactly as in the case of optical refraction at a single curved surface we can find conjugate points on an axial line containing the centre of the surfaces such that the distances l and l' of object and image respectively, assuming paraxial conditions, are given by the equation [1]

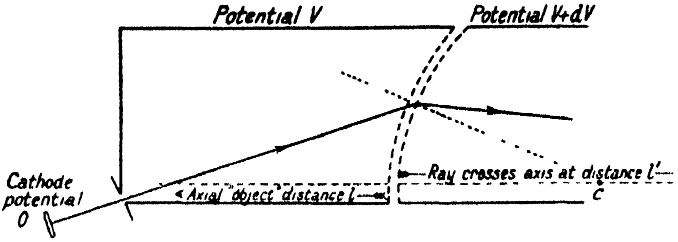

FIG. 4.

$$\frac{\sqrt{V + dV}}{l'} \qquad \frac{\sqrt{V}}{l} \qquad \sqrt{V + dV} - \sqrt{V}$$

If we make dV small in comparison with V, and make l' infinite, we find, to a first approximation, $l = -2rV/dV$; and this must be the "first focal length." If we add a symmetrically disposed layer we can clearly have both conjugates numerically equal at a value equal to twice the focal length, f', of the equi-curved "lens." Hence $f' = rV/dV$ for this case, approximately.

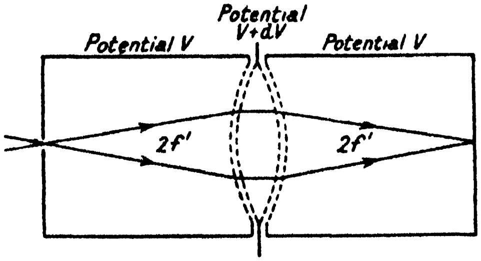

FIG. 5.

As in optics, the establishment of the conjugate relation for a single surface for all paraxial rays, no matter what their exact path in the paraxial region, has far-reaching significance, since we conclude that the imaging properties must hold for axially symmetrical systems generally, and for points close to the axis as well as on the axis. The magnification will be given by the usual Smith-Helmholtz relation; in this case

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[1] The sign convention used is of the ordinary Cartesian character.

$$\sqrt{V}h\alpha = \sqrt{V'}h'\alpha',$$

where V and V′ are the initial and final potentials, α and α' are the angles between the axis and the initial and final directions of a ray, and h and h' are the linear magnitudes measured perpendicular to the axis of object and image respectively. The conception of an "object" point is, of course, one from which electrons radiate.

Networks of these kinds have been used in practice, but the quality of the images, for reasons which are obvious, is poor. Better effects are produced by the use of axially symmetrical electric fields given by charged diaphragms or cylinders, and of which the action may be explained in terms of the curvature of the equipotential surfaces which exist around them. On account of the axial symmetry of the field the forms of the potential layers near the axis may be represented to a first approximation by segments of spheres, and closer consideration confirms that paraxial imagery may be expected. To follow the effect of the surfaces, it must be remembered that the acceleration of the electron at any point is in the direction of increasing potential, but that any deviation produced by a given change of potential will diminish with increasing velocity. Take, for instance, the symmetrically disposed lens illustrated in Fig. 6A. On entry, the comparatively slow electrons suffer considerable acceleration toward the centre, but owing to their rapid increase in velocity they are little deviated by the marked obliquity of the equipotential lines near the centre.

The following formula connects the conjugate distances l and l' of a "thin" electrostatic lens; it is a generalisation of one given by Johannson and Scherzer [2].

$$\frac{\sqrt{V'}}{l'} - \frac{\sqrt{V}}{l} = \frac{1}{8}\int_{-\infty}^{+\infty} V_z^{-3/2}(\partial V_z/\partial z)^2 dz,$$

where V and V′ are the steady initial and final energy potentials of the electrons respectively, and V_z is the energy of the electron at the point z. The potentials may be expressed in volts. The integration limits simply represent an integration through the whole effective range of the field which, however, is supposed to be so limited in extent that the radial distance of an electron does not suffer more than a negligible change in traversing it; this is not, of course, inconsistent with a big change of *direction*. In applying the conjugate distance relation, the object and image distances are assumed large as compared with the range of the "lens."

In cases where the initial and final potentials are different, the systems have been called, in analogy with optics, "immersion" systems. Figs. 6B and 6D show arrangements described by Zworykin [3] and by Brüche [4] respectively. In each case the original positive or collective action is followed by a "negative lens" effect. The equipotential lines sketched approximately in the figures are characteristic of a perfect vacuum free from discharge currents between the electrodes. The form of the above formula shows that a lens of the above kind of limited range cannot have a negative power as a whole; its action must always be collective.

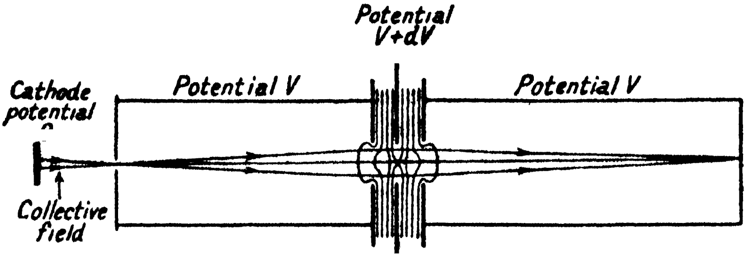

FIG. 6A.—"Single" Lens.

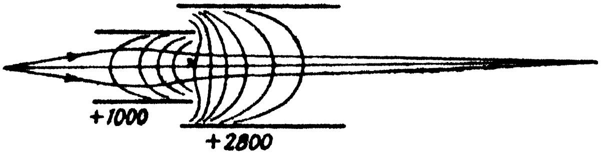

FIG. 6B.—"Immersion" Lens.

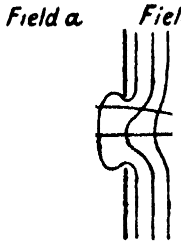

FIG. 6C.—Diaphragm with circular Aperture separating regions of different Electric Force.

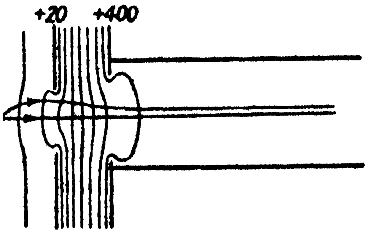

FIG. 6D.—Immersion Lens.

If a circular diaphragm in a sheet of conducting material separates two axially symmetrical fields of different strength, an electron passing the aperture suffers a deviation (in the region of changing field strength), which can be described as a lens action of which the focal length is given by

$$\frac{1}{f'} = -\frac{E_b - E_a}{4V},$$

where E_b and E_a are the axial field strengths (say in volts per cm.), and V is the potential in volts representing the energy of the electrons in passing. If therefore E_b is a positive field strength (accelerating a positive charge in the positive direction) and E_a a negative field, or a positive field of smaller magnitude, the effect of the aperture alone will be that of a negative lens; the formula takes no account of the fact that the paths of the electron, before and after passing the aperture, will in general be curved. It is not a complete formula as it stands.

Focusing by Space-Charge

We have only room to mention here that sometimes, as in oscillograph tubes, a certain amount of residual gas is present; this becomes ionised along a ray track. The negative ions are generally electrons of small mass and they quickly diffuse away, but the positive ions are atoms (which have lost one or more electrons) and are comparatively heavy and slow-moving. The result is that there is formed a "space-charge" with more or less cylindrical symmetry giving rise to a radial electrostatic force which tends to give an electron an acceleration towards the axis. Electrons from a given point source, therefore, tend to focus along the direction of the mean ray track from that point, and the image formation is erect and of unit magnification if the mean ray tracks from an assemblage of points are parallel.

Focusing can also be effected by space-charges produced by auxiliary means, but in general the image formation is extremely dependent on the nature and pressure of the residual gas, and the energy of the electrons. It is less amenable to control than the other devices, and may interfere to an uncertain degree with the precision of their action unless the vacuum is made as perfect as possible.

Focusing by Magnetic Fields

It is a commonplace in books on physics that a charged particle moving in a direction perpendicular to the lines of force of a magnetic field takes a circular path of radius $r = vm/He$ where v is the velocity, m the mass, H is the strength of the magnetic field and e the magnitude of the charge. If the particle has also a velocity component in the direction of the field it takes a spiral path (Fig. 7), crossing the same line of force at equidistant intervals. Busch used this phenomenon to obtain a measurement of the e/m ratio, working out the theory [1] as a particular case of the general problem of the effect of axially symmetrical electro-magnetic fields. He dealt also with the case of a magnetic field of limited range of action, and showed that it has properties analogous to those of a lens. As in the case of an electrostatic "thin" lens of limited range, the conjugate distance relation

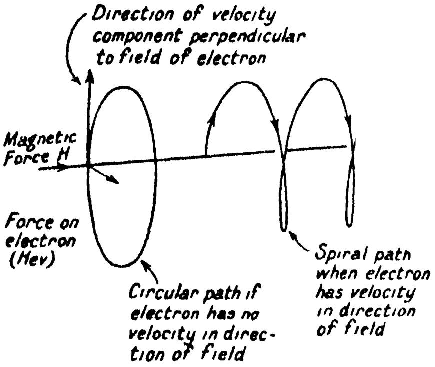
Direction of velocity component perpendicular to field of electron
Magnetic Force H
Force on electron (Hev)
Circular path if electron has no velocity in direction of field
Spiral path when electron has velocity in direction of field

FIG. 7.

$$\frac{1}{l'} - \frac{1}{l} = \frac{1}{f'},$$

holds, if the initial and final energy potentials are the same, where

$$\frac{1}{f'} = \frac{\eta}{8V}\int_{-\infty}^{+\infty} H_z^2 dz,$$

η being the ratio e/m, V is the energy of the electrons passing the coil and H_z is the strength of the magnetic field at the point z. Electromagnetic units are now assumed for all quantities. In the case of a short solenoid, a first approximation is

$$f' = \frac{16Va}{3\eta\pi^2 n^2 J^2}\left\{1 - \frac{3}{8}\left(\frac{l}{a}\right)^2\right\},$$

where $2l$ is the length of the coil and a its radius, J is the current strength and n is the number of turns per centimetre. The above formulæ are only reasonably accurate if the conjugate distances are long in comparison with the effective range of the coil's magnetic field. The formula shows that negative powers cannot be obtained. If both "thin" electrostatic and magnetic fields are present, the conjugate distance relation is

$$\frac{\sqrt{V'}}{l'} - \frac{\sqrt{V}}{l} = \frac{\sqrt{V'}}{f'},$$

where

$$\frac{1}{f'} = \frac{1}{8\sqrt{V'}}\left\{V_z^{-\frac{3}{2}}\left(\frac{\partial V_z}{\partial z}\right)^2 dz + \eta\int_{-\infty}^{+\infty} V_z^{-\frac{1}{2}} H_z^2 dz\right\}.$$

Very intense axially symmetrical fields can be obtained with coils of many turns, heavy currents, and special pole pieces.

ABERRATIONS

Those familiar with optical theory will readily recognise that the electrostatic lens systems discussed above will exhibit all the general types of aberration characteristic of lens systems when the images and rays considered are no longer of paraxial character. Theory shows that the same is true of magnetic systems, but there are then further complications.

The analogue of chromatic aberration lies in the difference of velocities of the electrons, but the effect is small in practice. Take the case of a tube using 1,000 volts. Electrons emerging from the cathode with energies varying between 0 and 2 volts will possess final energies between 1,000 and 1,002 volts, and the formulæ show that the focal length of a net lens, or coil, will have the same proportionate variation. The small apertures of the systems used at present make this chromatic aberration negligible in comparison with other difficulties. Inspection of the formulæ already given will show that both for simple electric and magnetic lenses the focal lengths are (at least to a first approximation) directly proportional to the potentials representing the energies of the electrons, and this removes the possibility of effecting achromatism by combinations of such simple lenses. It has been claimed [4], however, that a single diaphragm can function as a positive or negative achromatic lens; this is justified by a closer consideration of the theory, which calls for a modification of the approximate formula given above for the "single diaphragm" case. The conditions for achromatism would seem, however, to be of little value in practice. Spherical aberration affects the paths of "rays" at finite angles with the axis, but in practice the angular aperture may be restricted to a degree which would not be possible with light while still retaining sharp images; a magnetic "microscope lens" may work at a numerical aperture of 1/50. The considerations given in the early part of this article show that the extreme angular divergence of the rays in a discharge tube may be very small unless special means are adopted to increase it. With improving technique, no doubt spherical aberration will have to be studied and removed if anything approaching the ultimate possibilities of the method is to be realised.

Coma, astigmatism, curvature of field, and distortion will be encountered both in electric and magnetic lenses, but magnetic focusing fields produce rotations of the images in addition. Glaser [6] has shown that the paraxial image formation can be described in terms of Gaussian optics, with its principal planes, focal points and the like, but a paraxial ray in a plane containing the axis will, after refraction, lie in a new plane also containing the axis but rotated through a certain angle ϕ with respect to the first where

$$\varphi = -\frac{1}{2\sqrt{2}}\sqrt{\eta}\int_{-\infty}^{+\infty}\frac{H_z}{\sqrt{V_z}}dz.$$

The rotation is suggested in Fig. 8, and it may be regarded as constant for paraxial rays, but the more exact analysis shows that the value of the turn depends on the slope of a ray with respect to the axis when this becomes finite. For a finite-sized aperture this must result in a loss of definition ; rays at different finite angles do not meet in the same point. Glaser calls this action "*bildzerdrehung*" or "image twisting." We shall have, however, to deal also with an action analogous to distortion in optics in which we find, not only the loss of definition of the image due to coma, etc., but also a displacement of its parts when the aperture is indefinitely small. If English terms may be suggested we might call the differential rotation of the various parts of the image (which would be seen with an indefinitely small aperture) "image twisting." The rotation aberration causing a loss of definition when the aperture is increased could be called "image wringing" since this word is occasionally employed to signify injury through twisting.

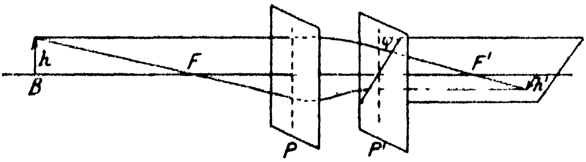

FIG. 8.

APPLICATIONS AND POSSIBILITIES

While considerations of electron optics have had important applications in the design of cathode ray oscillographs [4], the most attractive possibilities appear in microscopy. Very high magnifications can be given by the use of successive "lenses." Fig. 9 shows an electron microscope of the type used by Knoll and Ruska [7], in which a cold cathode discharge (10 to 100 kv. may be employed) acts as the source of radiation. Successive magnetic coils, ironclad to concentrate the axial field, act as condenser, objective, and projection lens ; these project the image on to a fluorescent screen, where it can be observed or photographed. The objects studied should conduct electricity (that they may not accumulate a charge) and be capable of withstanding the discharge; they must be mounted so that heat and electricity can be conducted away. So far, Ruska and his colleagues have only studied simple objects like metal foils showing small apertures, also carbonised cotton fibres and the like, but it will probably be possible to study biological preparations after suitable " staining " and mounting methods have been developed.

The ultimately possible resolving power is presumably only limited by the aperture of the system and the de Broglio wavelength, $\lambda = 10^{-8}\sqrt{150/V}$, where V is the electron energy in volts. If the ordinary resolving power formula for the microscope, $d = 0{\cdot}5\lambda/A$, where A is the numerical aperture, is applied to this case, we find that the limit of resolution in the object will be $1{\cdot}6 \times 10^{-4}\mu$, if and when an aperture of 0·1 can be used with 1,500-volt electrons. So far it has not been found practicable to use apertures exceeding 0·02, but the definition appears to allow a useful magnification of 8,000 or more [8], and the resolution already attained is of the order of $0{\cdot}05\mu$, whereas the best that can be done with ultra-violet microscopy is about $0{\cdot}1\mu$.

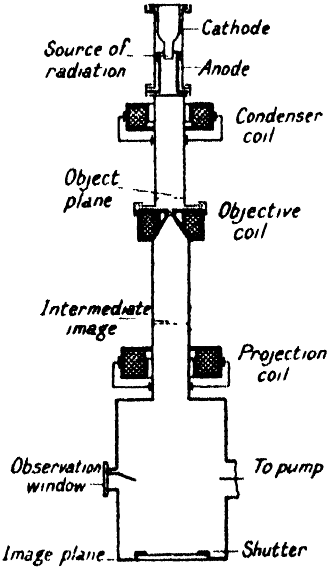

FIG. 9.

Much remains to be done in the study of the aberrations and in development of the vacuum mountings and vacuum photography.

At the Imperial College we have developed a type of apparatus in which optically flat joints are used in place of the rubber gasket joints used by Ruska; we have also now produced and used a camera which allows of the introduction and withdrawal of a photographic plate into the vacuum without the admission of air (which may spoil a " hot " cathode). Although as Ruska has shown, very high magnifications can be obtained, they will be of little use till perfection of the vacuum technique and a perfect control of the whole of the conditions can be ensured. If it should prove possible to correct the aberrations over an aperture of about 0·1 the Abbe principle indicates that we may even begin to study the space-lattice structure of crystals by direct images, since the electron diffraction rings in many cases are included in apertures of this order.

In bacteriology there is ground for hope that even filter-passer organisms, hardly amenable to study by ordinary optical methods, may ultimately be " brought to book," since the limits of resolution set by the finite wave-length of ordinary light can now be surpassed. Since the writing of this article a comprehensive treatise [9] by Brüche and Scherzer on the whole subject has appeared, and reference to this book may be made for fuller details. The author is indebted to Herr Ruska and to Herr Julius Springer of Berlin for permission to reproduce Fig. 9. I am grateful also to my colleagues Dr. H. J. Starks and Mr. D. H. Parnum for their assistance in preparing this article.

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THE PREVENTION OF MINE EXPLOSIONS

With special reference to the work of the Safety in Mines Research Board.

By J. W. WHITAKER, Ph.D., F.I.C., M.I.Min.E.

The explosion at Gresford Colliery, near Wrexham, in September last, in which 265 lives were lost is another tragic reminder that safety from such disasters is by no means yet achieved and that continued research, education, and supervision are necessary. Unfortunately calamities of this kind are sometimes used by the unscrupulous for propaganda to the effect that the scientific work in which the dangers of firedamp and coal-dust (and the causes of mine accidents in general) are being investigated is futile or incompetent. Before reviewing the work done, therefore, attention must be called to the fact that the scientist's sphere is bounded by and ceases with the recommendations he makes: the matter then passes to the hands of the legislator, and if it goes further the final carrying out of the instructions is in still other hands. When accidents happen it is by no means certain or even likely that the scientist is at fault. Occasionally a new set of conditions arises which might have been foreseen, but the probable cause is " human nature "—*i.e.* apathy, thoughtlessness, fatigue or foolhardiness on the part of those whose duty it is to carry out the various instructions given.

To deal with " human nature " is not within the scope of the scientist as ordinarily understood: it is the sphere of the educationist and of the psychologist. This development is already on the way.

Firedamp and Mine Lamps

At the beginning of the present century the study of firedamp was little further advanced than where Davy had left it seventy years earlier. Davy had shown that firedamp was essentially methane, and that compared with hydrogen and other common inflammable gases it was difficult to ignite. He gave its approximate limits of inflammability in air, and thus fixed its explosive range. " When mixed with between 5 and 6 times its volume of air it exploded feebly. It exploded with most energy when mixed with 7 or 8 times its volume of air, and mixtures of firedamp and air retained their explosive properties when the proportions were 1 of gas to 14 of air (*i.e.* 6·7 per cent.). One of carbonic acid added to 7, or 1 of nitrogen added to 6 parts of the explosive mixture rendered them inexplosive." (*Collected Works*, VI, 10, 1840.) Davy notes that red-hot iron (or glowing charcoal) will not ignite an explosive mixture of methane and air blown over it; also that "the gas in any proportion with common air will not explode in a small tube the diameter of which is less than $\frac{1}{8}$ inch or even in a larger tube if there is a mechanical force urging the gas through the tube."

Most of the explosions in mines were due at that time to ignition of firedamp from the flames of the lamps or candles used, and Davy in designing his earliest lamps relied on the following principles, "First, a certain mixture of azote (N) and carbonic acid (CO_2) prevents the explosion of the firedamp, and this mixture is necessarily formed in the safe lantern. Secondly, the firedamp will not explode in tubes or feeders of a certain small diameter. The ingress into and egress of air from my lantern is through such tubes or feeders; and therefore when an explosion is *artificially* made in the safe lantern, it does not communicate to the external air." His first lamps were designed in the above way with small inlet and outlet tubes. Later he used concentric tubes for the inlets, and finally the well-known wire gauze cage through which both the feed air and the products of combustion had to pass.

Davy further showed that firedamp was non-poisonous, that one could breathe a mixture of 1 part of the gas with 3 of air, and went so far as to suggest the use of a coil of platinum wire inside the gauze in order that emergency work might be carried on in explosive atmospheres, by the burning of the gas itself and the light emitted from the incandescent platinum! This procedure has not commended itself to mining men—which is to be expected in view of the fact that the risks are high enough when precautions are taken and the atmosphere normally contains only traces of methane.

Davy's simple gauze lamp—there was generally one gauze with another fitting on the top as a cap—was proved unsafe in explosive mixtures of firedamp and air moving with a velocity greater than a few feet per second. The safety factor, however, was improved considerably by placing the lamp in a container (to protect it from draughts) with a glass window. Later a cylindrical glass was fitted to the lamp, the gauze was cut away round the flame, two complete gauzes were provided, both fitting directly on the lamp glass, and finally a bonnet or shield was added to protect the gauzes from air currents and mechanical damage.

In some lamps an internal chimney was also provided—*e.g.* the Mueseler, and various combustion-tube lamps. Internal chimneys of this kind increase the candle power and, if the chimney is carefully designed, improve the safety factor of the lamp.

The most dangerous conditions for a flame lamp, assuming that it is mechanically sound and properly assembled, are those when the flame is very low—and as the fireman or deputy tests for gas with a low flame, it is important to understand the conditions.[1] The lamp may fail in two ways (1) by passing the flame of an internal explosion, or (2) by passing the flame when gas has been burning continuously in the gauze. An explosion inside the lamp is most violent when there is about 9 or 10 per cent. of gas present, and the lamp is full of this mixture. If the flame is high, the products of combustion (present in large quantity) are useful, not only in excluding the explosive mixture of gas and air, but also in tending to extinguish any inflammation. Assuming, however, that the mixture is ignited with a low flame, the inflammation travels into the gauze with increasing velocity; and the taller the gauze the less is its ability to arrest the flame. Incidentally the manner in which flame is arrested by a gauze or small tubes is a far more complicated problem than originally assumed.

In the second place gas ignited inside the gauze may burn there until the gauze is hot enough to give external ignition. To obtain the most dangerous conditions in this respect, one requires a rapid supply of a 9·5 per cent. methane mixture, but the period of contact of the mixture outside the gauze should be less than the "lag on ignition." The worst conditions will exist when such a flow is suddenly stopped, and safety lamps have to pass a test of this kind before approval.

The provision of a bonnet or shield for the gauzes reduces the rate of supply of the (explosive) mixture to the lamp, and thus prevents the rapid generation of heat necessary to cause the gauzes to pass flame. In addition the bonnet also acts as a protection against damage by a blow.

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[1] The fireman uses a low flame (about $\frac{1}{10}$ inch high) in order to make it practically non-luminous. Under these conditions he is able to see the methane burning on the top of the flame as a pale-blue cone whose size varies with the percentage of gas present. With experience it is possible to say whether this percentage is 1, 2, 3, or 4. The lower limit of inflammation is about 5½ per cent. Men are withdrawn from any place if 2½ per cent. is present in the main body of the air at that place.

Davy's lamp was a small one, little over half the size of some modern flame lamps—and in general small lamps are safer than large ones, even if the latter have the air feed restricted. The glass is the really weak spot in the modern flame lamp—although it is well protected by pillars and is of stout construction. With regard to the candle power of the lamp, it may be said here that modern researches by Wheeler and Woodhead have shown that a coarser mesh of gauze than that used by Davy is safe, and results in a much improved light. Their investigations have also shown (1927) that slightly taller gauzes of Davy's mesh (28 apertures to the linear inch) give still better results, and are equally safe. Duplicate gauzes of this kind or of the coarser mesh (20 apertures to the inch) are now extensively used. The candlepowers obtained with these lamps are about twice those given by the older types—*i.e.* prior to 1927. Much of this work was the outcome of the investigations of the Miners' Lamps Committee (1920–25).

VENTILATION REQUIREMENTS OF THE COAL MINES ACT, 1911

The development of the miner's lamp in the nineteenth century was rather haphazard. Various commissions were appointed to consider the question of lamp safety and design, and of the other causes of mine explosions—but little systematic scientific research was done. However, the fact that the lower limit of inflammation was attained when between 5 and 6 per cent. of "gas" was present soon became well appreciated, and the Coal Mines Act 1911 embodied the following regulations: (1) Men must be withdrawn from any place in which the percentage of inflammable gas in the main body of the air exceeds 2½, and the place must be fenced off in all means of ingress. (2) In naked-light mines, safety lamps must be substituted for naked lights if the percentage of inflammable gas in any place rises to 1¼, or the men must be temporarily withdrawn. (3) Safety lamps must be used instead of naked lights in any seam where the return air from any ventilating district contains normally more than ½ per cent. of firedamp. (4) All electric machinery must be switched off if there is present more than 1¼ per cent. of firedamp in the air.

These figures seemed to allow a fair margin of safety—especially as the safety lamps were supposedly safe. But the evolution of "gas" in the mine is by no means steady, and absolutely continuous testing for gas at all working places is impracticable. Naked-light mines may seem to be treated rather generously, but in such mines gas is exceptionally rare and sometimes unknown.

SYSTEMATIC INVESTIGATION OF EXPLOSIONS

In spite of the above and earlier regulations on the proportion of gas allowable, and in spite of the fact that many mines were almost free from gas, large explosions continued to occur. The reports of the Royal Commissions were indefinite, and it was not until the Mining Association experiments were started in 1908 at Altofts near Leeds, that systematic investigation was carried out. The experiments were done first under the direction of Sir Wm. Garforth. In 1911 the experimental gallery, 750 ft. long and made of boiler shells 7 ft. 6 in. in diameter, was transferred to Eskmeals and the work taken over by the Home Office, Dr. R. V. Wheeler acting as chief chemist and later as director. The Eskmeals experiments were continued from 1911 to 1924. The Safety in Mines Research Board took over the work in 1921, and in 1924 recommended that a new and smaller diameter gallery should be constructed and the research station moved to Buxton. The experiments have been continued there since 1926 in the new gallery which is 4 ft. in diameter and 1,000 ft. long.

These experiments have proved beyond question that *coal-dust* clouds in air are violently explosive,[1] and that coal-dust is particularly dangerous in its ability to spread explosions throughout the mine. In other words, while coal-dust is not particularly easy to ignite, yet it is highly dangerous if an ignition of dust (or gas) does occur, as the dust raised supplies the necessary medium for the propagation of the explosion. The experiments, however, were valuable not only for studying coal-dust explosions and their prevention, but also because they necessitated scientific staff and laboratory equipment for the study of all problems relating to mining accidents. Consequently, these experiments laid the foundations of systematic scientific research into mining accidents and their prevention. For some years the direction of the research has been in the hands of Dr. Wheeler, who is also Professor of Fuel Technology in the University of Sheffield. Much of the small scale work is carried out at the Sheffield laboratories.

IGNITION AND EXPLOSION OF FIREDAMP—LATER RESEARCHES

Wheeler gives the following limits of inflammability for firedamp-air mixtures—contained in a closed glass tube 2 metres long and 5 cm. diameter.

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[1] Lyell and Faraday in 1844, Galloway in 1870, Hall in 1876, and J. J. Atkinson and J. B. Atkinson in 1886 had all supported this view.

Direction of Propagation.	Methane, per cent.	
	Lower Limit.	Upper Limit.
Upward	5·4	14·8
Horizontal	5·4	14·3
Downward	6 0	13·4

If the dimensions of the tube are greater there is no marked difference; but if less, the values change, the lower limit rising and the upper falling, thus narrowing the explosive range; until with a tube 0·45 cm. diameter the only mixture which will ignite at all is one containing 9·95 per cent. of methane. The theoretical mixture for complete combustion in air is one having 9·5 per cent. of methane; and it is interesting to note that the mixture which best propagates flame is richer than this, yet that which is most easily ignited by a feeble spark or other means is weaker—generally a 7 or 8 per cent. mixture.

In the mine common sources of ignition of methane are the flames from explosives used in blasting and the heat (adiabatic compression) developed by the pressure, the flames of defective lamps and naked lights, the flames and red-hot material of fires (started by spontaneous combustion or other cause), the filaments of damaged electric lamps, electric sparks, and the sparks generated by friction of certain very hard rocks. Most rock sparks are harmless; indeed, it is generally where a hot spot develops in the rock that ignition takes place.

Wheeler and his co-workers have shown that the factors governing the ignition of methane-air mixtures are (1) the proportion of "gas" present; (2) the temperature of the igniting source; (3) the temperature of the mixture; (4) the rate of heat loss to the surroundings; (5) the pressure.

When a source of heat is introduced into an inflammable mixture of methane and air, the time interval before inflammation occurs is known as the "lag on ignition." During this period "preflame combustion" is taking place, and a small amount of heat is developed. Unless, however, the temperature rises beyond a certain value actual inflammation will not occur. With a flame as the igniting source the lag period is small, generally less than 100 milliseconds; but if a source of heat of less temperature be employed the lag may extend to several seconds; and if the gases are in motion, thus helping in the dissipation of the heat, the lag period is still greater. Indeed, actual ignition is not easy to produce in a current of methane and air, unless the temperature of the igniting source be well above that of red-hot iron or charcoal—as Davy claimed. It is also found that if the mixture be heated (to 700°–800° C.) inside a quartz vessel (quartz, to reduce catalytic effects of hot surfaces) it is inflammable if only 4 per cent. is present; and the hotter the vessel, the shorter the lag. H. B. Dixon has given the ignition range of methane in air as 680° C.–740° C., an exact value being unattainable owing to surface effects. The above range refers to ordinary pressure conditions, the methane and the air being heated separately before mixing.

Dixon has also found the "ignition point" using adiabatic compression in a cylinder fitted with a piston. The piston is driven in by a falling weight to a certain (variable) distance; the piston is then arrested and held by catches, and observation made. It is found that the most sensitive mixture contains about 7·5 per cent. of methane, and that it is fired when the original volume is compressed to 8·7 per cent. of its value, the pressure then being 28 atmospheres and the calculated temperature 428° C. In other words at this stage the mixture had attained a self-heating point hot enough to result eventually in inflammation. The practical results of these experiments are that the shock waves from explosives in blasting may ignite methane-air mixtures present near the shot hole or in crevices communicating with it.

ELECTRICAL APPARATUS AND IGNITION OF FIREDAMP

With regard to ignition by electric sparks it has been found that the mixture most easily ignited contains about 8·3 per cent. of methane, no matter what the nature of the electric spark may be. Wheeler in *The Electric Ignition of Firedamp* (Paper No. 20 of the Safety in Mines Research Board) distinguishes between Capacity Sparks and Inductance Sparks, the former being "jump" sparks such as those obtained from a high-tension magneto exploder. The incendivity of these magneto sparks is high. The spark when analysed by photographic means is seen to consist of a bright flash lasting only for a fraction of a millisecond followed by a series of smaller flashes which eventually become a band of light. The first flash is called the "capacity component," the succeeding phenomena the "inductance component." Inductance sparks, such as those obtained when the current in an inductive circuit is interrupted by breaking the circuit, are of less incendivity, the less the inductance of the circuit, and the less rapidly the contacts are separated. The nature and volatility of the metals at the contacts are also of importance—with capacity or inductance sparks—the more volatile the metal, the longer is the arc, and the more likelihood is there of ignition of the gas. Capacity sparks have greater incendivity than inductance sparks because, according to Wheeler, they communicate their energy the more rapidly to the mixture of gas and air. Inductance sparks are produced in bell signalling in the mine, not only at the trembler contacts, but also at the wires when the circuit is broken.

The Senghenydd Colliery explosion in 1913 in which 439 lives were lost, and the Garswood Hall Colliery explosion as recently as 1933 in which 27 lives were lost, have been attributed to sparking on the signal bell circuit in use at the time. The General Regulations of the Coal Mines Act require that all electrical apparatus must be so constructed, installed and maintained that there is no danger, and the usual way of guaranteeing this is to instal only apparatus that has been certified by the Mines Department.

Safety Pamphlet No. 8 issued by the Department in 1933 explains the conditions which make for safety in signalling circuits, and also the grounds upon which apparatus for this work is now approved or rejected. The spark-suppressing devices may consist of insulated closed circuits, wound along with the ordinary operating winding of the bell or relay; or a copper sleeve may be fitted over the bobbin. Either of these devices functions as a short-circuited winding of the secondary coil of a transformer and thus absorbs the energy latent in the primary when the circuit is broken. Non-inductive resistances are also used as shunts across the operating windings, thus offering discharge paths for the current when the circuit is broken. The maximum signalling voltage allowed in coal mines is 25.

Precautions have also had to be taken with mining telephones of the magneto ringing type as a result of investigations at the Mines Department Testing Station at Sheffield.

ELECTRIC LAMPS

Ignition of firedamp by the filaments of damaged electric lamps is an obvious source of danger in the mine, and has been studied by Wheeler and Shepherd, and by Allsop and Thomas in Papers 36 and 80 respectively. Wheeler and Shepherd have shown that the tungsten filament of a lamp which is ordinarily at 2,000° C. (± 200° C.) ignites a gas mixture by convected heat; but if the tungsten filament is at a much lower temperature (less than 1,200° C.), ignition of the methane-air mixture may not take place for several minutes, when it is eventually effected by the actual burning of the wire.

Miners' electric handlamps (*i.e.* lamps with their own accumulators) have their bulbs protected—as have ordinary fixed electric lamps—by an outer glass of strong construction; but even so they are by no means safe. Allsop and Thomas have investigated the possibilities of devising suitable protective cut-outs. They have found that if the bulb is broken and the filament exposed, when the current is greater than 2 amps, it must be switched off before the exposure of the filament if ignition is to be avoided. When the filament current is less than 1 amp., the permissible delay is between 0·02 and 0·12 second. This is small, but large enough to leave open the possibility of devising a cut-out. Gas-filled bulbs are less dangerous than vacuum bulbs; and filaments carrying lighter currents are less dangerous than those carrying heavier—less dangerous as measured by the time lag on ignition. The dangers of ignition of the hydrogen evolved from the accumulators have also been investigated by the research staff of the Board. These dangers are naturally greater where a switch is provided with the lamp.

FLAME-PROOF ELECTRICAL APPARATUS

General Regulation, 132, of the Coal Mines Act contains the following: "In any part of a mine in which inflammable gas, although not normally present, is likely to occur in quantity sufficient to be indicative of danger, the following additional requirements shall be observed:

(i) All cables, apparatus, signalling wires and signalling instruments shall be constructed, installed, protected, worked and maintained so that in the normal working thereof there shall be no risk of open sparking.

(ii) All motors shall be constructed so that when any part is live all rubbing contacts (such as commutators and slip-rings) are so arranged or enclosed as to prevent open sparking.

It is this second part which enforces flame-proof enclosures for electric motors, switches, and the like. A flame-proof enclosure is not a gas-proof enclosure (which would be practically impossible) but one in which either (*a*) the casing is strong enough to withstand any internal explosion or (*b*) the casing has vents which allow of release of pressure but not of the passage of flame.

The Safety in Mines Research Board have given careful attention to this matter—which is indeed of vital importance. Papers Nos. 10, 21, 35, 49 and 60, have dealt with various aspects of the problem and with different designs of apparatus, including Protection by Flange, Ring-relief, and Perforated-plate Devices.

The tendency, however, is to make the casings more robust—strong enough to withstand 100 lb. per sq. in. pressure, the maximum normally attainable—and not to rely upon narrow spaces (less than $\frac{1}{50}$ in.) between flanges or the fine openings in other devices for pressure relief. The experiments in these researches have also shown the importance of " Pressure Piling." Wheeler and Statham in the 1929 summarising report (Paper No. 60) state that " if two vessels coupled together, the ratio of the volumes being less than 10 : 1, are filled with an explosive mixture and ignition occurs in the larger, a considerable degree of turbulence will be produced within the smaller vessel owing to the explosion in the larger, and the flame projected into the smaller vessel will be of large size. The explosive mixture in the smaller vessel will therefore be turbulent and the source of ignition large. Under these conditions a sudden and rapid production of pressure takes place within the smaller vessel. If the vessel into which the flame of the explosion is travelling is large, and that in which the explosion begins is small, the turbulence produced in the larger vessel is inconsiderable, whilst the flame projected through the communicating passage is small in comparison with the volume of the vessel." The former case is clearly the more dangerous, and if the volume ratio is far more than 10 : 1 the pressures developed may be very great. " The causes of these high pressures, which are rapidly developed, are: (*a*) Compression of the mixture before ignition, (*b*) turbulence of the mixture, owing to the rapid in-rush of gases, ahead of the flame, from the larger casing, and (*c*) ignition by a large flame projected through the communicating passage." . . . " The results indicate that in the design of flame-proof enclosures for electrical apparatus, care must be exercised to avoid interconnected departments."

It has been demonstrated that coal-dust in the apparatus does not impair the safety, but it is recommended that such dust should be excluded as far as possible as it may be detrimental simply as foreign matter. Wheeler and Statham (loc. cit.) suggest that to attain this " with flange protection rough machined flanges or spring relief bolts probably offer the most convenient means. With the ring relief device, a hood extending down the outside of the rings prevents excessive entry of dust. With the perforated plate and the plate relief devices, a covering of lead foil is effective."

Attention has also been given to strength and shape of the casings, the danger of unbottomed holes, the flange width, clearance at shaft or spindle bearings, fittings at the cable entry, the design of safe plug-and-socket connections, and the interlocking of apparatus to prevent misuse, meddling or neglect. The recommendations of Paper No. 60 on these heads are worthy of most careful reading by all concerned with flame-proof casings.

The question of oil-immersed gear raises its own special problems. Gases are produced from the oil on breaking the circuit—the gases being mainly (70 per cent.) hydrogen with some 20 per cent. of acetylene and about 5 per cent. of ethylene and 5 per cent. of methane.

Mixtures of these gases with air are far more violently explosive than a methane-air mixture. For instance, the flames produced from a hydrogen-air explosion easily passes through flanged[1] joints 2 in. wide and $\frac{1}{100}$ in. apart. It is suggested that added robustness of casing is the soundest solution to the problem—unless venting devices designed specially for arresting such rapid flames are employed. The matter is at present under investigation.

The question of the projection of the flame of a firedamp explosion has been investigated by Burgess (Papers Nos. 27, 42 and 83), who has shown that a 1 : 9 firedamp-air mixture on explosion may expand sufficiently to be capable of projecting flame throughout a volume equal to six times the volume of the original mixture. Burgess has also shown that with certain arrangements of side galleries the maximum distance of flame projection is obtained when there is more than 10 per cent. of methane present, and this distance may be somewhat greater than the calculated value. The importance of these researches and conclusions is obvious to the mining man who has to combat gob-fires by building stoppings behind which a "gas" explosion may occur.

GOB-FIRES

Gob-fires are fires started by the oxidation of small coal and pyrites in the gob or goaf, *i.e.* in the area left after the coal has been extracted. This area is packed to varying degrees of thoroughness with stone, and subsidence helps eventually in the sealing process. Nevertheless, air leakage through the gob is quite common and sometimes difficult to avoid, and with certain coals oxidation and selfheating to ignition takes place. Pyrites help in the process in many seams. Underground fires may thus be started, and if the seam is "gassy," explosions are likely. In any case gob-fires are a source of great anxiety and danger.

Researches into the spontaneous combustion of coal have been carried out by a number of investigators including Richter, Fayol, Haldane, Winmill, Wheeler, Sinnatt, Graham, Drakeley, etc.; but perhaps the recommendations of E. O. Simcock (in *Fuel in Science and Practice*, 1925, **4**) have been of the most practical help in enabling the mine manager to deal with such fires, once they have started. The indications of gob-fires are well known to the mining men working in seams liable to spontaneous combustion: they include the particular smell of "gob stink," sweating of the strata (*i.e.* condensation of water vapour or steam from the fire), traces of H_2S, CO and CO_2 in the air, and a lowered oxygen content.

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[1] Flanged joints 1 in. wide and $\frac{3}{64}$ in. apart are just safe with methane-air mixtures.

The usual manner of treatment if the fire is small is to dig it out, fill into iron tubs, quench with sand, and send to the surface. If, however, digging out be impracticable, it is common to seal off the district with stoppings built of stone, sand and clay, and thus exclude the air. It is behind such stoppings that explosions may occur, especially just after sealing; and in any case the heated material may remain hot for very long periods—months and years—so that the reopening of a district is a dangerous procedure.

Simcock had observed in practice that the most dangerous fires to seal off were the small ones, especially when the sealed-off district was comparatively large, and the rate of sealing slow. He recommended, therefore, that temporary stoppings (made of unburnt bricks or the like with sand or soil, but made of timber in emergency) should be built as near as possible to the fire, and that the work should be done rapidly. No attempt should be made to subdue the fire *gradually*. In fact, the more vigorously it is burning, the more easily is it brought under control: the products of combustion create an extinctive atmosphere behind the stoppings and thus prevent an explosion of gas there.

Simcock's contentions led to the construction of the experimental gob-fire chamber at Buxton, and the researches carried out there have largely verified his experience in the mine. They have also led to a study of the alterations in the atmospheres behind such stoppings (Paper No. 75), and to a study of the revival of heatings by inleakage of air (Paper No. 76). Recently the chamber has been reconstructed, and further researches are in progress.

IGNITION BY EXPLOSIVES

The Safety in Mines Research Board has published several papers on explosives and the means of making them less dangerous. All explosives give flame, and although the tests which an explosive has to pass before being placed on the Permitted List are severe (untamped shots being fired in an inflammable air-gas mixture) safety is by no means yet attained. In "gassy" or in dry and dusty mines none but Permitted explosives may be used; and their manner of use, the procedure in cases of misfires, the testing for gas within 20 yds, the neutralising of coal-dust within 5 yds, and so forth are carefully laid down in the Explosives Order of the Coal Mines Act.

Permitted explosives have been developed in an effort to keep down the flame temperature. There are several types, but the commonest consist essentially of ammonium nitrate, T.N.T., and sodium chloride; or of ammonium nitrate, woodmeal soaked in a nitrated mixture of glycerine and ethylene glycol, and sodium chloride. The woodmeal is used as an absorbent, and the sodium chloride helps to cool the flame by volatilisation.

Among the more recent papers of the Board on the subject of explosives is one on Stemming Materials by Ritson and Stafford (Paper No. 84) in which it is shown that clay as commonly used is inefficient, that sand is highly efficient but of course difficult to insert into a horizontal shot hole, and that a 3 to 1 mixture of sand and clay with up to 5 per cent. of calcium chloride (to keep the material moist) makes a stemming material practically equal to sand alone. They also state that a reduction of as much as 33 per cent. in the cost of explosives has been indicated in their experiments by the use of this stemming, and that it helps in preventing blown-out shots and in reducing the quantity of fumes.

In Paper No. 85 on Simultaneous Shot Firing, it is claimed that this procedure (hitherto prohibited in gassy or dusty mines, except in stone drifts) far from being dangerous results in increased safety, greater efficiency of blasting, economy of effort, and saving of time. The one disadvantage is the possibility of an undetected misfire. Battery exploders are recommended in preference to the magneto type, because (1) they are safe, as the circuit is non-inductive; (2) they give the full current on the instant the circuit is completed, whereas with magnetos the output depends upon the rate of revolution. Low-tension detonators in series were employed in the experiments, and it was shown that grading of the bridge wire resistances closer than the commercial limits of 0·9 and 1·3 ohms. was unnecessary, simultaneous shot-firing (without misfires) being easily attainable when sufficient current was employed. Battery exploders are now being tested in which a galvanometer and testing circuit are incorporated, so that the shot-firer may be certain of the continuity of the detonator circuit before firing the shot.

Coal-Dust Explosions

Mention has already been made of the fact that clouds of coal-dust in air are explosive. In addition, the fumes and CO produced often mean death to those who may have escaped the first effects. Research has been undertaken at Altofts, Eskmeals, Buxton and Sheffield with a view to investigating the causes and if possible the means of prevention.

In the experiments in the large galleries—model roadways of a mine—ignition is generally effected by the firing of 20–24 oz. of gunpowder or blasting powder from a shot-hole stemmed with coal-dust. It is estimated that this source of ignition corresponds in intensity to that given by a firedamp explosion that has travelled 50 ft.—10 per cent. methane being present in the explosive air-gas mixture. The gallery is strewn, not very heavily, with coal-dust, and the violence developed is enormous.

Coal-dust is produced inevitably in the mining of coal, not only at the face, but also in transport (*i.e.* haulage and winding) and at the surface screens. The quantity may no doubt be reduced, but entire prevention is probably impossible. Moreover it has been shown at the U.S.A. Bureau of Mines station that as little as 0·032 oz. per cub. ft. will propagate an explosion ; and at the Liévin station in France propagation has been obtained with even less. A dense cloud, however, is generally necessary to initiate the explosion. The practical upshot of the work, however, is to show the futility of simply " cleaning up " as a method of prevention.

The theoretical quantity of coal-dust for complete combustion is 0·12 oz. per cub. ft. of air. At Eskmeals it has been found that the most violent explosions are given with about 0·35 oz. per cub. ft.—*i.e.* with 3 times the theoretical quantity. The coal used yielded about $\frac{1}{3}$ of volatiles (at 900° C.) ; and the question arises whether the volatiles are not the main factor. English bituminous coals yield as a rule between 30 and 40 per cent. of volatiles ; and in general dusts from these coals are more inflammable than dusts from coals with less volatiles, the inflammability being measured by the speed of flame or by the quantity of inert (stone) dust necessary to prevent explosion.

It is unfortunately impossible to enter into the details of these researches in the space of this article. Suffice it to say that the staff of the Safety in Mines Research station have investigated the influence of (1) the composition and rank of the coal ; (2) the fineness of the coal-dust ; (3) spontaneous electrification in dust clouds ; (4) the presence of small quantities of firedamp ; (5) as preventatives, stone dusts (their fineness, dispersability, composition and quantity required); and (6) the wetting of dusts and the use of wetting agents.

The earlier work led to the (1920) Regulations with legal requirements concerning watering or stone-dusting, and the routine testing of the dust on mine roadways. Such dust must not contain more than 50 per cent. of combustible matter, unless 30 per cent. of water is present. With certain highly inflammable dusts this regulation is not quite stringent enough, according to later researches—see below.

A. L. Godbert in Paper No. 87 (1934) has described a modified form of his earlier apparatus for determining in the laboratory whether the mine dusts are inflammable, and how much inert dust is required to be added to attain safety. It is perhaps necessary to remind the reader that the main conclusion reached from the Altofts and Eskmeals experiments was that coal-dust if mixed with an equal (or greater) proportion of stone-dust was not ignited by the firing of the 24 oz. of gunpowder. Stone-dust is thus an effective medium for preventing the initiation of a coal-dust explosion. It is not so effective in preventing propagation.

Godbert's apparatus is compact, easily assembled and cheap. It should be installed at all colliery laboratories. Briefly, it consists of a heated tube through which a cloud of the dust is blown by oxygen under slight pressure. One is able to observe whether inflammation occurs or not at the open end of the tube, "the criterion of inflammability being the projection of a white flame outside the inflammation tube into the open air." The results obtained compare with those given by the large scale tests in the 4 ft. diameter gallery at Buxton within very narrow limits. Needless to say the dimensions and details of procedure, *e.g.* pressure of oxygen, are important. Only one gramme of the dust is required for the experiment.

It may be mentioned in passing that Godbert finds that the "inert dust limit" can be calculated from the expression $L = 100 - \frac{1250}{V}$ where L is the necessary percentage of stone-dust required to prevent inflammation and V is the percentage of volatiles (on a dry ash free basis) present in the coal. Thus with coal of 30 per cent. of volatiles, there should be 58 per cent. of stone-dust, or rather 58 per cent. of inert matter (*i.e.* including stone-dust, ash of the coal and moisture). It has also been shown that hydrated dusts such as gypsum ($CaSO_4 \cdot 2H_2O$) are far more effective than ordinary stone-dust—due no doubt to their higher capacity for heat.

Wetting of the dust is not easy, and evaporation makes the treatment unsatisfactory unless it is continuous. Recently, however, it has been found that the addition of certain "wetting agents" (1 per cent.) to the water results in quite satisfactory moistening of the coal-dust (and stone-dust), and also gives a dried caked residue when the usual evaporation has taken place. An additional advantage of this procedure is that further deposits of dust on the residue can be "fixed" by simple watering; but the work is still largely in the experimental stage.

RECENT WORK

For some years the speed and movement of the flame of a methane-air explosion have been studied in detail by Wheeler and his co-workers in tubes of different diameters, closed or open at the ends, and some with one end closed and the other open (as in a mine gallery leading to a face of coal or rock). Paper No. 82 by Coward and Wheeler summarises this work, and shows how to estimate the influence of, (1) the composition of the gaseous mixture; (2) the type and shape of the enclosure; and (3) the position and nature of the igniting source, on the development of the explosion. The speed of flame in a firedamp explosion may vary from less than 1 metre per second to some 2,000 metres per second. The actual combustion of methane, however, has been difficult to investigate. Slow oxidation is known to be largely a series of hydroxylations; but spectrum analysis of the flame has shown the presence of such fugitive associations as C—C, C—H and O—H.

The work of the research staff at Sheffield has also proved the incendivity of the mixture through which flame has passed. In the 1933 Annual Report of the Board it is noted that "a mixture of methane and air through which flame has passed may ignite a fresh explosive mixture into which, under especially favourable conditions, it is projected. Various observations indicate that the gases have not completed their combustion during the passage of the flame. The continued radiation from the incendive gas is being measured by the photo-electric cell and is being analysed by optical filters, certain of the radiating molecules and radicles being identified by their spectra." The spectrum of flame from the mixture $CH_4 + 2O_2 + 6N_2$ "shows bands characteristic of each of the associations C—C, C—H and O—H," and also evidence of the production and burning of carbon monoxide. The spectrum of the "afterglow in the gas through which the flame has passed shows the bands of OH . . . but the bands C—H and C—C have disappeared. One conclusion from this is that the afterglow is not due to the combustion of molecules of methane that had escaped oxidation in the flame front."

On investigating the radiation effects of the afterglow by measuring changes in the electrical conductivity, it appears that recombination of the ionised molecules is mainly responsible for the visible radiation, and "it is proposed to study the electrical conductivity under conditions that will permit of a measurement of the number of ions in the flame. From these measurements, together with the known flame-temperature it may be possible to calculate the ionisation potentials of the various intermediate products or free radicles in the flame gases, and so to assist in their identification."

Researches are also in progress on the effects of "inhibitors"; *i.e.* the effects of the presence of small quantities of particular vapours on the inflammability of firedamp. Numerous substances have been tried, but so far none more effective than bromine and iodine and certain of their compounds. These substances present only in traces raise the ignition temperature of methane by about 100° C.

An automatic firedamp recorder has recently been devised and is fully described in Paper No. 86, by H. Lloyd. It is essentially an automatic gas analysis apparatus in which the firedamp is estimated from the drop in volume after combustion by a glowing platinum wire.

CONCLUSION

The work reviewed has been essentially that concerned with the prevention of firedamp and coal-dust explosions—and this by no means exhaustively. Such investigations are of necessity connected with work on spontaneous combustion, electrical safety devices, mine-lighting and mining explosives. Researches are still being vigorously pushed forward in these subjects with a view to greater and greater safety. Active co-operation with similar work in foreign countries, notably the United States, France and Belgium, is also maintained, joint conferences held, and changes of staff arranged temporarily.

The impression that explosions cause the majority of the deaths due to mine accidents is widespread but erroneous. In reality they cause as a rule less than 10 per cent., but with care this number should be reduced. Research is, however, now undertaken by the Board in such subjects as "falls of ground," haulage, mine ventilation, dust-inhalation, etc.; and it is to be hoped that the accidents due to " falls of ground " and to haulage (which together form the major sources of all mine accidents) will diminish.

Further, to make sure that the work is not lost sight of by the workmen and mine officials, programmes of lectures are arranged, an information service supplies educational material such as drawings, prints and lantern slides, and popular pamphlets are drawn up in non-technical language on " What Every Mining Man Should Know." Conferences of mining teachers are regularly held, and visits of mining students and mine officials to the research stations are organised throughout the summer months. About 3,000 of such visitors were received at the Buxton station last year.

It is probable, however, that the educational programme will have to be extended and the instructional activities intensified, if the results of the scientific work already accomplished are to be fully utilised. Legislation alone cannot deal with the various safety problems that arise daily in the mines : a fuller understanding of the dangers, a more " scientific " appreciation of the situation, is necessary on the part of the officials especially, but also on the part of the workmen.

The stimulation of interest in such problems is now the main problem. The cinema and the wireless have been used in this direction already, and will no doubt be used more effectively as the technique of propaganda improves ! But what is really required in the long run is the full realisation that the need of a scientific outlook for the mining section of the community is probably greater than it is for any other ; and the educational facilities in mining areas should be strengthened accordingly.

THE INDIAN LAC INDUSTRY

By T. HEDLEY BARRY, F I.C.

THE Indian lac industry has been the subject of much attention and not a little anxiety during the last few years.

The industry is of peculiar interest from whatever point of view it is regarded whilst the fact that it is practically confined to India and that, so far, no satisfactory substitutes for lac have been found make it of especial interest to the British Empire.

The forests of India cover an area of more than twice that of the British Isles and support, albeit in a very impoverished state, a population of some 400,000,000. The rapid and uncontrolled development of synthetic processes in the Western World and the obsession of the modern scientist with problems of the town and factory threatens to make the condition of this vast population and of all peasants the world over still more unfortunate.

In the case of India with its population of 400,000,000 the great majority of which is dependent upon the products of the soil, this aspect of the case is of vital importance. The Indian forestry and agricultural services have rendered great services to India and the Empire for which they have received very little recognition. The public and the politician can hardly be expected to appreciate these matters but there is urgent need that the scientist should take a wider view of his responsibilities and direct his energies to the more uniform development of the world's resources.

In the present article it is proposed to give a short summary of the work which has been carried out in India with the object of placing the lac industry on a scientific basis and thus enable it to compete on more level terms with the mass production methods of Western civilisation by supplying its products in forms more suitable for use and of the consistency and uniformity in quantity, price and quality which is essential to modern industry.

From what has been said, it will be appreciated that the problem is not simple. Social and economic factors are of dominating importance. Both the collection of the lac resin and its conversion to shellac and other forms required by the Western World are village industries. No other country in the world is so rigidly bound by tradition as India and the people engaged in the lac industry are mostly poor and ignorant, judged even by Indian standards.

It is clear that much more than an enthusiasm for scientific research is necessary to ensure success in dealing with the problem of lac production.

It remains to mention briefly the complementary problem of ensuring a market for the product.

Apart from the danger of replacement of lac products by synthetic substitutes, there is always the possibility of processes and methods involving the use of lac products being replaced by others from which they are eliminated. This question is now receiving attention and a system by which industrial lac research in the chief consuming countries of Europe and America is co-ordinated with the work of the lac research organisation in India has recently been inaugurated.

It is clear that a subject of such wide ramifications cannot be dealt with even briefly in a single article. In the present article the work in India will be discussed, but the significance and value of that work can only be estimated when the conditions under which it is done and the ideals which animate those responsible for it are appreciated.

Although the lac insect and its products have been the subject of investigation by many distinguished scientists since the industry was first described by Jan Huyghan van Linschoten in 1596, it is only comparatively recently that the lac resin has become the subject of almost exclusive interest.

In addition to the resin, the lac insect produces a dye which until the development of synthetic dyestuffs was the more important product, though the value of the resin in the varnish industry was appreciated.

It was fortunate that the development of the moulding industry, and particularly the gramophone, created a market for the resin which more than compensated for the loss of the dye trade. Until the recent economic depression the gramophone industry absorbed nearly half the total lac production and the threatened permanent loss of this market is one of the primary causes of the urgency of finding new uses for the resin.

The publication of Lindsay's and Harlow's classical survey of the whole industry in 1921 (*Ind. Forest Record*, 1921, **8**, Pt. I) may be said to at once summarise the work of the era of individual research and lay the foundation for a new era of co-operative and organised research which is the subject of the present article.

At this point it is desirable to describe briefly the conversion of sticklac into suitable forms for industrial application.

Stick lac is the raw material produced by the insect in the form of an encrustation on the branches of the tree upon which it feeds. It contains, in addition to the twigs, the dead bodies of the insects and its various products other than lac, principally the lac dye, sugars and proteins.

The twigs with their encrustation of lac are sometimes sold by the smallholder as stick lac but in most cases the encrustation is removed from the twigs and reaches the market as scraped stick lac. In any case, a further purification is necessary to remove the dust and finer vegetable débris. This is done by grinding and winnowing. The lac is then treated with water to remove soluble impurities and the bulk of the dye.

The dried product, seed lac, is then converted into either shellac or button lac. In many cases varying amounts of rosin and orpiment are added to produce the required product. In either case the mixture is fed into a long cylindrical bag about 2 inches diameter and 20 feet long. This travels in front of a fire, one end being continuously twisted to force the contents towards the operator seated at the other side of the hearth. Under the influence of heat and pressure the lac melts, oozes through the cloth and is scraped off.

If button lac is being made, the plastic mass is allowed to fall on a metal (usually zinc) surface and flows out into cakes about 4 inches diameter and ¼ inch thick.

In the case of shellac the plastic mass is spread on the outside of a bottle containing hot water and the sheet so obtained finally stretched in front of the fire. When cool it is broken up into the familiar flakes known as shellac.

This interesting process is conducted by natives according to tradition and the question at once arises as to how far it is amenable to such scientific control and modification as to be made suitable for large scale operation in factories organised according to Western ideas of efficiency.

The possibility of eliminating all native processes subsequent to the production of seed lac has naturally been considered for, although the heat treatment and manipulation involved in the conversion of seed lac to shellac probably affects the physical properties of the lac, it would seem quite possible for this to be effected in the consumer's country with the advantage that the process would be scientifically adjusted to produce the type of lac required for specific purposes.

Although considerable advance has been made, particularly in Germany, in the mechanical treatment of seed lac it does not at the moment seem desirable to push development in that direction too rapidly. A sudden change in this direction would severely disorganise the industry and deprive thousands of Indians of an important source of livelihood. At the same time, this would be better than the whole industry being lost through inability to produce the required product and withstand the competition of synthetic materials. Consequently, careful study of the properties of lac with special reference to the effect of the processes to which it is subjected is essential.

It may be pointed out, however, that the development of mechanical methods of making shellac provides a market for the waste product, *kiri*, which is left in the native shellac maker's bag and contains about 60 per cent. of lac. This can be extracted by solvents and used in the mechanical process. Lac made from *kiri* suffers from the disadvantage that it cannot be bleached satisfactorily though improvements in this respect are being made, particularly in Germany. It is, however, quite satisfactory for many purposes.

RESEARCH CONCERNING THE PRODUCTION OF LAC

Turning to the production of lac it is necessary in the first place to describe briefly the life history of the insect and the processes by which its resinous excretions are converted into marketable products.

Of the total output of lac, over 97 per cent. comes from India, the remainder from the adjacent Eastern countries Assam, Burma, Siam and Indo-China. Much of the lac produced in these latter countries is sent to India to be worked up. Of the Indian produce about 90 per cent. comes from the Central Provinces and Bihar and Orissa.

The individual trees on which the insect feeds, however, are generally speaking not gregarious, some being in jungle country whilst the majority grow in groups of a few trees scattered over the holdings of the natives. This in itself constitutes a serious problem in the organisation of the industry, for the 25,000 tons exported is made up of the contributions of thousands of smallholders each of whom seldom produces more than a few pounds of stick lac.

Organisation and education are therefore extremely difficult.

As the lac crop cannot of itself provide a livelihood for the owner of the trees there is a natural tendency to strip the trees in years when other crops are bad and to neglect them when other crops are good and this has a serious effect upon both the quality and the quantity of the product.

One of the first steps in fostering the industry is therefore to investigate carefully the various trees which act as suitable hosts for the insect and to encourage the cultivation of those which are most adaptable to systematic planting and exploitation.

About 90 different trees have been described as providing breeding accommodation for the lac insect but in practice only about seven are used to any great extent. These are: *Butea frondosa* Roxb. (Palas or Dhak), *Schleichera trijuga* Willd. (Kusum), *Zizyphus Jujuba* Lam. (Ber), *Zizyphus Xylopyrus* Willd. (Ghont), *Ficus religiosa* Linn. (Pipal), *Acacia arabica* Willd. (Babul), *Cajanus indicus* Spreng. (Arhar).

Some other trees, however, appear from the experiments carried out at the Research Station [1] to offer considerable advantages. The Khair tree (*Acacia Catechu* Willd.) for example appears to be particularly suitable as it can be grown almost anywhere and can be used in conjunction with the Kusum tree which, though it yields the best lac crop, requires two years' rest interval between each crop in order to maintain its healthy condition.

Transferring the brood lac from the Kusum tree to the Khair tree has been shown actually to improve the vitality and fertility of the insect and the Khair brood can be transferred back to Kusum again.

Apart from this, the tree is of extremely rapid growth and if properly tended can safely be infected at the age of two years. The necessity for giving the host trees an adequate period of rest and so arranging the sequence that there is always available new host trees with young shoots in a fit condition to receive the brood lac, presents a complex problem and affords much scope for ingenuity in organising plantations so that the balance between the number of trees in the various conditions—bearing, resting and preparing to receive the new brood—is correctly maintained.

Evidently, the seasonal activities of the tree must be complementary to those of the insect it has to support and therefore it is desirable that as many different species of trees as possible should be available and the characteristics of each and their relation to the life cycle of the insect should be known in the greatest detail.

Such work is necessarily a matter of years and it is, in fact.

surprising that so much accurate and definite knowledge should have been accumulated in a few years by a small staff.

It should be pointed out that there appears to be no connection between the botanical affinity of a tree and its capacity to act as a lac host nor has it been possible so far to formulate any chemical or biological test, short of actual trial, which will indicate with certainty that a particular species of tree will prove a satisfactory host. A minimum amount of suitable food for the insect must, of course, be available, but it does not necessarily follow that the insect will prove able to use it to full advantage.

As to the insect, although both male and female insects encase themselves in a coating of resin the amount produced by the female insect is much larger than that produced by the male, which having fertilised the female dies. The female insect, however, continues to excrete resin at a greatly increased rate after fertilisation, the total amount being about 300 milligrammes per insect. It is desirable, therefore, to maintain the ratio of females to males as high as is consistent with efficient fertilisation. In this connection, recent research has resulted in some extremely interesting discoveries concerning the possibility of parthenogenesis. It appears that under suitable conditions the female insect is capable of producing a new brood of insects without fertilisation and consequently the importance of the male insect is still further reduced [2].

About 2–4 months after fertilisation the new brood emerges and the larvæ proceed to take up their positions on suitable branches to which they attach themselves by sticking their long probosces into the bark. Having found a suitable anchorage they remain stationary—the female until its death after the emergence of the next brood, the male until it reaches maturity and emerges to fertilise the female.

This cycle occurs twice a year. There are, however, two distinct series. Insects bred on hosts other than the Kusum tree (*S. trijuga*) have two unequal cycles a year, one of three months and one of nine months, whilst insects bred on Kusum have two approximately equal cycles a year.

The lac cultivator has therefore four crops a year, two from insects bred on Kusum trees and two from those bred on other species.

A rather curious fact is that insects bred on Kusum trees retain the Kusum cycle even when transferred to other species of trees for the next crop [3].

In South India lac insects bred on a tree, *Shorea Talura*, go through three life cycles in thirteen months, but when brood lac from this source is transferred to North India the subsequent brood assumes the characteristic periodicity of the locally produced lacs [4].

The exact period occupied by each stage of the life cycle is in any case influenced to some extent by the condition of the host tree, the health of the brood and the climatic conditions.

The systematic production of lac therefore demands close attention from the cultivator and offers immense scope for research both as regards the care of the trees and the methods of dealing with the insect.

As regards the tree, the power of the insect to penetrate the bark and thus obtain nourishment is limited, and it is therefore important that the tree to which the lac insect is transferred should be pruned in such a way that at the critical moment of the emergence of the swarm of larvæ from the mother cell there should be close at hand a plentiful supply of young twigs and branches capable of affording them suitable habitation.

The radius of action of the new brood is only a matter of yards and unless it finds a suitable breeding ground within twenty-four hours the insects die.

During this transition stage, of course, the insect is at the mercy of wind and weather and it is therefore essential to reduce the time to a minimum.

The method of infecting the trees destined to support the new brood consists essentially in taking portions of the branches encrusted with healthy lac from the old tree just before the emergence of the new brood and tying them to the branches of the new tree in such a way that the insects can easily reach their new feeding ground.

The selection of the moment at which infection of the trees should be effected to produce the best results is obviously a matter of much importance and though by experience the native cultivator manages very well, the irregularity of the insects' life cycle due to various disturbing factors, particularly climate, creates a considerable degree of uncertainty which can only be eliminated by an accurate scientific knowledge of the sequence of events occurring within the cell which completely covers the insect.

Entomological research is therefore of fundamental importance.

In the case of the smallholder with a few trees, the infection of the new hosts can be carried out at very short notice as the emergence of the brood occupies a few days, but with large plantations this cannot be done. The systematic development of the industry would be greatly facilitated by the development of larger areas under uniform control, but the native system of land tenure, which it is rightly the policy of the British Government to respect as far as possible, makes this, at least for the present, almost impossible and the policy of the Research Association must be to accept the position and make the best of it. At the present time large holders do not account for more than about 5 per cent. of the production of stick lac.

The Lac Research Institute's first duty was naturally to obtain precise knowledge of the details of the insect's life history and this has already proved of the greatest value in educating the native cultivator in the best way to conduct his operations.

The date of swarming may vary as much as a month from one year to the next. As the lac to be used as brood lac for the next crop must be cut soon enough to ensure that there is no loss of the new brood and, on the other hand, must not be cut too soon lest the mother insect be weakened through lack of nourishment at the critical period, the decision when to cut brood lac and infect the new host is a vital one.

As a result of observations over a long period, it has been possible to prepare a time-table correlating the characteristic and visible alterations in the appearance of the insect and the time at which the brood will emerge. Armed with this knowledge the cultivator can make his plans accordingly. Normally, he cuts his brood lac and infects his tree two or three days before swarming is due. Accurate foreknowledge of the date of swarming and ability to recognise healthy brood lac has an extremely important bearing upon the stability of the industry and consistency of crops as it enables the cultivator to cut brood lac and send it to considerable distances. Thus areas in which the crop is good can help those in which local conditions have caused disaster.

It has also been shown that the healthy female insect is capable of arresting temporarily the emergence of the new brood when climatic conditions are unfavourable to their survival, consequently quite apart from the improvement directly due to the health of the insect the danger of the young brood being destroyed by sudden and unexpected temporary changes in the weather is reduced by ensuring that it is produced under the most favourable conditions.

There are, of course, many other directions in which biological research has greatly improved the prospects of the lac industry, so far as the production of the raw material (stick lac) is concerned. Questions of systematic plantation and cultivation of suitable host trees and the destruction of insect pests are of great importance and valuable information has been accumulated both at the Research Station and in various lac areas.

The educational side of the Research Station's work is, as will be appreciated, of first importance and the translation of scientific discovery into simple working directions and advice such as the poorly educated native can understand put into practice is an important function of the Indian Lac Cess Committee.

In the foregoing paragraphs attention has been directed mainly to the problems which arise in the attempts to reduce the production of the raw material, stick lac, to an orderly and scientifically controlled process and to eliminate so far as possible those violent fluctuations in both quality and quantity which constitute a serious, if not fatal, defect in any raw material that is to be used as an ingredient of scientifically produced and graded finished products.

The discussion of the subject beyond the production of stick lac is outside the scope of the present article but it is clear that whether or not the conversion of stick lac into other forms such as seed lac, shellac and button lac remains a native industry will depend entirely upon the ability of those responsible for the organisation of the industry to supply a suitable article at an economic price.

Apart from the factory in India, Germany in particular is successfully converting stick lac into shellac and other forms by mechanical processes. These modernised methods have the advantage that they can utilise the material, " kiri," which is left in the bag after the extrusion of the lac by the native process. The lac produced from this, however, is at present of inferior quality in some respects and seed lac is largely used as a raw material. In the production of bleached lac, particularly in America, seed lac is being used to an increasing extent and the exports of seed lac show a consistent increase almost wholly at the expense of shellac. Probably these developments are due in part to the abnormal economic conditions obtaining, but it is clear that these developments must react unfavourably upon India unless this section of the lac industry can be organised and equipped to meet them.

There appears to be no unsurmountable technical difficulty in converting seed lac, or even stick lac, into the desired form in the consuming country if necessary.

It has, however, already been pointed out that the organisation of the lac industry involves many questions more subtle than mere technology and, even with regard to technology, our knowledge of the properties and possibilities of a pure lac is still by no means complete. At the present time, therefore, organisation of the industry as it exists at present combined with scientific and technical research into the properties and possibilities of lac is the urgent and immediate question.

The ultimate form and scope of the industry in India must obviously be decided in the light of such research. In any case, the survival of the lac industry in any form at all must ultimately depend upon the ability of those responsible for its organisation to provide the consumer with a suitable product in such a form and at such a price that it can compete successfully with synthetic products which are already being placed on the market in such variety and at such a price as to constitute a distinct menace to the very existence of the lac industry.

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NEW IDEAS IN MODERN PHYSICS

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In recent months the discoveries of the neutron and positron have had a profound effect on the theories of atomic disintegration, and in a series of three advanced lectures at University College, delivered at the invitation of the University of London, Dr. G. Gamow formulated in a very interesting manner the new ideas which are at present occupying the minds of theoretical physicists. In this article an attempt will be made to give an outline of these ideas as they were unfolded in these lectures, and at the recent International Conference on Physics.

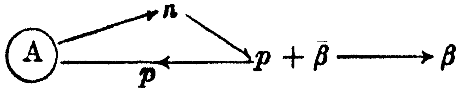
It is known that difficulties arose when the nucleus of an atom was pictured as an assemblage of protons and electrons. The main difficulty was that while the protons, because of their heavy mass, would obey the Schrödinger equation, the electrons, because of their small mass and very high velocity, would not ; yet the electrons did not seem to affect the behaviour of the protons in any way. With the discovery of the neutron the nucleus has come to be regarded as an assemblage of neutrons and protons and the difficulty vanished, for both neutrons and protons obey the wave equation. It is now necessary, however, to postulate that a neutron may become a proton by losing an electron, and a proton become a neutron by losing a positron, in other words, to consider the neutron and proton as the same heavy particle in two different quantum states.

These new ideas bring about many changes in the discussion of nuclear problems. Thus, if the mass defects or binding energies of the atomic nuclei are calculated on the basis of the older views, the mass defects continuously increase up to the element tin, after which they decrease, but in a rather peculiar manner resembling a series of steps, until we reach the radioactive elements which finally provide a continuous decrease. When the mass defects are calculated on the basis of the newer constitution of the nucleus a continuous increase of the mass defect right up to the group of radioactive elements is obtained, after which a decrease takes place. On the older views we are called upon to explain why every element of atomic number higher than that of tin is not radioactive; on the newer we have to explain the spontaneous disintegration of the radioactive elements alone.

In dealing with the question as to whether the neutron and proton are elementary particles, Gamow suggests that the question has no physical significance. A neutron and proton pair may be regarded as one in which the neutron continually changes to a proton by the emission of a negative electron, and the proton continually changes to a neutron by the emission of a positive electron. The forces between such a pair may be likened to the exchange forces between a hydrogen ion and a hydrogen atom, so successfully handled by Heitler and London. The force between two neutrons may be likened to the much weaker homopolar forces which may exist between two non-ionised atoms. Now, assuming such forces, Heisenberg has calculated the mass defect or binding energy of a nucleus consisting of neutrons and protons, from which the new potential energy curve of the nucleus is obtained. This curve is such that no element may break down spontaneously with the emission of a neutron, that emission of a proton may occur only if the atomic number is greater than about 160, and that an α-ray disintegration may occur if the atomic number is about 100. Applying the theory of quantum levels to the potential energy curve it is easy to see how an α-ray may leave an atom while a proton may not. Incidentally the new ideas are found satisfactorily to explain the emission of α-rays of short range by samarium.

Some interesting points are brought out in a description of the results obtained by plotting the radius of the nucleus against its mass. The radii of the light elements are deduced from measurements on the anomalous scattering of α-rays, and those of samarium and of elements of the well-known radioactive series are obtained from data on α-ray levels. It is not possible to draw a smooth curve through all the points, and the values for the radioactive elements show marked anomalies. Less definite anomalies are found in the curve of mass defect against atomic weight, and in the curve obtained when the ratio of the number of neutrons to the number of protons is plotted against the sum of the neutrons and protons in the nucleus. Yet the values of atomic radii obtained from neutron-scattering experiments are stated to show no such anomalies. Gamow explains this by suggesting that the α-particles in a nucleus, having an even number charge, 2, would not obey the Pauli exclusion principle and could therefore all occupy the same energy level. The neutrons, however, have to obey this principle and have to occupy a definite set of levels, and may even form a system of closed shells. Hence a neutron fired against a nucleus would interact only with the α-particles, but an α-particle so fired would interact with both α-particles and neutrons, so that α-ray measurements may be taken to refer to the neutron shells and neutron measurements to the α-particle core.

The new ideas can be applied to the problem of β-ray disintegration. In order to determine the condition for the release of a β-particle the following picture is used.



It is imagined that a neutron is removed from the nucleus A and subsequently breaks up with the formation of a proton and an electron. The latter is set free as a β-particle and it is imagined that the proton is returned to A. The condition for this release may therefore be written $\partial E/\partial n_1 + D - \partial E/\partial n_2 < 0$, where $\partial E/\partial n_1$ represents the work necessary to remove a neutron from A, D the work to disintegrate the neutron and $\partial E/\partial n_2$ the work to remove a proton from A. The condition for release of a positron may be obtained in a similar manner; a proton is removed from A, and forms a neutron and a positron, the latter leaves and the neutron is returned to A.

It is, however, doubtful whether energy is conserved in cases of β-ray and positron emissions, for it is very difficult to maintain the principle of conservation and explain the emission of a continuous β-ray spectrum by β-radiating nuclei; this spectrum appears to have a definite upper energy limit. It may be thought that all β-rays are initially liberated with the same energy, but that they suffer internal conversion or anomalous scattering in the nucleus and are so transformed into heat. To test this point Ellis attempted to measure the total energy set free by RaC by a suitable calorimeter method, and thus to calculate the energy liberated per β-ray emitted. If energy were conserved then the value obtained should correspond to the upper limit of the β-ray spectrum. Ellis actually found a value equal to the mean value given by measurements on the continuous β-ray spectrum. The discrepancy cannot be explained by γ-ray emission or by β-ray transmissions between two nuclei. To explain it Pauli has put forward the rather revolutionary idea that the nucleus contains particles of small mass with no charge and of high penetrating power. Such a particle is called a neutrino; it is very elusive, for Chadwick has shown that if a neutrino exists then it does not form more than one pair of ions in a track of 150 kilometres in air.

Further light is thrown on these problems when the relation between the energies of the β-ray emissions and the decay constants of the parent substances is considered ; *i.e.* when evidence of a kind of Geiger-Nuttall relation is sought for β-rays. It is known that by plotting the logarithm of the decay constant against the logarithm of the corresponding maximum β-ray energy, two straight lines, one above the other, are obtained. Now, Fermi assumes that the nucleus contains protons and neutrons, and that it is possible for a neutron to change into a proton with the emission of an electron and neutrino, the latter being assumed to be created at the moment of emission. He calculates the decay constant as a function of the β-ray energy, assuming a certain type of interaction between the particles in the nucleus, and he shows that the probability of emission is greatly decreased if the initial and resultant nuclei have not the same spin. He explains the upper graph by assuming that transitions with no change of spin are responsible for the points thereon, and the lower by assuming a difference of one unit between the spins. Points on still lower graphs are to be expected when the difference in spins is more than one unit, considerations which Gamow has used with satisfactory results in attempting to account for the successive disintegrations in the thorium series, assigning spins which are consistent with γ-ray data.

In like manner Ellis has applied these considerations to the transition RaB to RaC. He concludes that the measured β-ray distribution is in general a superposition of several partial spectra, all of the same shape for a given difference in initial and final spins, and has shown that the RaC to C′ β-ray spectrum may be built up of partial RaE to F β-ray spectra with intensities corresponding to the probabilities of emission. This success is obtained because the RaC to C′ components arise from spin transitions like those of the simple RaE to F spectrum. Unfortunately, more extensive treatment is hampered by lack of exact experimental data for the lower energy portions of the spectra, which is hard to obtain. Incidentally, there seems to be some disagreement between Fermi's theory and the observed positron emission of the artificial radioactive elements ; *e.g.* Ellis and Henderson find that in the positron spectrum from radiophosphorus—artificially produced as described in the last issue of SCIENCE PROGRESS, by exposing aluminium to α-rays from polonium—few, if any, positrons have an energy less than 200,000 electron volts, whereas the theory predicts a smooth fall to zero energies with an exponential drop only below 10,000. This discrepancy may possibly be caused by the particular form of interaction in the nucleus which Fermi assumes.

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By E. Maitland Wright, M.A., D.Phil., Christ Church, Oxford.

The Class-number in Imaginary Quadratic Fields and for Binary Quadratic Forms of Negative Discriminant.—The discriminant of a binary quadratic form $ax^2 + bxy + cy^2$ [a, b, c being integers and $(a, b, c) = 1$] is the number $d = b^2 - ac$. The two forms

$$Q(x,y) = ax^2 + bxy + cy^2, \quad Q'(x,y) = a'x^2 + b'xy + c'y^2$$

are said to be equivalent if there is a substitution $x = rX + sY$, $y = tX + uY$, where r, s, t, u are integers and $ru - st = 0$, which transforms $Q(x,y)$ into $Q'(X,Y)$. If this is so, the discriminants of $Q(x,y)$ and $Q'(x,y)$ are equal. If Q, Q', Q'' are three forms of discriminant d, and if Q is equivalent to Q' and Q' to Q'', then Q is equivalent to Q''. Hence all forms of the same discriminant d may be divided into classes such that (i) all forms in the same class are equivalent and (ii) no two forms belonging to different classes are equivalent. The number of classes of forms of discriminant d are known to be finite; this number is denoted by $h(d)$ and called the class-number.

If $d < 0$ and $a > 0$, then $Q(x,y)$ is a positive definite form, that is, for all real values of x and y (except the simultaneous values $x = y = 0$), we have $Q(x,y) > 0$. If $d < -4$, the class-number is equal to the number of classes of ideals in the quadratic field $P(\sqrt{d})$. If $h(d) = 1$, then there is only the principal class in $P(\sqrt{d})$ and the Fundamental Theorem of Arithmetic holds in this field.

Gauss conjectured (1801) that $h(d) \rightarrow \infty$ as $d \rightarrow -\infty$ through even values. The restriction to even values of d is due to the fact that Gauss only considered forms with an even b. Hans Heilbronn (*Quart. J. of Math.*, **5** (1934), 150–60) has recently proved Gauss's conjecture in the slightly more general form: (1) $h(d) \rightarrow \infty$ as $d \rightarrow \infty$. Important steps had been made before

Heilbronn towards proving the conjecture. Thus Hecke (1918) proved: (2) *If*

$$L(s,\chi) = \sum_{n=1}^{\infty} \chi(n)n^{-s} \neq 0$$

for $R(s) > \frac{1}{2}$ *and for all real characters different from the principal character, then* (1) *holds.* Deuring (1933) proved: (3) *If Riemann's function* $\zeta(s)$ *has at least one zero in the half-plane* $R(s) > \frac{1}{2}$, *then* $\lim_{d \to -\infty} h(d) \geq 2$. Recently Mordell has proved: (4) *If Riemann's function* $\zeta(s)$ *has at least one zero in the half-plane* $R(s) > \frac{1}{2}$, *then* (1) *is true.* Heilbronn proves: (5) *If there is, to modulus m, at least one character* χ, *principal or not, so that* $L(s,\chi) = 0$ *for at least one value of s in the half-plane* $R(s) > \frac{1}{2}$, *then* (1) *is true.* Now either the assumption of (2) or that of (5) is true, and so (1) is proved.

THE NUMBER OF REAL ROOTS OF A QUINTIC EQUATION.—Chaundy (*Quart. J. of Math.*, **5** (1934), 10–22 and 240) has recently found the conditions for a quintic to have one, three or five real roots, in a simple form. He considers the quintic in the "standard form"

$$t^5 + 10A_2t^3 + 10A_3t^2 + 5yt + x = 0 \quad . \quad . \quad . \quad (6)$$

and regards this as a straight line in the xy-plane. The equation of the envelope of the line may be written in parameter form

$$\left.\begin{aligned} x &= 4t^5 + 20A_2t^3 + 10A_3t^2, \\ y &= -\ t^4 - 6A_2t^2 - 4A_3t. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (7)$$

The number of real roots of the equation (6) is equal to the number of real tangents which may be drawn to the curve (7) through the point (x,y). The xy-plane may be divided into regions from the points of which one, three or five real tangents may be drawn. If Δ is the discriminant of (1) and if

$$\theta = x^2 + 16A_2y^2 - 76A_2A_3x - (272A_2^3 - 108A_3^2) \\ + 24A_2^2(40A_2^3 + 27A_3^2),$$

then the results are:

I. Five real roots: $\Delta \geq 0,\ 4A_2^3 + A_3^2 < 0,\ \theta \leq 0$;
$\Delta = 0,\ 4A_2^3 + A_3^2 = 0,\ \theta = 0$.

II. Three real roots: $\Delta < 0$;
$\Delta = 0,\ 4A_2^3 + A_3^2 > 0$;
$\Delta = 0,\ 4A_2^3 + A_3^2 = 0,\ \theta \neq 0$;
$\Delta = 0,\ 4A_2^3 + A_3^2 < 0,\ \theta > 0$.

III. One real root: $\Delta > 0,\ 4A_2^3 + A_3^2 \geq 0$;
$\Delta > 0,\ 4A_2^3 + A_3^2 < 0,\ \theta > 0$;
$\Delta = 0,\ 4A_2^3 + A_3^2 > 0,\ \theta = 0$.

A MACHINE FOR COMBINING SETS OF LINEAR CONGRUENCES.—Many problems in the theory of numbers depend for their solution on finding an integer x which is congruent to one of certain residues to modulus p for a sequence of primes p_i. D. H. Lehmer (*Math. Annalen*, **109** (1934), 661–7) has constructed a machine to solve this problem practically; the theoretical solution, of course, does not present any difficulty. The principle of the machine is that to every p_i corresponds a wheel with p_i holes equally spaced round the circumference and that each wheel moves forward one hole at a time. Each hole represents one residue (mod. p_i) and pegs are inserted to close the holes corresponding to residues (mod. p_i) which do not satisfy the particular problem considered. The machine is then set working and requires no further attention. When a number is reached which satisfies all the congruences a series of open holes is in line; a beam of light passes through and falls on a photo-electric cell. This stops the machine and the operator can reverse it slowly until he finds the exact solution by eye.

NORMAL NUMBERS.—Let $B \geq 2$ be a fixed whole number. Then every whole number can be expressed in exactly one way in the form

$$m = a_\mu B^\mu + a_{\mu-1} B^{\mu-1} + \ldots + a_1 B + a_0, \quad . \quad . \quad (8)$$

where the a_i are whole numbers, $0 \leq a_i \leq B - 1$ $(i < \mu)$ and $1 \leq a_\mu \leq B - 1$. We take $\varepsilon > 0$, $0 \leq a \leq B - 1$, and write $s(m,a)$ for the number of times that a appears in the expression of m in the form (8). If

$$s(m,a) = \left(\frac{1}{B} + \theta_a \varepsilon\right)\mu, \quad |\theta_a| \leq 1,$$

for all the values $0, 1, \ldots B - 1$ of a, then m is said to be ε-normal. Besicovitch (*Math. Zeitschrift*, **39** (1934), 146–56) has proved the two following theorems: 1. "If $\varepsilon > 0$, $\eta > 0$, then for sufficiently large n at least $(1 - \eta)n$ of the integers less than n are ε-normal." 2. "If $\varepsilon > 0$, $\eta > 0$, then for sufficiently large n at least $(1 - \eta)n$ of the numbers less than n have their squares ε-normal."

ASTRONOMY. By R. W. WRIGLEY, M.A., F.R.S.E., Royal Observatory, Edinburgh.

THE BRIGHTNESS OF COMETS.—The study of the brightness of comets, possibly as important a feature as their orbits and general appearances, was first undertaken by Dr. Holetschek. His work has lately been continued by S. Vsessviatsky, who, in the *Russian Astronomical Journal*, **10,** No. 3, publishes a General Catalogue of Absolute Magnitudes of Comets, comprising 442 appearances dating from Halley's Comet in 1066 to that of Brooks at the end of 1932. The absolute magnitude of a comet is defined as its general brightness expressed in stellar magnitudes assuming it to be placed at unit distance from both the earth and the sun. If there is no phase effect the apparent brightness obviously varies as the inverse square of the comet's distance from the earth, and it is assumed that it is also proportional to a certain power of its distance from the sun. Accordingly the absolute magnitude may be assumed to vary according to the law $(m - y \log r) - 5 \log \Delta$, where r and Δ are the heliocentric and geocentric distances, m the apparent magnitude, and y a constant. The latter can be determined for any comet if a sufficient number of observations of its brightness are available together with the corresponding values of its heliocentric distance. This has been done for several scores of comets, and the average value for y is found to be very close to 10, implying that the change of intrinsic brightness of any comet in its motion round the sun is inversely proportional to the fourth power of the radius vector. The absolute magnitude of a comet can therefore be immediately determined if its apparent brightness has been observed and its orbit calculated.

This law of the inverse fourth power would be uniformly true if the physical energy of the sun was always constant, and its action on a comet varied, like its illumination, according to the inverse square law. The solar influence, however, shows distinct changes of intensity, and it is interesting to note that the years 1905–7, which include a time of sunspot maximum, according to Vsessviatsky show exceptionally large values of y. On the other hand, the sunspot maximum of 1917 does not show the same effect, and the years 1913–17 furnish three cases of anomalous negative values.

The gradual decrease in brightness of short period comets seems to be definitely established. Encke's comet is stated to be growing fainter at the rate of one magnitude per century, others are disintegrating still more rapidly, and there is not a single example known of the reverse phenomenon. A similar process is also indicated in several comets of much longer period, including the famous one of Halley, whose magnitude is given as 2 to 3 in 1066 and only 4·6 in 1910.

The catalogue gives for each cometary appearance the value of y and the corresponding absolute magnitude if available, the magnitude assuming $y = 10$, the logarithm of the perihelion distance, the period, and the angle of inclination of the orbit to the ecliptic. Undoubtedly the most remarkable comet in the list is that of Sarabat, which appeared in 1729. Its absolute magnitude is given as – 3 and its perihelion distance as four units, so it was evidently a unique object. The next in order is Tycho Brahe's comet of 1577 with a magnitude of – 1·8 and a perihelion distance of one-sixth of a unit, and the great comet of 1811 ranks third with absolute magnitude 0·0. Donati's famous comet of 1858 is given a magnitude of 1·0, while in the present century the brightest is comet Delavan (1914, V), with magnitude 1·1. The faintest comet in the catalogue is Temple-Swift, 1908, with absolute magnitude only 15·1 and perihelion distance one unit.

The difficulty in representing cometary brightness by any mathematical expression and the danger in drawing general conclusions as to the history and future disintegration of comets from such data as are available are, however, illustrated by the striking variations observed in the brightness of the periodic comet 1925 II, described by G. Van Biesbroeck in the *Astrophysical Journal*, **79**, 5. This comet, which bears the name of Schwassmann-Wachmann, was discovered in 1927, and in many ways is decidedly exceptional. Its period is 16 years, its inclination only 9·5°, and its eccentricity only 0·135, so that its orbit is distinctly planetary, more circular than that of Mercury, and situated entirely between the orbits of Jupiter and Saturn. It has been continually under observation since its discovery, and, as it passed its aphelion in July 1933, it provides the only instance of a comet which can be observed all round its orbit. From the comparatively little change which takes place in its heliocentric distance, which always remains large, one would naturally expect the comet to be inert and inactive, closely resembling an asteroid, but actually the exact reverse is the case. Ever since its discovery it has been subject to mysterious and sudden outbursts of light, accompanied by great changes in its structure and appearance. For example, on February 10, 1931, it suddenly brightened to magnitude 13 after remaining fainter than magnitude 16 for over three months; early in January 1933 its light suddenly increased from magnitude 17 to magnitude 12, by February 18 it was of magnitude 15, on March 15 it was of magnitude 16, and on April 22 it had become a bright circular nebulous object of magnitude 12. A further spectacular change in brightness, illustrated by two plates, was observed by Professor van Biesbroeck between March 10 and 14, 1934. On the earlier plate the comet, then barely of magnitude 18, shows a tiny nucleus surrounded by a faint round coma traceable on the original negative to a diameter of 30″. Four days later the diameter is only 15″, but the whole coma is bright and sharply defined, the image of the comet being more like a star of magnitude 13. The rise in brightness of the comet must obviously be rapid, while its decline is relatively slow, but it is not yet possible to establish any periodicity in the fluctuations. No satisfactory explanation can as yet be suggested. It seems most improbable that internal action in the comet can be responsible for such wide variations in brightness, and solar action also seems inadequate, especially as outbursts occur when the comet is near its aphelion. Mr. L. E. Cunningham has examined plates taken by Dr. Reinmuth at Königstuhl and has identified images of the comet photographed by accident in 1902. The size of the images makes it probable that an outburst was in progress at that time, and, as the comet was then near its aphelion, it seems that such occurrences must be frequent when it is in this part of its orbit. The heliocentric distance remains always between 5 3 and 7·3 units, and it is difficult to imagine any solar influence capable of producing such great changes in the comet's brightness, the effect of which often persists for several weeks.

Discordances in Time Observations.—The anomalies which almost invariably present themselves in a series of time observations, both in the transits of individual stars observed on the same night and in the determinations made at different observatories at varying seasons of the year, have generally been ascribed to personal and instrumental causes. In *Monthly Notices*, *R.A.S.*, **94**, No. 3, A. J. Bamford suggests that an appreciable number of these small variations have in reality a meteorological origin, and can therefore be classed as systematic instead of accidental. Dr. Bamford was until recently the Director of the Colombo Observatory, and most of his evidence is drawn from work done there with a 4-inch transit instrument fitted with electric travelling micrometer. Colombo is an exceptionally suitable place for an analysis of this kind owing to the regularity of its seasons. The south-west monsoon reaches it after a long journey entirely over the sea, and the site of the observatory is such that a difference of a few degrees in the direction of the pressure gradient during the monsoon means practically no difference in the previous life history of the air currents. From pilot balloon observations it has been found that the wind at the surface of the ground is decidedly less than it is at a height of one kilometre, while the direction remains similar. Bamford suggests that this reduction in velocity is caused by the friction of the land surface with the result that more air is continually being brought over the sea from the west at the lower levels than is being carried away eastwards at those levels. This causes a congestion of air where the velocity begins to change, which is ultimately relieved by ascent. The immediate result is a coastal banking up of air, producing a block of high optical density east of the meridian which persists during the life of the monsoon. At Colombo, where declinations and zenith distances differ by only 7°, such a structure of the atmosphere would, by refraction, make all clock stars to be observed too soon, those near the zenith being least affected and therefore observed relatively later than the rest.

Bamford brings forward varied evidence in favour of his hypothesis. When groups of star observations are analysed in terms of zenith distance it is found that, during the south-west monsoon season when there is a definite pressure gradient across the meridian, the clock errors form a curve with a definite convexity, the most positive values being derived from stars near the zenith which have little or no azimuth coefficient. At other seasons of the year this effect is absent. This analysis certainly is in agreement with the theory of an increase in the refractive index of the air from west to east across the meridian. Longitude determinations made between 1926 and 1928 show more easterly values at times of steeper barometric pressure gradients from the south-west, and there is also a noticeable correlation between the value found for the longitude and the amount of bright sunshine during the day preceding the evening's observations. The longitudes determined after sunny days are greater than after dull ones, the former apparently being times of slower convection currents. Further support is received from measuring by pilot balloon observations the differences between the wind velocity at the ground surface and that in the main monsoonal stream at a height of half a kilometre. When these differences are plotted against the longitudes a mutual increase is at once apparent, suggesting that greater frictional effects, likely to be associated with a banking up of air and a change in the refractive index near the coast are definitely connected with the early observation of the clock stars. Bamford also discusses the general instrument question with special reference to level and pivot errors, and states that an attempt to connect the observational anomalies with star magnitude or with shift of pole proved fruitless. At Greenwich a definite correlation has been found between the discordances of the Cookson Zenith Telescope plates and the direction of the wind, but they had no connection with its strength. It certainly seems, in spite of some evidence to the contrary, that meteorological influences are by no means negligible in longitude determinations.

The attainment of greater accuracy in the rates of observatory clocks would go far towards solving these and kindred problems. In the *Observatory*, June 1934, G. A. Tomlinson has an article describing recent developments in precision time keeping, with special reference to oscillating clocks. Oscillating crystals have been found capable of maintaining a more constant rate than the best commercial pendulum clocks over short intervals, but for longer periods the latter are still the more reliable. Scheibe and Adelsberger at the Reichsanstalt claim for one of their crystal clocks a precision in daily rate of ± 0·002 seconds a day over a period of six months, but this is admittedly an exceptional run, and such clocks cannot normally be relied on for more than a few weeks. At the National Physical Laboratory the late D. W. Dye obtained a similar order of precision with a quartz crystal in the form of an annular ring in which the mode of vibration was one of uniform circumferential strain, the ring expanding and contracting with a period proportional to its mean diameter. Dye also utilised a tuning fork as oscillator, and this instrument is capable of maintaining a constant rate for six weeks to within 2 or 3 parts in 10,000,000. Tomlinson and Sears have developed a successful vibration clock from an elinvar bar one metre in length maintained in longitudinal vibration by electrostatic forces applied at one end. It is housed in an exhausted steel vessel controlled in temperature to within ± 0·002° C.

These oscillation clocks have the advantage of being independent of the small periodic variations in gravity which affect all pendulum clocks. Loomis and Marrison in New York, by a comparison of three Shortt free pendulum clocks and one crystal clock not only demonstrated the extremely constant rate of the last over daily periods, but were able to detect the effects of the diurnal variation in gravity on the others. It is also possible to radiate the frequency of an oscillation clock as a modulation tone, and so to operate other clocks in perfect synchronism at widely distant stations, a property which may prove valuable in longitude determinations or in gravity survey work.

The difficulty in maintaining stability of frequency makes the oscillation clock comparatively unreliable over long periods. Tomlinson suggests that, for astronomical purposes, a group of 3 such clocks should always be installed. This he regards as a minimum number for ensuring continuity of time keeping in view of the inevitable stoppages of each individual clock. The time for an overhaul should be only a few hours, two days should be sufficient for establishing the rate after the commencement of a fresh run, and the probable duration of an uninterrupted run is estimated at about two months, so it seems very unlikely that all three clocks would be out of action at once. The inherent potential accuracy of such a system is obviously much greater than that of any pendulum clocks now in use.

A crystal controlled time signal clock has been installed at the Mare Island Navy Yard, California, and has been operating satisfactorily since January 1934. It is described by Commander Offley and A. R. Rice in *Publ. Ast. Soc. of the Pacific*, 272, and the makers are the General Radio Co., Cambridge, Mass. It is intended to install similar clocks at Washington, Honolulu, the Panama Canal, and the Philippines, so that time signals based upon the transit observations at Washington and with an accuracy of ± 0·01 sec. may be made available throughout the world.

PHYSICS. B. L F. BATES, Ph.D., D.Sc., F.Inst.P., University College, London.

THE INTERNATIONAL CONFERENCE ON PHYSICS —At the time of writing the majority of physicists have been following with some interest the proceedings of the Joint Conference of the International Union of Pure and Applied Physics and the Physical Society, held in London and Cambridge from October 1–6, under the joint presidency of Professor Millikan and Lord Rayleigh. This conference was well attended by British and foreign members, although no doubt many of the former had their time at the conference sessions severely limited by the fact that nearly all University terms began during the first week in October. The conference virtually consisted of two portions, one on the solid state and one on nuclear physics, the latter covering the phenomena of cosmic radiation and artificial disintegrations or transformations of the nucleus; some of the more important communications dealing with recent work on cosmic rays will be described here, not always in the sequence in which they were presented.

The discussion on cosmic radiation was opened by Professor Millikan, who presented the results of a survey by I. S. Bowen, H. V. Neher and himself of the effect of latitude upon cosmic ray intensities carried out at very high altitudes over a region extending from Northern Canada to Peru. Observations were made in aeroplanes at altitudes up to 22,000 ft. and in three stratosphere ascents over the United States, including one made by Lt.-Commander Settle, who reached a height of 62,000 ft. In discussing the results of the survey, Millikan assumed at the outset that the conclusions of Epstein and Lemaître-Vallarta (see SCI. PROG. XXVIII, 98, 1933) on the effect of the earth's magnetic field on electrons travelling towards the earth were correct. In addition, he considered that, in general, the immediate ionising agents responsible for the observed ionisation associated with cosmic rays were free positive and negative electrons liberated by incoming photons. He did not postulate that other ionising agents were entirely ineffective, but he considered that their effects were negligible. In view of atmospheric absorption, then, it would be necessary to have a primary beam of electrons of 20,000 million electron-volts to account for the ionisation observed at the earth's surface, whereas only a few thousand million electron volts would be sufficient if the incoming radiation were entirely composed of photons or highly penetrating electromagnetic radiations.

The survey showed that the cosmic radiation consisted of two parts, one field-sensitive and the other non-field-sensitive, *i.e.* one was dependent on the magnetic latitude in which the observations were made and the other was not. In equatorial districts 93 per cent. of the ionisation at sea level was due to the non-field-sensitive portion ; of the remaining 7 per cent. one-sixth was attributed to the direct action of high-speed electrons and the rest to secondary ionisation produced by them. These high-speed electrons with energies of 10,000 million electron volts were considered sufficient to account for the observed changes in ionisation between eastern and western directions in high altitudes. Millikan finally considered that the enormous energies found in cosmic ray phenomena could only arise from the annihilation of matter.

The photon hypothesis is, however, not generally accepted, for, as Blackett pointed out in his contribution, comparatively little is known of the radiation in the upper parts of the atmosphere, in particular its variation with latitude and its azimuthal distribution are unknown. While he was certain that the greater part of the radiation which penetrates into the lower half of the atmosphere must be regarded as positively charged, he was not so certain that the conclusion formed by Johnson that nearly the whole of the incident radiation must consist of positively charged particles was strictly correct, and he felt that it was necessary to assume the existence of multiple charged particles in the upper atmosphere.

Auger and Leprince Ringuet also discussed the latitude effect for cosmic rays. They considered in particular the results obtained on a voyage from Le Havre to Buenos Aires. They found that the number of particles arriving vertically, the numbers of primary particles and of secondary particles, decreased in the same manner as the equator was approached. The distribution of the rays for different angles with the vertical was compared at the equator and at Buenos Aires, 38° S. At the equator the distribution curve was flatter and less symmetrical than at Buenos Aires, and this difference was taken as evidence that the greater part of the primary particles were positively charged, and arrived at the equator when moving from west to east after suffering considerable absorption in the greater thickness of air through which they passed.

While Blackett considered that it was at present uncertain whether the positively charged particles arriving at the earth's surface were positrons or protons, Anderson and Neddermayer inclined to the view that the majority of the high energy cosmic ray particles at sea level had electronic mass, and they considered that practically all the cosmic ray electrons observed at sea level arose only from photon encounters with nuclei.

Rossi, of Padua, opened his contribution with the following question. If several Geiger-Müller counters, separated by several centimetres of lead, are arranged in a straight line, and they are simultaneously discharged, does it really follow that this simultaneous discharge or coincidence is due to the passage of the same particle through all the counters ? This question was answered in the affirmative on the basis of the experiments outlined below. Three counters, each 2·5 cm. in diameter and 25 cm. long, were arranged vertically above one another with their axes parallel. Seven cm. of lead were placed above the first counter, 45 cm. between the first and second, and 45 cm. between the second and third. Under these conditions 1·9 coincidences per hour were observed, which were due either to particles which passed through all three counters and the interposed 90 cm. of lead, or else to non-ionising radiations which produced three different showers of secondary rays above the three counters, each shower exciting one counter. On removing the lead from above the first counter, the number of coincidences was raised to 2·2 per hour. On displacing the second counter laterally by 3 cm., with the initial conditions otherwise unchanged, the number of coincidences fell to 0·29 per hour. These results would follow from the first hypothesis. Consequently, far the greater part of the coincidences observed in the first experiment were due to ionising particles which passed through 90 cm. of lead.

Accepting the substantial correctness of coincidence experiments, Rossi considered that they might be summarised as follows. At sea level or on mountains the cosmic rays are divided into two sharply defined groups, a " soft " group for which the mean penetrating power is equivalent to several centimetres of lead, and a " hard " group for which the mean penetrating power is equivalent to several metres of lead. The absorbing power of different substances for cosmic particles is apparently roughly proportional to their densities.

The hard and not the soft group of cosmic rays is the one which is deflected by the earth's magnetic field, for on filtering out the soft radiation with suitable absorbers the east-west or azimuthal dissymmetry becomes more pronounced. The hard group is considered by Rossi to belong to the primary radiation entering the atmosphere, and from the latitude effect it is deduced that the primary particles observed at sea level must enter the atmosphere with an energy of at least 3,000 million electron volts. The soft group of particles is certainly of secondary origin, for, owing to absorption in the atmosphere soft group particles produced at considerable heights above the earth's surface would be unable to reach it, yet they are numerous at sea level.

Rossi then turned to results on the formation of " showers "—numerous groups of positive and negative electrons—obtained from coincidence measurements. He considered first an experiment in which the three counters were arranged at the apices of an inverted equilateral triangle, with a lead screen above them. The frequency of triple coincidences was measured for different thicknesses of lead, so that information was obtained about the way in which the number of showers from the lead varied with its thickness. Assuming that showers are formed by the hard primary corpuscular radiation, then with increasing screen thickness the number of coincidences should at first rise rapidly, reach a kind of saturation value for a thickness of several centimetres and then fall slowly according to the law of absorption of the primary corpuscular radiation. Yet, at sea level, it was found that the fall from the maximum value was quite rapid, showing that the radiation producing the showers is absorbed by lead much more rapidly than the primary radiation.

Again, when the lead screen was replaced by screens formed from different elements, all having the same mass per unit area, a rapid increase in the number of coincidences with increasing atomic number was found (see J. H. Sawyer, *Phys. Rev.*, **44**, 241, 1934). Yet, on placing above the counters a lead screen of slightly greater thickness than that at which the maximum number of coincidences occurred, and surmounting this with different screens, each with the same mass but different atomic number, it was found that the absorption of the radiation producing the showers increased with atomic number. Actually, it is approximately proportional to Z^2. Thus, Rossi concluded that the shower-producing radiation was a new component of radiation which had to be added to the above hard and soft components. The soft component clearly consists of particles from showers, *i.e.* secondary radiations from the shower-producing component of the cosmic rays.

Rossi then put the following question. Is the shower-producing radiation a secondary radiation from the hard group of cosmic particles, or must it be considered as a primary component of cosmic radiation ? The experimental facts are that the shower-producing radiation increases more rapidly with altitude than the number of cosmic particles , *e.g.* Johnson (*Phys Rev.*, **45**, 569, 1934) found an increase of 1 : 6·9 in the frequency of showers and of 1 : 3·78 in the number of particles, found by coincidences with three counters in the same vertical plane, between sea-level and a height of 4,250 mm. Moreover, the barometric effect, *i.e.* the diminution in intensity associated with increase in atmospheric pressure is more noticeable for the shower-producing radiation than for corpuscular radiation. Again, the curve of coincidences obtained with lead screens of different thicknesses above the three counters, at first falls off very rapidly and then very slowly ; indeed, some workers think that at 10 cm. it begins to turn up again to give a faint maximum at 18 cm. thickness.

From these results Rossi concluded that if the shower-producing radiation is a secondary radiation from the hard cosmic rays, then the intensity of the secondary radiation in equilibrium with a given intensity of primary radiation is greater in air than in lead. If, however, the shower-producing radiation is a primary component of cosmic rays, then he concluded that at sea level it is composed of two groups of rays of different penetrating powers, of which the softer is entirely absorbed by a few centimetres of lead. There is not yet sufficient evidence to permit us to decide in favour of either of these suggestions.

Blackett also considered the problem of shower-production in some detail. He pointed out that the total energy in a shower may amount to a total of 3,000 million electron volts. He was inclined to identify the shower-producing radiation with photons of high energy, mainly because the particles in the showers were positrons and electrons in about equal numbers, and hard gamma-rays are known to be absorbed mainly with the production of positron-electron pairs, and it was significant that the absorption coefficient of gamma-rays of 2 to 5 million volts energy is numerically about the same and varies with Z^2 of the absorber like the shower-producing radiation. This suggestion means that photons of energy up to 1,000 million electron volts have about the same penetrating power as photons of a few million electron volts, a conclusion not yet proved experimentally.

Blackett therefore pictured the formation of showers in the following way. A primary particle, a positron, electron or proton, of energy, 1,000 to 10,000 million electron volts, makes collisions with atomic nuclei resulting in the liberation of a number of photons of high energy, mainly in the forward direction, the actual effective cross-section for a collision being probably of the order of a few per cent. of the area of the nucleus. The photons so produced are absorbed with the formation of single and multiple positron-electron pairs, the cross-section involved now varying roughly as Z^2, and being much larger than the nuclear area for elements of high atomic number. Thus Blackett favours the hypothesis that the shower-producing radiation is a secondary component of cosmic rays.

Another aspect of cosmic ray phenomena was emphasised by Hoffmann, of Halle, who described experiments on Stösse. A Stoss is a relatively enormous burst of ionisation which occurs on rare occasions in high pressure ionisation chambers (see SCI. PROG., XXVIII, 95, 1933), being equivalent to about 4 million ions, when used above the surface of the earth. He pointed out that expansion chamber photographs frequently contained a set of multiple tracks, which *a priori* might be considered a Stoss ; but when one considered the number of ions involved, one realised that several hundred tracks would be required. The probability of finding several hundred tracks present at the same time is practically zero. Yet bursts occur far too frequently to be regarded as instrumental breakdowns, and in any case nobody has found any reason for such breakdowns. Thus Swann found Stösse even with potential differences of 4·5 volts between the collector and the walls of the chamber, *i.e.* under conditions which ruled out all possibility of ionisation by collision. Messerschmidt used pressure ionisation chambers of medium size to avoid statistical fluctuations of non-Stoss origin, which were balanced by a compensation device. He recorded 800 bursts and confirmed Steinke's observation that the number per day was connected with the shielding of the instrument, at first increasing rapidly and then becoming less with increase in shield thickness, the maximum number of ions liberated in a burst remaining constant at 3·8 million. Different shield materials gave different characteristic values of the thickness for which the maximum number of bursts per hour occurred. As the number of bursts diminished with increasing thickness of lead, the hardest components of the cosmic rays could not be regarded as responsible for their production.

Hoffmann therefore took the view of Swann and Locher (*Phys. Rev.*, **44**, 779, 1934) that neutrons were liberated in the Stoss reaction, the number and efficiency of these neutrons increasing with the atomic number of the shielding material. Compton and Bennett, in their communication, described measurements made with a 20-litre steel chamber containing argon at 50 atmospheres pressure and protected by a layer of lead shot equivalent to a lead shield 12 cm. thick. A compensation method was used, and the potential difference across the chamber was only 18 volts, which was less than the ionisation potential. Single bursts of as many as 200 million ions were observed under these conditions. With higher voltages bursts of 1,000 million ions were found at sea-level and of 7,000 million at high altitudes. The latter figure corresponds to an energy equivalent to the total mass of an atom of atomic weight 20, and they felt it was unlikely that the bursts could be attributed to any kind of nuclear reaction. It is clear, however, that the phenomena are sufficiently definite to require an adequate explanation in any theory of cosmic rays which may be put forward.

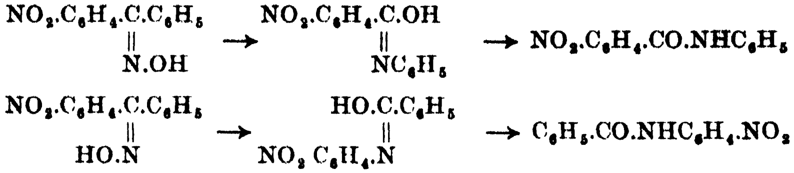
In view of the article contributed by Dr. Feather in the last issue of SCIENCE PROGRESS the contributions on artificial radioactivity will not be summarised here. Certain other aspects of the work of the Conference will be more extensively considered in an article on new ideas in modern physics which will be found in this issue. It is hoped later to provide an account of the contributions on the solid state.

The writer of this article looks forward with interest to the appearance of the reports to be issued by the Symbols and Nomenclature Committee of the International Union on Physics. He regards himself as fairly internationally-minded, but he finds it extremely difficult to accept the recent recommendation, namely, that the unit of intensity of magnetic field in future be named the oersted instead of gauss; moreover, he has yet to meet an English physicist who approves the change.

GENERAL AND ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

THE CONFIGURATION OF THE OXIMES.—The determination of the configuration of geometrical isomerides by chemical means presents certain difficulties as reactions do not always occur in the simple manner that is sometimes assumed. For this reason physical methods offer particular attractions and recently dipole moment measurements have been applied to the problem of oxime configuration. Before discussing these, however, it is necessary to review the position briefly as there are few topics in organic chemistry so inaccurately treated in the text-books.

Two chemical methods were used originally by Hantzsch, one for deciding the configuration of ketoximes and the other for aldoximes. In the former case the Beckmann change was employed and the assumption was made that the hydroxyl and the aryl group which were on the same side of the plane of the double bond between carbon and nitrogen changed places thus :

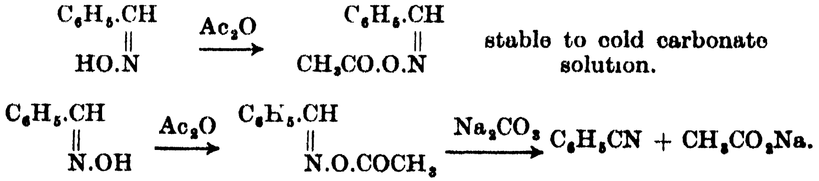


$$\begin{array}{c}\mathrm{NO_2.C_6H_4.C.C_6H_5}\\ \|\\ \mathrm{N.OH}\end{array} \rightarrow \begin{array}{c}\mathrm{NO_2.C_6H_4.C.OH}\\ \|\\ \mathrm{NC_6H_5}\end{array} \rightarrow \mathrm{NO_2.C_6H_4.CO.NHC_6H_5}$$

$$\begin{array}{c}\mathrm{NO_2.C_6H_4.C.C_6H_5}\\ \|\\ \mathrm{HO.N}\end{array} \rightarrow \begin{array}{c}\mathrm{HO.C.C_6H_5}\\ \|\\ \mathrm{NO_2\,C_6H_4.N}\end{array} \rightarrow \mathrm{C_6H_5.CO.NHC_6H_4.NO_2}$$

The group which appeared in the product of the Beckmann change attached directly to nitrogen was assumed to be, in the oxime, on the same side of the C = N plane as the hydroxyl group, a *cis* interchange of groups being supposed to take place.

With the aldoximes, Hantzsch relied upon the differential behaviour of the acetyl derivatives with sodium carbonate solution. On treating the isomeric aldoximes with pure acetic anhydride at 30° C. acetyl derivatives were formed , of these one, that prepared from the β-aldoxime, on the addition of cold aqueous sodium carbonate gave mainly the corresponding nitrile whilst the other, from the α-aldoxime, was unaffected. The assumption was made that the compound which readily lost acetic acid had the hydrogen atom and the acetoxyl group on the same side of the C = N plane thus :



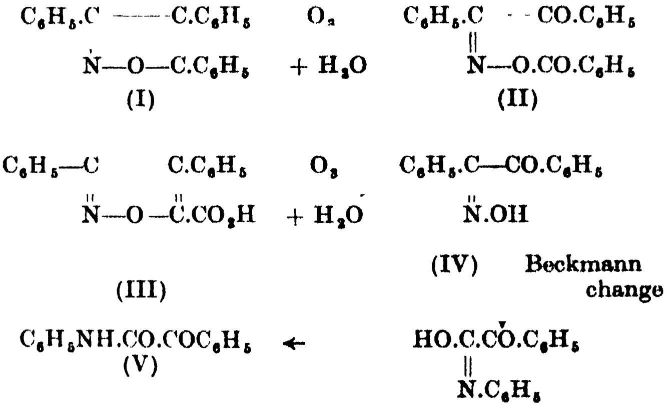
$$\begin{array}{c}\mathrm{C_6H_5.CH}\\ \|\\ \mathrm{HO.N}\end{array} \xrightarrow{\mathrm{Ac_2O}} \begin{array}{c}\mathrm{C_6H_5.CH}\\ \|\\ \mathrm{CH_3CO.O.N}\end{array} \quad \text{stable to cold carbonate solution.}$$

$$\begin{array}{c}\mathrm{C_6H_5.CH}\\ \|\\ \mathrm{N.OH}\end{array} \xrightarrow{\mathrm{Ac_2O}} \begin{array}{c}\mathrm{C_6H_5.CH}\\ \|\\ \mathrm{N.O.COCH_3}\end{array} \xrightarrow{\mathrm{Na_2CO_3}} \mathrm{C_6H_5CN + CH_3CO_2Na.}$$

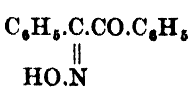
Here a *cis* elimination of acetic acid was supposed to occur most readily. The common statement that one aldoxime loses water to give a nitrile more easily than the other is not accurate, we have no evidence one way or the other on this point.

This method for determining the configuration of aldoximes is, at best, only an approximate one since the acetyl derivatives of both isomeric forms of 3 : 4-dimethoxy-5-bromobenzaldoxime (Brady and Wentworth, *J. Chem. Soc.*, 1920, **117**, 1045), 3 : 4-dimethoxybenzaldoxime (Brady and Dunn, *ibid.*, 1923, **123**, 1800) and *o*-methoxycinnamaldoxime (Brady and Grayson, *ibid.*, 1924, **125**, 1419) are unaffected by cold 2N sodium carbonate solution. In these three cases, however, one of the pair of acetyl derivatives is decomposed by 2N sodium hydroxide to give nitrile but, as was pointed out by Hantzsch (*Ber.*, 1891, **24**, 21) and by Hauser and Sullivan (*J. Amer. Chem. Soc.*, 1933, **55**, 4611) varying amounts of oxime are formed at the same time.

In 1921 Meisenheimer (*Ber.*, 1921, **54**, 3206) found that treatment of triphenylisoxazole (I) with ozone and water gave benzoyl-β-benzilmonoxime (II) whilst Kohler (*J. Amer Chem. Soc.*, 1924, **46**, 1734) obtained β-benzilmonoxime (IV) from 3 : 4-diphenylisoxazole-5-carboxylic acid (III). Now β-benzilmonoxime in the Beckmann change gives benzoylformanilide (V) therefore, if Meisenheimer's contention that his ring fission would leave the benzoyl and the hydroxyl or benzoylated hydroxyl groups on the same side of the C = N plane is correct, the two groups on opposite sides of this plane interchange in the Beckmann change, that is there is a *trans* migration of groups.

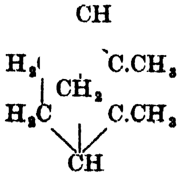


It is claimed that the mild treatment involved was unlikely to result in a *trans* ring fission of the isoxazole leading to the production of the isomeric

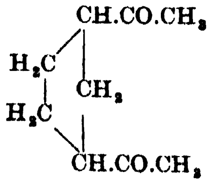


$$\begin{array}{l} C_6H_5.C.CO.C_6H_5 \\ \quad \| \\ HO.N \end{array}$$

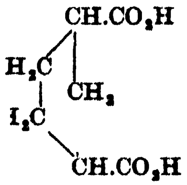
but santene (VI) on similar treatment gives a diketone (VII) which on further oxidation gives *trans* not *cis* pentamethylene-1 : 3-dicarboxylic acid (VIII) (Semmler and Bartelt, *Ber.*, 1908, **41**, 385).



(VI)

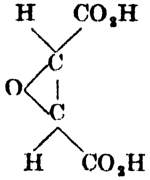


(VII)

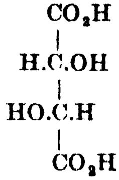


(VIII)

Unless the oxidation of (VII) to (VIII) results in a change in the orientation of the groups attached to the ring, the diketone (VII) must be the *trans* compound and the ring fission by ozone must have been in the *trans* sense. *Trans* ring fission is not uncommon even in the mildest reactions, for example Kuhn and Ebel (*Ber.*, 1925, **58**, 919) found that *cis*-oxidoethylenedicarboxylic acid (IX) on boiling with water gave quantitatively dl-tartaric (X) and not mesotartaric (XI) acid.

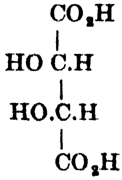


(IX)



$$\begin{array}{c} CO_2H \\ | \\ H.C.OH \\ | \\ HO.C.H \\ | \\ CO_2H \end{array}$$

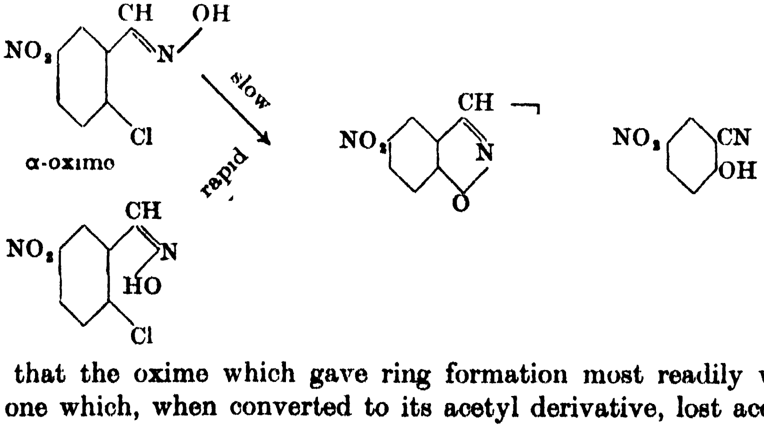
(X)



$$\begin{array}{c} CO_2H \\ | \\ HO\ C.H \\ | \\ HO.C.H \\ | \\ CO_2H \end{array}$$

(XI)

In 1925 Brady and Bishop (*J. Chem. Soc.*, 1925, **127**, 1359, compare also Meisenheimer, Zimmermann and von Kummer, *Annalen*, 1926, **446**, 210) suggested that the elimination of acetic acid from the acetyl derivatives of aldoximes occurred more easily in the *trans* sense, that is, when the hydrogen and the acetoxyl group were on opposite sides of the C . N plane. They showed that of the two 2-chlor-5-nitrobenzaldoximes, one lost hydrogen chloride more readily than the other under the influence of alkali thus :

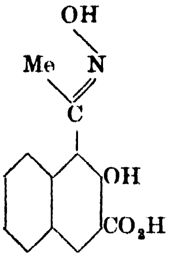


and that the oxime which gave ring formation most readily was the one which, when converted to its acetyl derivative, lost acetic acid most readily when treated with sodium carbonate.

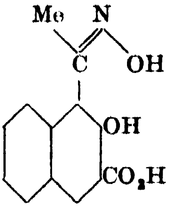
This work and that of Meisenheimer resulted in the inversion of the configurations assigned to oximes by Hantzsch and this action has been supported by a number of other investigations most of which, however, depend upon ring formation with its ambiguities.

Recently methods depending on physical properties have been applied to the problem. Auwers and Ottens (*Ber.*, 1924, **57**, 446) consider that their determinations of the optical properties of the isomeric aldoximes and their ethers support the new configurations but Brady and Goldstein (*J. Chem. Soc.*, 1926, 1918) measured the dissociation constants of pairs of isomerides and found that if the analogy of the cinnamic acids was accepted their values supported the old configurations ; in the latter case, however, the analogy cannot be satisfactorily justified.

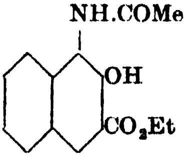
Meisenheimer, Theilacker and Beiswenger (*Annalen*, 1932, **495**, 249) have very adroitly made use of the asymmetry and consequent optical activity conferred on a molecule by restriction of rotation of a group, as described in a recent article (SCIENCE PROGRESS, 1934, **29**, 106). When alkaloidal salts of the two oximes (XII) and (XIII) were crystallised only in the case of (XIII) was there any indication of resolution into optical isomerides.



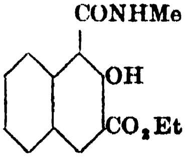
(XII)



(XIII)

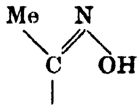


(XIV)

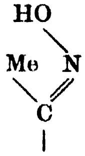


(XV)

The asymmetry of the molecule in (XIII) is due to the fact that the

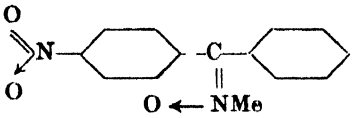


group is not co-planar with the naphthalene ring and cannot become so, as can the group

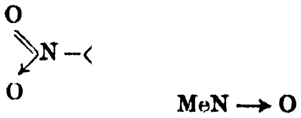


in (XII), owing to the restriction of rotation of the group about the bond joining it to the ring due to the interference between the oximino hydroxyl group and the ring on the left and the phenolic hydroxyl group on the right. Since the esters of (XII) and (XIII) in the Beckmann change give (XIV) and (XV) this reaction must involve a transmigration of groups as Meisenheimer originally postulated.

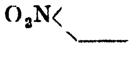
The dipole moments of a number of oxime derivatives have been measured and the results applied to the problem of their configuration. Sutton and Taylor (*J. Chem. Soc.*, 1931, 2190) investigated the isomeric N-methyl ethers of *p*-nitrobenzophenone oxime (XVI) and (XVII)



(XVI)



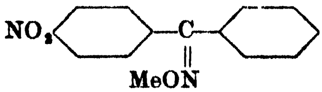
(XVII)



(XVIII)

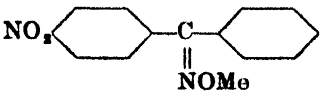
The electron displacements towards oxygen in the nitro group and in the N ether group will re-inforce one another in (XVI) but will counteract in (XVII) and the authors assigned the configuration (XVI) to the compound with the higher dipole moment and the configuration (XVIII) to the oxime from which this ether was prepared. As this oxime in the Beckmann change gives *p*-nitro-benzanilide the evidence supported Meisenheimer's views provided no change of configuration occurred during the preparation of the N-ethers.

Since only one N-alkyl ether can be obtained from stereoisomeric aldoximes the above method could not be employed with these compounds but Taylor and Sutton (*J. Chem. Soc.*, 1933, 63) have overcome the difficulty by comparing the dipole moments of the O-ethers of ketoximes and aldoximes. It is not possible, at present, to speak with the same confidence about the electron configuration about the = NOMe group as in the case of the MeN → O group but these authors found that the *p*-nitrobenzophenone oxime O-methyl ether (XIX) related to the N ether (XVI) through its preparation from the same oxime (XVIII) has a lower dipole moment than the isomeric ether (XX)



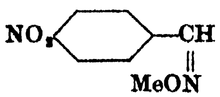
d.p.m. 3·75 × 10^{-18} e.s.u.

(XIX)



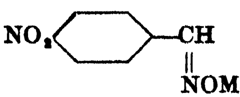
d.p.m. 4·26 × 10^{-18} e.s.u.

(XX)



d.p.m. 3·39 × 10^{-18} e.s.u.

(XXI)

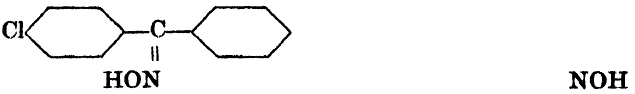


d.p m. 3·88 × 10^{-18} e.s.u.

(XXII)

The assumption was made that the aldoxime O-methyl ether with the lower dipole moment would have the same relative configuration as the ketoxime O-methyl ether and the two O-methyl ethers of *p*-nitrobenzaldoxime were assigned the structures (XXI) and (XXII). Now (XXI) was prepared from the oxime whose acetyl derivative gave nitrile with sodium carbonate so these results support the contention that acetic acid is more readily eliminated when the H and $OCOCH_3$ groups are in the *trans* position.

Finally Parsons and Porter (*J. Amer. Chem. Soc.*, 1933, **55**, 4745) have measured the dipole moments of the oximes themselves in dioxan, a solvent in which they are not appreciably associated. They find that, within the limits of accuracy of their method, the two *o*-chlorbenzophenone oximes have the same moments but that in the cases of *m*- and *p*-chlorcompounds the ones with the higher melting points, which in the Beckmann change give products with the unsubstituted phenyl group attached to nitrogen, have the lower moments; using Meisenheimer's configurations this means that (XXIII) has a lower moment than (XXIV)



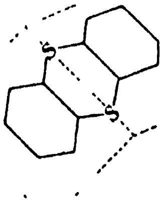
$2\ 320 \times 10^{-18}$ e.s.u $2\ 381 \times 10^{-18}$ e.s.u.

(XXIII) (XXIV)

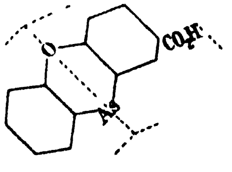
Since the measured moment is the vector sum of several moments in the molecule it is difficult to interpret these results. The authors state that if the hydroxyl group, as a whole, is positive with respect to the nitrogen atom their measurements support Meisenheimer's mechanism of the Beckmann change, if the reverse the original view of Hantzsch. It seems, however, impossible to take this simple view. Since isomeric oximes have different dissociation constants (Brady and Chokshi, *J. Chem. Soc.*, 1929, 2271) the electronic distribution about the oxygen atom must be different in the two isomerides and in one case the hydroxyl may be positive and in the other negative with respect to the nitrogen. This may account for the small difference in dipole moments, $0{\cdot}061 \times 10^{-18}$ e.s.u., compared with those found by Taylor and Sutton for the *p*-nitrobenzophenone oxime O-methyl ethers, namely $0{\cdot}51 \times 10^{-18}$ e.s.u. It is noteworthy that the difference is greatest in the case of *m*-chlorbenzophenone oxime ($0{\cdot}11 \times 10^{-18}$ e.s.u.) where the halogen will have least, and smallest (nil) in *o*-chlorbenzophenone oxime where it will have the greatest influence on the dissociation constant.

CONFIGURATION OF HETEROCYCLIC COMPOUNDS.—Lesslie and

Turner (*J. Chem. Soc.*, 1934, 1170) have published a paper of first-class importance on this subject. The planar structure of fused carbocyclic rings of the type of naphthalene and anthracene has been established but little is known of similar heterocyclic systems. The configuration of such rings will be influenced by the atomic diameters and the valency angles of the elements present in them. Now Bennett and Glasstone (*ibid.*, 128) have confirmed the result of Bergmann and Tschudnowsky (*Ber.*, 1932, **65**, 457) that thianthren (XXV) has a dipole moment and must, therefore, be non-planar, the two benzene rings being inclined to one another. The stereochemical stability of this structure is so far unknown but it occurred to Lesslie and Turner from geometrical considerations that, if the two non-carbon atoms in the system were different and



(XXV)



(XXVI)

of as widely divergent atomic radii as possible, the stability of the folded structure would be at a maximum, and that by choice of a suitable compound optical isomerism due to the molecular asymmetry should be obtainable. They have succeeded in resolving 10-methyl-phenoxarsine-2-carboxylic acid (XXVI) into its enantiomorphs. The optically forms are remarkably stable, for example, heating the l-acid for six hours at 100° with N-sodium hydroxide caused no measurable racemisation.

Apart from the folded structure the asymmetry may be due to an asymmetric tervalent arsenic atom but, in view of the much smaller dipole moment of arsine than that of ammonia and the repeated failures to resolve substituted ammonias, this contingency seems unlikely, especially when one considers the remarkable optical stability of the enantiomorphs. Further support for the folded structure is provided by the action of ethyl iodide on the optical isomerides, the optical activity soon falling to zero. Though no arsonium salt has been obtained which is optically stable it seems unlikely, if the original activity was due to a particularly stable asymmetric tervalent arsenic atom, that the addition of ethyl iodide to the active acid would be accompanied by complete loss of activity since an asymmetric arsonium ion is produced. On the other hand, since the formation of an alkiodide is accompanied by the acquisition of a positive charge by the arsenic atom the effective size of the latter must undergo instantaneous diminution, the arsenic atom in the arsonium ion being, probably, little larger than an oxygen atom. It is almost inconceivable that a folded molecule could withstand the agitation consequent on this change of volume without complete loss of optical activity.

BENZENE SUBSTITUTION.—The very general interest in problems relating to benzene substitution makes the recent work of Wibaut, van de Lande and Wallagh (*Rec. trav. chim.*, 1933, **32**, 794) of considerable importance. Sheffer and Brandsma (*ibid.*, 1926, **45**, 522) have pointed out that the variation in the amounts of isomerides formed in the nitration of certain benzene derivatives at different temperatures is in accordance with the supposition that the differences between the energies of activation for the *ortho*-, *meta*-, and *para*-positions are the deciding factors for the relative amounts of isomerides which are formed, whilst the differences between the entropies of activation are small and may be neglected. From this it follows that the amount of an isomeride which is produced in small quantity during a substitution in the nucleus increases with rise of temperature. Hollemann, for example, has shown that in the nitration of toluene, the proportion of the *meta*-isomeride formed rises from 2 7 per cent. to 4·0 per cent. between − 30° and + 60°. Linden (*Rec. trav. chim.*, 1911, **30**, 305) found that in the bromination of bromobenzene at 55° in the presence of ferric bromide 84·6 per cent. *para*, 13·4 per cent. *ortho* and 1·9 per cent. *meta* dibrom derivatives were obtained. The present authors now find that at 400° the proportions are 57 per cent. *para*, 20 per cent. *ortho* and 23 per cent. *meta*, at 450° 25 per cent. *para*, 20·5 per cent. *ortho* and 54 5 per cent. *meta* whilst at 630° the ratios are 21·2 per cent. *para*, 18·9 per cent. *ortho* and 59·9 per cent. *meta*. There appears to be an abrupt change in the amount of the *meta*-isomeride formed between 400° and 450° and further rise of temperature then has little effect. The authors, therefore, point out that their results do not conform with the theory of Scheffer in its simple form.

PHYSICAL CHEMISTRY. By A. H. HUGHES, Ph.D., Laboratory of Colloid Science, Cambridge.

COLLOIDAL ELECTROLYTES.—The ramifications of Colloid Science have become so great in the last few years that severe subdivisions must be made in reviewing this field. The class of colloidal electrolytes alone, which was the subject of a recent Faraday Society discussion, provides material of increasing importance in biological as well as technical processes. Colloids possessing an electrical conductivity are of two main types, the colloidal suspensions and the colloidal solutions (true "sols"), exemplified by colloidal gold and soap respectively. According to Freundlich the term colloidal electrolyte should be reserved strictly for those *pure* substances which spontaneously form conducting solutions with colloidal properties in respect to dialysis and Tyndall phenomenon.

Even such a clear demarcation as this leaves us with a rapidly growing class of substances. In addition to the soaps we may now include the long chain sulphonic acids and amino compounds, of considerable technical importance as emulsifiers and detergents. We are concerned with many dyestuffs bearing a chemical resemblance to the sulphates and amines. The proteins are colloidal electrolytes; while salvarsans and certain starch compounds have some claim to inclusion.

Solutions of such types of ionising molecule owe their colloidal properties to the large size of the ions of one sign, the so-called ionic micelles, and on this count they fall into three classes. In the first place the micelle may in concentrated sols be composed of an aggregate of many molecules, undergoing reversible dissociation into monomeric ions on dilution. This occurs in the case of the soaps, where the individual ions are too small for a very dilute solution to exhibit colloidal properties. Secondly, reversible dissociation may occur to ionised single molecules of size sufficient to preserve the colloidal nature, as in the case of the congo-dyes. Finally the micelle may remain unchanged on dilution, as observed with most proteins over a wide range of hydrogen ion concentration.

The electrical properties of the solution are by our restriction due to ionisation, but to obtain anything like a clear picture we require information as to the size and shape of the micelle, its valency, and further the mode of distribution of the charges around the micelle with respect to the crystalloid "gegenions"[1] of opposite sign in the dispersion electrolyte. In the case of the proteins, as we shall see, the situation is further complicated by the amphoteric nature of the colloidal ion, and by the fact that the colloidal particle is *one* molecule contributing many charged centres instead of an aggregation of molecules each contributing one charged centre. Even in the simplest idealised case of an ionising colloid there is much doubt as to the best way in which to represent the arrangement of the electric charges in the particle surface. The fundamental conclusion of Hardy that the charge is responsible for the stability of the system and also for its electrophoresis leaves open two avenues of approach. First, electrophoresis may be regarded as a direct counterpart to electro-osmosis and streaming potentials, in which case we refer to the electric double layer of Helmholtz[1] and Gouy, and express it in terms of a potential. Secondly, we may consider the phenomena as an extended case of electrolytic conductivity, treating the particle as a highly polyvalent ion. In each case an extrapolation is implied, in the first case from macroscopic systems downwards, in the second from crystalloid ions upwards. As a morphological description of the particle surface it is immaterial whether it is viewed as a wall with an electric double layer or as a polyvalent electrolyte, but if we wish to penetrate further we must choose that hypothesis which most fully accounts for the statics and dynamics of colloids. Hartley (*Trans. Farad. Soc.*, 1935) has considered in detail the difficulties presented by the application of the Debye-Huckel theory to colloidal electrolytes. Such attempts are optimistic since the theory is only strictly applicable to small ions of low charge in true and dilute solution, but it is of interest that the von Smoluchowski cataphoresis formula deduced from the earlier view of Gouy of a " diffuse double layer " surrounding a colloid particle, becomes for large and polyvalent spherical ions the same expression as the Debye-Huckel-Onsager ionic mobility relation.

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[1] Helmholtz conceived the boundary layer as a charge on the rigid surface separated from a layer of ions of opposite sign (the so-called "gegenions") situated at a small distance from the surface.

Kemp and Rideal (*Trans. Farad. Soc.*, 1935), using solutions of gliadin and of quartz particles covered with gliadin, have attempted a correlation of the observed cataphoretic mobility with the theoretical Debye-Huckel expression. The relation holds only over a limited range of electrolyte concentration, from an ionic strength of 0·008 to 0·013. At high ionic strengths anions are preferentially adsorbed, which at this *p*H (4·40) reduces the surface density of charge and gives a correspondingly low mobility. It is, however, encouraging that any agreement is obtained at all.

A new possibility which needs further investigation has been suggested by some experiments of de Jong and van der Linde (*Biochem. Z.*, **262**, 161, 1933). It was found that in gum arabic and certain other sols there appear to be more electric units on the particle surface than there are possible sources of ionisation. This indicates another source of electrokinetic activity quite distinct from ionisation; and a very well known and even more puzzling case is that of the high negative electrokinetic potential shown by non-ionising substances such as naphthalene or paraffin against pure water.

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[1] See note, p. 493.

In the case of the inorganic sols there are two views as to the origin of the particle charge, that of surface adsorption and that of surface ionisation. Thus As_2S_3 was considered to adsorb sulphide ions giving a negative charge while the positive charge on ferric hydroxide could be ascribed to a discharge of OH ions from the surface of the micelle. The work of Pauli (*Trans. Farad. Soc.*, 1935) definitely indicates that the latter view, namely the presence of an ionising complex in the micelle, is of greater generality than was hitherto suspected. Pauli has succeeded in preparing metallic sols of platinum, gold and silver in great purity and high concentration by means of electrodialysis and the refinement of electro decantation, in which the sol is concentrated by electrodialysis and repeatedly washed with pure water. These sols, in addition to organic sols such as the congo dyes and gum arabic, show clearly defined ionising complexes, and should thus be regarded as colloidal electrolytes. Rabinowicz (*Trans. Farad. Soc.*, 1935) points out that considerable caution is needed in a general extension of he conception of colloidal electrolytes to lyophobic colloids, particularly in regard to silicic acid sols.

Donnan (*Trans. Farad. Soc.*, 1935) has examined the possibilities of applying the thermodynamic theory of membrane equilibria to the determination of the osmotic pressure and molecular weights of colloidal electrolytes in general. It appears that the results will be independent of any special formulation of the nature of the colloidal electrolyte. In regard to the determination of the valency or " free charge " of a colloidal ion, several methods are available, all open to severe criticism. Thus the total acid or base equivalent of the ion can be estimated by chemical analysis, together with measurements of conductivities, osmotic pressure, and electrometric measurements of the activity of the ions. In such cases the apparent ionisation may be much lower than the true value owing to interionic forces. The state of affairs is considerably complicated when we come to deal, as is more often the case, with mixtures of colloids and crystalloids, the effect of the latter on conductivity or osmotic pressure being difficult to assess. With certain limitations the least objectionable line of attack seems to be from measurements of membrane potentials, and Adair and Adair (*Biochem. J.*, **28**, 199, 1934) have discussed fully the evaluation of valency therefrom. They have found that the best approximation to an absolute value is obtained over a range of concentration where the membrane potentials are small, the limiting value for very low concentrations of colloid being found by extrapolation from a series of measurements where the composition of the dialysate is maintained constant.

One of the peculiar phenomena noticeable with certain colloidal electrolytes, particularly the soaps, is the relation between solubility and temperature. The experimental fact is that with many long chain acids and salts a sudden and very great increase in solubility occurs over a very short range of temperature giving rise to the misleading expression "the melting point of curds" (the Krafft point) (Krafft and Wiglow, *Ber.*, **28**, 2566, 1895). Krafft himself attempted to relate this critical temperature to the melting point of the fatty acid from which the soap was derived, the two temperatures being in several cases within a few degrees of each other. Lawrence (*Trans. Farad. Soc.*, 1935) further points out the close correspondence between the melting points of the acids $C_nH_{2n+2}COOH$ and the hydrocarbons $C_{2n}H_{4n+2}$ which is possibly due to the retention of the binary association of the carboxyl groups in the acid melt, lateral adhesion between the double chain length being the governing factor. While this explanation may suffice for the case of fatty acid and hydrocarbon, it would scarcely seem generally applicable to the Krafft phenomenon. Thus the results of Murray and Hartley (*Trans. Farad. Soc.*, 1935) for cetyl sulphonic acid and its alkali salts show that the Krafft point is different for the sodium, lithium and potassium salts. The sodium salt has a solution temperature 4 Centigrade degrees above the melting point of the acid (54° C.), while the acid has its solution temperature 18 centigrade degrees below its own melting point. Incidentally the melting points of the salts are all above 300° C. Murray and Hartley explain the phenomenon on the basis of a mass law equilibrium between simple molecules and large micelles. The concentration of micelles of m simple molecules will vary as the mth power of the simple molecule concentration, and as soon as the micelles reach an appreciable fraction of the whole they will increase rapidly if m is large. On raising the temperature it is clear that after a fairly sharp critical concentration is reached the solubility will increase very sharply and over a relatively short temperature range. Any difference in "nonmicellar" solubility will naturally be reflected in a different "Krafft point." Murray and Hartley deduce mathematically the form of curve to be expected for cases of nonionic micelle formation and apply the necessary modifications for the case of ionic micelles. They conclude that in dilute solutions the aggregation is mainly ionic.

The nature of aqueous solutions of dyestuffs has been clouded in the past by the persistent use of solutions containing uncontrolled amounts of foreign electrolytes. It is only recently, in the hands of Robinson, Valko and others (*Trans. Farad. Soc.*, 1935; *Proc. Roy. Soc.*, **131**, A, 576, 596. 1931; *ibid.*, **143**, A, 630, 1934) that highly purified dyes have been prepared and trustworthy data obtained. The properties of these pure dyes are affected in a marked degree by small quantities of electrolytes. Thus in the presence of electrolyte, benzopurpurin 4B gives the well-known needle-shaped particles in the ultramicroscope, exhibits streaming double refraction and has a fairly high viscosity. The pure dye is found to be optically clear, shows no streaming double refraction, and the viscosity is low. Three methods of attack offer themselves in determining the size and composition of these submicronic micelles; osmotic pressure, diffusion and mobility determinations. Many colloidal electrolytes, and among them dyes such as congo red, give a low osmotic pressure and a high conductivity, the usual explanation being found in ionic aggregation. From osmotic data alone the aggregation can only be gauged qualitatively except for those dyes whose particle size is unaltered by presence of electrolyte; since from Donnan's theory of membrane equilibrium, a true measure of the osmotic pressure due to the non-diffusible particles alone is only obtainable in presence of excess of an electrolyte such as sodium chloride. Such dyes are exceptional and usually we must rely on diffusion or mobility measurements for estimates of the "aggregation number" (number of ions per micelle).

In considering the diffusion coefficients of these solutions, it is again impossible to obtain results of any significance as to particle size from measurements in electrolyte-free solutions. In the absence of salt the non-colloidal ions tend to diffuse away rapidly from the colloid ions and create a field which pulls the latter along. If excess sodium chloride be added, no potential gradient is set up and the colloid ion will diffuse independently. The Stokes-Einstein equation is now applicable to these lower diffusion coefficients. If we assume (*a*) that the particles are spherical; (*b*) that the salt has not aggregated the dye; (*c*) that the particle density is the same as for the solid dye, we can now calculate the particle mass and hence the aggregation number.

In this manner Robinson finds for Bordeaux Extra a particle weight of 900 (Molecular Weight 700), *i.e.* a very slight degree of association, but the method is obviously inapplicable to many dyes for which the salt content necessary to ensure Stokes-Einstein conditions is large enough to cause aggregation or flocculation. The relative states of aggregation of different dyes may be deduced from the change of mobility with concentration. In general the mobility increases with concentration to a maximum, due to ionic aggregation, but subsequently falls off owing to interionic forces and inclusion of "gegenions." It is possibly these latter reasons which may account for the marked quantitative discrepancy always found between the osmotic pressure and diffusion results on the one hand and the mobility results on the other hand.

The particle size of dyestuffs is of immediate concern in the actual dyeing process. Thus in a series of direct cotton dyes, Morton (*Trans. Farad. Soc.*, 1935) has shown that the dyeing speed increases as the degree of aggregation decreases, the particle size being estimated from ultrafiltration measurements, using cellulose membranes. It appears that rapid dyeing occurs where the dye particles are smaller than the micro-capillaries of the material to be dyed. It is of interest, however, that for many dyes no adsorption occurs in the complete absence of salts. Since salts in higher concentrations usually cause aggregation and even flocculation of the dye it is indicated that the crystalloid ions must play some other part in dyeing; possibly preferential adsorption of cations is necessary in discharging the negative charge on the dyeing substrate, such as viscose.

In considering the proteins as colloidal electrolytes we are faced with several fresh possibilities. First, according to the nature of the protein there is a very wide range of actual molecular magnitude, from molecular weights of about 3,000, as in clupeine, to several millions as in the case of hæmocyanins. Secondly, the colloidal particle appears to be in most cases an individual rather than an aggregate of individual molecules. Finally, the most outstanding feature is the amphoteric nature of the proteins. One molecule of egg albumen contains about 200 amino-acid residues, with a like number of potential charged centres, positive and negative. The fact that these ionising groups are part of one and the same polypeptide chain imposes a spatial restriction on their mutual tendency to symmetry, which restriction is absent in the case of simple crystalloids. At the iso-electric point there is a balance between the oppositely charged centres in the zwitterion[1] state of the molecule, and the unique effect of hydrogen ions on the behaviour of proteins is undoubtedly due to its ability either to combine with the $-COO^-$ centre or to be released from the $-NH_3^+$ centre, thus destroying the zwitterion balance. Jordan

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[1] The now accepted structure of an amphoteric electrolyte when electrically neutral is that of a hybrid ion or "zwitterion" of the type $R(\overset{+}{N}H_3).CO\overset{-}{O}$.

Lloyd (*Trans. Farad. Soc.*, 1935) discusses the application of these ideas to the process of tanning. Tannin forms a negatively charged colloid and will not combine with the protein till the latter is made sufficiently acid to upset the zwitterion balance and leave $-NH_3^+$ centres in predominance. Another important point is that a large negatively charged colloidal ion such as occurs in tannin will displace crystalloid anions from association with protein, for the cohesion between large ions each with numerous points of electrostatic attraction is much greater than the cohesion between one large ion and many smaller ones of opposite sign.

In the more directly biological aspects of the proteins Caspersson and Hammersten (*Trans. Farad. Soc.*, 1935) publish some preliminary results which open up large fields of speculation. They have made some ingenious attempts to demonstrate a definite protein structure in cell nuclei, especially in chromosomes. They show, for example, that red blood corpuscles from *Triton* are precipitated both in the nucleus and in the protoplasm by sulphosalicylic acid, but only in the nucleus by lanthanum , the presence of the nucleic acids is, however, a serious complicating factor. In many biocolloids it is difficult to determine whether the presence of a minute proportion of a certain element or group is an impurity or an essential constituent. Thus in the case of starch sols, Samec (*Trans. Farad. Soc.*, 1935) brings forward evidence that the negative charge on the particle is due to phosphoric acid which is directly attached to the polysaccharide as an ester. Much fruitless discussion and confusion may result in similar cases, as to whether the " small impurity " is a direct chemical constituent of the colloidal molecule or merely a necessary factor in the stability of the physical state of the colloidal solution.

In conclusion it is necessary to add two notes of warning, first, in regard to premature speculation on purely thermodynamical grounds in a field which is at best more directly amenable to molecular-kinetic considerations , and secondly in regard to the use of high precision methods on complex physiological systems where it is possible to focus the attention too closely on some small part and to theorise on the whole.

NOTE.—References to *Trans. Farad. Soc.*, 1935, are to papers in the Faraday Society discussion on Colloidal Electrolytes, to be published in January 1936.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

MAJOR TECTONICS: STRATIGRAPHICAL AND REGIONAL GEOLOGY. —In his paper on " The Thermal History of the Earth," Prof. A.

Holmes (*Journ. Wash. Acad. Sci.*, **23**, 1933, 169–94) reiterates his hypothesis invoking subcrustal convection currents to explain the thermal behaviour and cooling history of the earth. He finds it consistent with a wide range of geological and geophysical data, competent to explain the distribution of continents and oceans, and "sufficiently complex to match the astonishing complexities of geological history."

The view put forward by G. F. S. Hills (*Geol. Mag.*, LXXI, 1934, 275–6) that the non-uniform distribution of granitic and basaltic basements in the earth's crust results from vertical convection currents in the viscous original melt, forms the occasion of a suggestive discussion by Dr. H. Jeffreys (*ibid.*, 276–80).

In his Presidential Address to the Geological Society of America R. A. Daly (*Bull. Geol. Soc. Amer.*, **44**, 1933, 243–64) discussed "The Depths of the Earth." In the depths the keys to orogenetic and epeirogenetic movements of the earth's crust, to the problems of former land bridges between continents, and to palæogeographic problems in general, are to be sought. Daly believes that "Cosmogonic theory, seismological results, study of thermal gradients and of isostatic adjustment, like the multitude of facts of tectonics, all seem to support a thesis : our planet is still too hot to crystallize at any depth greater than about 80 kms. or 50 miles."

Of importance for the study of the depths of the earth are three papers by W. A. Zisman ("Young's Modulus and Poisson's Ratio with Reference to Geophysical Applications" ; "Compressibility and Anisotropy of Rocks at or near the Earth's Surface" ; "Comparison of the Statically and Seismologically Determined Elastic Constants of Rocks" ; *Proc. Nat. Acad. Sci.*, **19**, 1933, 653–86) and one by L. D. Leet ("Velocity of Elastic Waves in Granite and Norite," *Physics*, **4**, 1933, 375–85).

The geothermal curve must have been affected by the Pleistocene ice sheet and the subsequent long-period surface temperature variations. W. O. Hotchkiss and L. R. Ingersoll (*Journ. Geol.*, XLII, 1934, 113–22) have made an attempt to find such an effect and to interpret it in terms of past surface temperatures by subjecting a series of geothermal measurements made in the Calumet and Hecla copper mines of Michigan to mathematical analysis. The results indicate that the glacial epoch ended in this region 20,000 to 30,000 years ago, and that it was followed by a period with ground temperatures distinctly warmer than the present which was succeeded by a cooler period lasting until recent times.

A useful summary entitled, "Comments on Isostasy. Made by Authors of Geological and Geophysical Books and Papers," has been compiled by W. Bowie, and published in mimeograph form (*Nat. Res. Council, Washington, D.C.*, 1932, 49 pp.).

From the spacing of raised beaches and other facts of uplift and subsidence R. G. Lewis (*Geol. Mag.*, LXX, 1933, 499–521) builds the theory that the mechanism of earth movements is based on "plastic anticlines" which travel slowly through the earth's crust, and he postulates two possible causes for this mode of movement. A further paper by the same author deals with "Subsidence in the European Area" on these lines (*ibid.*, LXXI, 1934, 76–85).

In a valuable discussion of "Die Zeitlichkeit der Erdkrustenbewegungen," E. Haarmann (*N.J. f Min* , B.–B. 71, Abt. B., 1933, 48–87) insists on the lack of definiteness in geological time divisions, and combats Stille's classification of crustal movements as orogenetic and epeirogenetic. In his view the earth's crust moves continuously, and it is not possible to separate in time the differently-disposed movements.

A. P. Coleman destructively criticises the Wegener theory in his paper entitled, "Ice Ages and the Drift of Continents" (*Journ. Geol.*, XLI, 1933, 409–17). In his view the Wegener theory completely fails to explain the Pleistocene and Permo-Carboniferous glaciations.

A. Born's essay, "Über Werden und Zerfall von Kontinentschollen." (*Fortschr. d. Geol. u. Pal.*, X, Heft. 32, 1933, 347–422) is a review of present opinion on the growth, structure, and breaking-up of the continents. Notices of this work are to be found in *Geol. Mag.*, LXXI, 1934, 91, and *Amer. Journ. Sci.*, XXVII, 1934, 231.

L. J. Chubb's paper on "The Structure of the Pacific Basin" (*Geol. Mag.*, LXXI, 1934, 289–302) is a most suggestive summary of the petrological, stratigraphical, and tectonic evidence on this subject. Mr. Chubb inclines to the view that inconstant land bridges may have existed over at least part of the Pacific region.

The interrelations of epeirogeny, stratigraphy, and sedimentation are dealt with in M. Y. Williams' paper "Land Movements and Sedimentation" (*Bull. Geol. Soc. Amer.*, **43**, 1932, 993–1002).

Investigation of the rifts produced by the earthquake of December 20, 1932, at Cedar Mt. Nevada, by V. P. Gianella and E. Callaghan (*Journ. Geol.*, XLII, 1934, 1–22) shows that they have a bearing on the origin of basin-range structures. The relative horizontal movement along the rifts is the same as on two great structural lines, the San Andreas Rift, and the eastern front of the Sierra Nevada. The conclusion is drawn that horizontal as well as vertical movement is concerned in the origin of basin ranges.

In a preliminary study Professor F. E. Suess makes some interesting comparisons between the " Grundgebirge " of the Alps, the Caledonides of Scotland, and of the Appalachians (*Akad. Wiss. Wien. Sitz. d. Math.-Nat.*, Kl. 5, Juli 1934, 9 pp.). A related study is that by H. Becker on " Das Hochgebirgeproblem in den südlichen Appalachen im Vergleich zu europäischen Gebirgen " (*Min. u. Petr. Mitt.*, **44**, 1933, 193–8). The " Hochgebirgsproblem " is the occurrence of metamorphic rocks overlying normally-developed sediments, and is treated from the point of view of metamorphism.

In Dr. E. Dix's important memoir on " The Sequence of Floras in the Upper Carboniferous, with Special Reference to South Wales " (*Trans. Roy. Soc. Edin.*, LVII, Pt. III, 1934, 789–838) an attempt is made to indicate the value of fossil plants in wide-range correlations. Nine plant zones have been distinguished in the Upper Carboniferous of the Swansea district, and traced in other parts of the South Wales coalfield. Most of these are recognizable in other British areas and compare closely with the floral divisions adopted in certain European areas. It is suggested that these divisions afford a more satisfactory basis for correlation of Upper Carboniferous in Great Britain than those established by Kidston, while they agree closely with those based on the non-marine lamellibranchs and on the distribution of marine strata.

Writing on " The Carboniferous-Permian Dilemma " H. E. Wheeler (*Journ. Geol.*, XLII, 1934, 62–70) concludes that " greater precision in correlating strata and events would be attained by a more extended use of the European stage designations than by continuing to employ terms of such variable and ambiguous connotation as " Carboniferous " and " Permian." This author does not appear to be aware of Dr. R. L. Sherlock's exhaustive work on this subject (see SCIENCE PROGRESS, October 1928, p. 223, and April 1932, p. 574).

The valuable up-to-date summaries of British regional geology so frequently published by the Geologists' Association are continued by three memoirs : R. G. S. Hudson *et alia*, " The Geology of the Yorkshire Dales " (*Proc. Geol. Assoc.*, XLIV, 1933, 227–69) ; C. I. Gardiner *et alia*, " The Geology of the Gloucester District " (*ibid.*, XLV, 1934, 109–44) , V. Wilson *et alia*, " A Synopsis of the Jurassic Rocks of Yorkshire " (*ibid.*, 247–91). A paper that should be mentioned in connection with the above is K. C. Dunham's " Structural Features of the Alston Block " (*Geol. Mag.*, LXX, 1933, 241–54).

The following are notable papers on various aspects of English stratigraphical geology : A. E. Mourant, " The Geology of Eastern Jersey " (*Quart. Journ. Geol. Soc.*, LXXXIX, 1933, 273–307) ; G.

H. Mitchell, " The Borrowdale Volcanic Series and Associated Rocks in the Country between Long Sleddale and Shap " (*ibid.*, XC, 1934, 418–44); S. Melmore, " The Glacial Gravels of the Market Weighton Area and Related Deposits " (*ibid.*, 141–57); J. F. Kirkaldy, " The Sandgate Beds of the Western Weald " (*Proc. Geol. Assoc.*, XLIV, 1933, 270–311); J. F. Kirkaldy, " The Tectonic Development of the Western Weald in Lower Cretaceous Times " (*Geol. Mag.*, LXX, 1933, 254–68); F. Raw, " Triassic and Pleistocene Surfaces on some Leicestershire Igneous Rocks " (*ibid.*, LXXI, 1934, 23–31).

In his review of the " Age-Problems of the Moine Series of Scotland," Professor H. H. Read (*Geol. Mag.*, LXXI, 1934, 302–17), on the basis of new evidence from central Sutherland, comes to the conclusion that this series and its metamorphism are of Pre-Torridonian date, while a Lewisian age is not excluded.

Professor T. J. Jehu and Dr. R. M. Craig complete their studies of the geology of the Outer Hebrides with a memoir on North Harris and Lewis (*Trans. Roy. Soc. Edin.*, LVII, Pt. III, 1934, 839–74). The belt of crushed gneiss which has been observed bordering the eastern sides of the southern islands reaches its greatest development in Lewis. Its base again rests on a thrust-plane along which there are conspicuous exposures of flinty-crush-rock and mylonite. The authors believe that the disputed red conglomerates and sandstones of Stornoway are most probably of Torridonian age.

Scottish Carboniferous rocks are dealt with in the following papers: M. Macgregor and J. Pringle, " The Scottish Millstone Grit and its Position in the Zonal Succession " (*Summ. Prog. Geol. Surv. for* 1933, Part II, 1934, 1–7); M. Macgregor and W. Manson, " The Carboniferous Rocks of Inninmore, Morvern " (*ibid.*, 74–84).

J. J. Hartley's impressive memoir on " The Geology of North-eastern Tyrone and the Adjacent Portions of Co. Londonderry " (*Proc. Roy. Irish Acad*, XLI, Sect. B, No. 16, 1933, 218–85) is mainly a description and interpretation of the Tyrone Igneous Series, which consists of tuffs and lavas of the spilitic series, dolerites, diorites, quartz-porphyries, granites, and gabbros, with their metamorphic equivalents. With considerable probability these rocks are correlated with the Highland Border Series of Scotland, and regarded as of Late-Cambrian or Cambro-Ordovician age. They have also a strong resemblance to the Arenig igneous series of the Girvan-Ballantrae area of Ayrshire.

Professor E. B. Bailey and Dr. W. J. McCallien describe and discuss " The Metamorphic Rocks of North-east Antrim " (*Trans. Roy. Soc. Edin.*, LVIII, Pt. I, 1934, 163–77). They establish the existence in Antrim of certain horizons in the Scottish Dalradian known as the Ben Lui Schists, the Loch Tay Limestone, Pitlochry Schists, Green Beds, and Ben Ledi Schists and Grits. The three last-named series are involved in a recumbent fold which appears to be analogous to the Carrick Castle fold of Cowal. The petrography of " Green beds " and of albite schists is discussed in some detail.

An important memoir on " Bau und Entwicklung des Apennins, besonders der Gebirges Toscanas " by P. de Wijkerslooth, has appeared (*Geol. Inst. Univ. Amsterdam*, Publ. No. 51, Privately published, 1934, 426 pp., 8 pls.). The author summarises previous work on the Apennines and combines with it his own field observations during the period 1929–32. His main conclusions are conveniently stated in twenty-three paragraphs at the end of the memoir.

The main object of the Summer Excursion of the Geologists' Association in 1934 was to examine the Oslo district of Norway with special reference to Holtedahl's epoch-making discovery of Permian plants amongst the rocks formerly assigned to the Devonian (see SCIENCE PROGRESS, January 1934, p. 491). A most valuable memoir on the geology of the region was prepared for the excursion by Professor Holtedahl and his colleagues, together with notes on the Finse and Bergen districts (*Proc. Geol. Assoc.*, XLV, 1934, 307–77).

Dr. H. von Eckermann presents a preliminary report on the geology of the Loos region in Hälsingland, Sweden (*Geol. För. Stockholm Forh.*, **56**, 1934, 341–58). The rocks are all of Archæan age, and the author states that the investigation of this district has " turned out to be of the greatest importance for the interpretation, parallelization, and chronological division of the different Pre-Cambrian formations within Fennoscandia." Over eighty new chemical analyses of the rocks are promised in a later work.

An exhaustive memoir by A. L. Lundegren (Swedish, with German summary) deals with the Cretaceous formation in the Kristianstad district of southern Sweden (*Geol. För. Stockholm Förh.*, **56**, 1934, 125–313).

E. H. Kranck continues the work on the tectonics of the oldest Urgebirge (Sveco-Fennides) of the islands of South-West Finland, which was begun by Wegmann and himself (*see* SCIENCE PROGRESS, April 1932, 581) with an investigation entitled: " Kinetisch-geologische Studien im Schärenhof von Ekenäs, S. W. Finland " (*C.R. Soc. géol. de Finlande*, No. 6, 1933, 30–53). The view that Archæan schists do not occur in definite mountain zones but uniformly over the whole of the earth's crust, is not confirmed by these detailed tectonic studies.

H. Frebold continues his useful summaries of Arctic geology (see SCIENCE PROGRESS, October 1933, 301) in a paper entitled "Tatsachen und Deutungen zur Geologie der Arctis" (*Medd. Dansk Geol. För.*, **8,** 1934, 301–26). He thinks that the extension of the Spitsbergen Caledonides to North Greenland postulated by L. Koch must be regarded as unproved until the exact age of the folding in this region is known.

Geological investigation in East Greenland proceeds with great vigour (see SCIENCE PROGRESS, October 1933, 301). L. R. Wager deals with the general geology of the region from Angmagsalik to Cape Dalton (*Medd. om Grönland*, **105,** No. 2, 1934, 46 pp., 7 figs., 12 pls.). It consists mainly of Pre-Cambrian gneisses and granites associated with a charnockite series. Mr. Wager also describes the Tertiary basaltic lavas (with four new chemical analyses) and associated sediments. An interesting feature of Tertiary igneous activity in this area is the existence of many plutonic centres, including one of nepheline-syenite.

The following memoirs have recently appeared: L. Koch, "The Geology of Inglefield Land" (*Medd. om Grönland*, **73,** No. 2, 1933, 38 pp.); H. Frebold, "Untersuchungen über die Verbreitung, Lagerungsverhältnisse und Fauna des oberen Jura von Ostgrönland" (*ibid.*, **94,** No. 1, 1933, 81 pp.); C. Teichert, Untersuchungen zum Bau des Kaledonischen Gebirges in Ostgrönland" (*ibid.*, **95,** No. 1, 1933, 119 pp.); G. Säve-Söderbergh, "Further Contributions to the Devonian Stratigraphy of East Greenland" (*ibid.*, **96,** No. 1, 1933, 40 pp.); J. P. Ravn, "New Investigations of the Tertiary at Cape Dalton, East Greenland" (*ibid.*, **105,** No. 1, 1933, 15 pp.). The most important of these appears to be C. Teichert's memoir on the structure of the Caledonian mountains. He holds that the western Crystalline Complex has been thrust over the sediments, and that the overthrusting and faulting must have taken place between the end of the Ordovician and the Middle or Upper Devonian.

The first full geological description of a Spitsbergen coalfield is given in A. K. Orvin's memoir on the "Geology of the King's Bay Coalfield" (*Skr. om Svalbard og Ishavet*, Nr. 57, 1934, 195 pp.). This Tertiary coalfield is notable for the extreme deformation of its strata by Late-Tertiary folding. The field has only been preserved amongst the older rocks by reason of intense folding, thrusting and faulting.

New Triassic and Jurassic profiles have been described and measured by the writer in his paper on "Stratigraphical Observations in the Stor Fjord Region of Spitsbergen" (*Trans. Roy. Soc.*

Edin., LVII, Pt. III, 1933, 675–97) to which Dr. J. Weir has contributed a valuable palæontological appendix. Observations on Lower Cretaceous strata at Whales Bay afford support to Frebold's view of their stratigraphical relations rather than to Obruchev's.

H. C. Cooke has written a useful review of Pre-Cambrian stratigraphy on the Canadian Shield under the title "Land and Sea on the Canadian Shield in Pre-Cambrian time" (*Amer. Journ. Sci.*, XXVI, 1933, 428–441, 457–74).

An important memoir by P. B. King (*Bull. Geol. Soc. Amer.*, **45**, 1934, 697–798) describes the Permian stratigraphy of Trans-Pecos Texas, where the North American Permian attains its greatest development.

R. H. Palmer contributes an interesting summary of the geology of Habana, Cuba (*Journ. Geol*, XLII, 1934, 123–45). Habana is situated at the western end of an anticlinorium consisting of rocks ranging from Upper Cretaceous to Pleistocene. East of the harbour there are intrusions of serpentine and diorite.

S. Taber deals exhaustively with the geology of the Sierra Maestra of Cuba (*Bull. Geol. Soc. Amer.*, **45**, 1934, 567–620) as part of the investigation which is necessary to understand the structure and history of the great Bartlett Trough in the Caribbean Sea, of which it is the northern rim.

Dr. C. T Trechmann has discussed at length the uplift and the origin of the coral rock of Barbados (*Geol. Mag.*, LXX, 1933, 19–47). The same author continues his detailed work on the geology of West Indian islands with an account of the "Tertiary and Quaternary Beds of Tobago" (*ibid.*, LXXI, 1934, 481–93).

Dr. J. A. Douglas offers another important contribution to Peruvian geology with his paper on "The Geology of the Marcapata Valley in Eastern Peru, with an Appendix on the Graptolites from the Quitari Area" (*Quart. Journ. Geol. Soc.*, LXXXIX, 1933, 308–56). A great diversity of plutonic rocks in this region makes an interesting contrast to the uniform calcic granodiorites of the Western Cordillera.

In a further paper on "Der Südantillen Bogen," Professor O. Wilckens (*Geol. Rundsch.*, XXIV, 1933, 320–35) reinforces his former conclusion that the island-groups which connect Staaten Island and Graham Land by means of a vast eastwardly-directed loop are fragments of a continuation of the Patagonian Cordillera (*see* Science Progress, October 1933, 302).

In his instructive comparison of the Pre-Cambrian of Fennoscandia and South Africa, Dr. T. W. Gevers (*C.R. Soc. géol. de Finlande*, No 9, 1934, 35 pp.) provides a valuable comparative table of the sequences of Pre-Cambrian events in the type-regions of Europe, North America, and South Africa. In his opinion South-West Africa, owing to the great vertical and horizontal range of its exposures, bids fair to become another classical region of Pre-Cambrian research.

A. Holmes discusses the Gordonia uraninite in its bearing on the age and correlation of the upper Pre-Cambrian formations of southern Africa (*Amer. Journ. Sci.*, XXVII, 1934, 343–53).

In his Presidential Address to the South African Geographical Society Dr. A. L. du Toit discusses " Crustal Movement as a Factor in the Geographical Evolution of South Africa " (*S. Afr. Geogr. Journ.*, XVI, 1933, 3–20). He describes the principal ridges and hollows into which the crust in South Africa was bent during the period ranging from about the middle of the Tertiary down to the present time.

In a special memoir W. A. Macfadyen deals exhaustively with the geology of British Somaliland (London : Crown Agents for Colonies, 1933, pp. 1–87, 4 pls. and map).

J. B. Auden's memoir on " The Geology of the Krol Belt " (*Rec. Geol. Surv. India*, LXVII, Pt. 4, 1934, 357–454) is an excellent contribution to Himalayan geology. The use of current bedding to determine whether the strata are or are not " right-way-up " has been applied to this region by Mr. Auden with satisfactory results.

A memoir on " The Permian Formations of Southern China " by T. K. Huang (*Mem. Geol. Surv. China*, Ser. A., No. 10, 1932, 129 pp.) is appreciatively reviewed by C. Schuchert in *Amer. Journ. Sci.*, XXVI, 1933, 454.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

Ecology.—Much has been written regarding climatic differentiation of soils to show that the character of the mature condition is largely a function of climate. Since climate and soil together play so prominent a part in determining the character of vegetation, if the nature of the latter is mainly determined by the former, climate appears in a very dominant rôle in relation to the vegetation. This is, however, probably only true where the climatic conditions are of a very pronounced character. The influence of soil on vegetation in an area of approximately uniform climate is therefore very instructive and the study by Newton and Stobbe of the Flora of Quebec in relation to soil types (*Macdonald College Techn. Bull.*, 14, Aug. 1934) is of this character. Four types of soil are here recognised. The heavy clay soils which are rather acid in reaction (*p*H.5·5–5·9) have a moderately high loss on ignition (8–15 per cent.) and are consequently moist. The Brown forest soils tend to have a higher organic content and wider range of acidity (*p*H.4–6). From these two types of soil the largest number of species were recorded, *viz.* 259 spp. from the forest soils and 244 spp. from the heavy clay. The two other soil types present are sandy clay (140 spp.) and podsols (162 spp.), both with a reaction near *p*H.5, but the podsols are not only poor in mineral nutrients but also have a low organic content and are relatively dry. The podsols bear mixed coniferous forest, the other soils mixed deciduous woods. Abundance of *Fragaria virginiana* is a feature of the podsols whilst *Trifolium repens*, *Ranunculus acris* and *Taraxacum officinale* are characteristically more frequent on the clays. It appears then that here the geological factors play a greater part than climate in determining the nature of the vegetation whilst the differences in the latter are in part responsible for the nature of the mature soils.

It has been known for a considerable time that plants grown in culture solutions brought about an alteration in the reaction. This has been investigated more extensively by A. Koslowska for a number of wild species (*Jour. Ecology*, 396, 1934). Of these some, such as *Corydalis cava*, *Mercurialis perennis*, *Asperula odorata*, *Dentaria glandulosa* and less markedly *Allium ursinum* exhibit a striking capacity to render acid solutions more alkaline. *Vaccinium myrtillus* rendered slightly acid or alkaline solutions definitely acid whilst species such as *Anemone nemorosa* and *Luzula pilosa* caused solutions above *p*H.6 to become more acid and below to become more alkaline. Ubiquitous species were found to vary in their capacity to affect the reaction according to the soil from which the plants were obtained, but species of restricted habitat showed constant behaviour whatever the soil in which they were grown. Experiments indicated that the power of altering the reaction was correlated with the degree of buffering of the cell sap. Whilst these results are of considerable interest as showing one possible factor in reaction preferences, it must be emphasised that the power of altering the reaction of a culture solution, even when buffered, may not be in any degree applicable to the root system when surrounded by a soil strongly buffered not by one substance but perhaps many.

In the same Journal, B. L. T. de Silva shows that the occurrence of calcicoles on soils devoid of any detectable carbonates is associated with the presence of considerable quantities of exchangeable calcium. Evidence is afforded that calcicoles can tolerate a low proportion of exchangeable calcium when the acidity is low and that calcifuges can tolerate more exchangeable calcium when the acidity is high.

The same number of the *Journal of Ecology* also contains descriptions of the vegetation of Akapatok Island off the coast of Labrador by N. Polunin, of that of the Chiltern Hills by A. S. Watt and a contribution to the ecology of the Humber by R. Good and W. L. Waugh.

The vertical distribution of the root systems of Lucern, Wheat, Beans, Rice and Citrus have been investigated by E. S. West (*Jour. Council Sci. Indust. Res. Australia*, 87–93, 1934). In a loam soil the roots of *Medicago sativa* were found to penetrate to depths of 4½ metres. Wheat roots extended to about 3 metres, roots of Beans and Rice to 90 centimetres and those of Citrus to 1 metre. Ninety per cent. of the roots of the Rice were present in the top 20 centimetres and all five plants showed a rapid decrease of root development from above downwards

REPRODUCTIVE MORPHOLOGY AND BIOLOGY.—An investigation of various Rosaceous flowers, including those of *Rosa*, by G. Jackson (*Amer. Jour. Bot.*, **21**, 453, 1934) has yielded results confirmatory of the interpretation by G. Bonne, published privately in Paris in 1928. The floral tube is held to be appendicular in character due to lateral fusion of the sepals, petals and stamens. *Dalibarda repens* and *Rubus triflorus* furnish examples of the simple condition. In *Dryas* there is congenital fusion also between the perianth traces and adjacent staminal traces though some of the latter are free. In *Geum* all the stamen traces are joined to perianth traces for almost the entire length of the floral tube. In *Rosa* the tube is partly appendicular but in its lower part axial and, presumably as a result of intercalary development in this region, below the level of insertion of the carpellary traces, these turn downwards into the concave base so that transverse sections show an outer ring of normally orientated bundles, supplying the perianth and stamens, and an inner ring of bundles with inverted orientation which supply the carpels.

The embryo sac development and embryology of *Radiola linoides* has been studied by J. Mauritzou (*Svensk Bot. tid.*, **28**, 84, 1934). A multicellular archisporium of as many as four megaspore mother cells may be formed one of which develops and forms a linear tetrad and a normal eight-celled embryo-sac. In *Biophytum sensitivum* during development of the endosperm the embryo-sac encroaches upon the surrounding tissue by the formation of ridge-like haustorial outgrowths with a well-differentiated nutritive jacket. In the same Journal Dahlgren describes (p. 103) the embryology of *Impatiens Roylei*. In this species the primary endosperm nucleus divides into two followed by a division of the embryo-sac into two cells. Of these latter the two lower exhibit free cell formation and the upper subsequently divides into three of which the uppermost, containing the egg, puts out a giant branched haustorium penetrating into the funicular tissue.

Details of the floral biology of Sorghum are furnished by J. C. Stephens and J. R. Quinby (*Jour. Agr. Res.*, 123, 1934). It is estimated that a single flower will produce some 15,000 pollen grains and that a large panicle might form over 100 million. The stigmas are receptive for from ten to eighteen days but pollen appears to retain its capacity for effecting fertilisation not more than five hours after being shed. The interval between pollination and fertilisation appears to be from six to twelve hours. Flowering takes place at night and the glumes remain fully open about twenty minutes.

A. C. Joshi and V. V. Raman Rao report that in the Menispermaceous *Tinospora cordifolia* seeds which are apparently quite normal develop without either fertilisation or pollination. These seeds contain no embryo but an endosperm is developed. A normal eight-nucleated embryo-sac is formed (*Current Science*, III, 62, 1934). In the same Journal (p. 97) Dr. P. Maheshwari dealing with the Indian Mango describes the abortion in the inflorescences. Although an inflorescence of *Magnifera indica* may contain from 1,500 to 4,000 flowers only two or three of these will produce fruits. Most of the flowers are only functionally male and of the five stamens which are normally present one, or rarely two, are fertile and produce pollen. Occasionally primordia of ten stamens may be present recalling the condition in *Anacardium*. Though one carpel only is normally present two carpels are sometimes observed.

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology—A. Hartzell (*Contrib. Boyce Thompson Instit.*, **6**, 1934, 211–23) has been studying the histopathology of insect nerve lesions caused by insecticides. Nerve lesions have been found in mealworm larvæ and adult grasshoppers killed by pyrethrum applied externally. The author concludes that death is caused by the destruction of the cells of the central nervous system accompanied by paralysis. No comparable destruction of nerve tissue was found in either of the above-mentioned insects when killed by rotenone.

K. Mansour and J. J. Mansour-Bek (*J. Exp. Biol.*, **11**, 1934, 243–56) claim that true wood-feeding insects do not depend on micro-organisms for the digestion of wood. They divide such insects into those without cellulose-breaking enzymes and those with such enzymes. The former derive the necessary carbohydrates from the soluble sugars and starch in the wood on which they live, while the latter utilise the cellulose of the wood through the activities of their own secretions. Further, the authors show that wood-eating insects, such as termites and Lamellicorn larvæ, with free-living intestinal organisms use these latter as direct food and derive no digestive help from them. The same authors have reviewed the subject of the digestion of wood by insects and the supposed rôle of micro-organisms (*Biol. Rev.*, **9**, 1934, 365–82).

M. Aschner (*Parasitology*, **26**, 1934, 309–14) has demonstrated that lice freed of their symbionts show that the symbionts play an essential part in the life of the louse, so that without them death of the larvæ results.

The habits and characteristics of nocturnal animals are discussed by S. C Crawford (*Qtly. Rev. Biol.*, **9**, 1934, 201–14).

An apparatus in which a steady temperature gradient can be maintained and in which conditions of temperature and humidity can be accurately measured is described by D. L. Gunn (*Zeits. f. vergl. Physiol.*, **20**, 1934, 617–25) in a study of the temperature preference of the cockroach, *Blatta orientalis*. In this connection a paper by P. A. Buxton and K. Mellanby (*Bull. Ent. Res.*, **25**, 1934, 171–5) deals with certain recent improvements in the technique of the measurement and control of humidity.

Coleoptera.—The need for thought in choosing methods for collecting in order to obtain a reasonably adequate idea of the fauna of a locality is well exemplified by the results of J. Omer-Cooper and C. E. Tottenham (*Ent. Mo. Mag.*, **70**, 1934, 231–4). By sweeping the air with a net for six hours in Wicken Fen, they captured 7,000 beetles, of which 60 per cent. of the individuals belonged to species new to this district. Eighty-one species were caught of which twenty-five were new to the list.

K. Mansour (*Bull. Soc. Roy. Ent. d'Égypte for 1933*, 1934, 190–203) recalls the fact that there are two types of adult mid-gut development in Coleoptera and urges that this should be of use by systematists in establishing well-defined family series. In the first type, which has been long known, the mid-gut epithelium develops from replacement cells scattered in between the bases of the epithelial cells of the larva; in the second type it develops from the end of the stomodæum during metamorphosis. No intermediate stages have been observed. Lists are given of various beetles which have one or other of these two types of development.

In an article on the flour beetle, *Tribolium confusum*, T. Park

(*Qtly. Rev. Biol.*, **9**, 1934, 36–53) points out the desirable features of this insect for many lines of research and not only for population studies. He also gives much information about its general biology.

The soil reaction and moisture content requirements for Elaterid larvæ have been dealt with by W. Subklew (*Zeits. f. wiss. Biol.*, **28**, 1934, 184–228). J. N. Oldham (*J. Helmin*, **11**, 1933, 119–36) has described in some detail the habits of a new nematode parasite of flea beetles.

LEPIDOPTERA.—The second paper by Maud J. Norris (Mrs. O. W. Richards) on insect fertility (*Proc. Zool. Soc. London*, 1933, 903–34) deals with experiments on the factors influencing fertility in *Ephestia kühniella* Z. Her previous paper (loc. cit., 1932, 595–611) was concerning the morphology and function of this moth's reproductive organs In her third paper (loc. cit., 1934, 333–60) experiments made to determine the importance of nutrition in the lives of the adult moths are described. For example, the fecundity, as measured by the number of eggs laid, and the longevity of *E. cautella* Wlk. and *E. elutella* Hb. are about halved if they are deprived of drinking water, but *E. kühniella* Z. is only slightly affected by it. Similarly, sugar-feeding further increased the longevity in two species but did not increase the fecundity any longer than water. A valuable part of the paper is the discussion in which the literature concerning the feeding habits and requirements of adult Lepidoptera and also insects in general is reviewed. It is shown that whereas a sugar diet suffices to maintain life, most adult insects need proteins of some kind for the normal requirements of their genital products and in many insects oviposition is entirely conditional upon the taking of such food. The adult Heteroneura have lost the need for nitrogenous foodstuffs owing to a series of modifications by which their requirements have come to be met by the food stored in the fat-body. The author has drawn up a table showing the probable evolutionary trends which have resulted in the reduction of mouthparts and the state in which no food at all is taken.

H. R. Hewer has followed his description of the genitalia in *Zygœna* (*Proc. Zool. Soc. London*, 1932, 33–75) by a paper on the mechanism of copulation and the passage of the sperm in the female (loc. cit., 1934, 513–27). There appears to be no likelihood of any form of mechanical inability to copulate among closely related species in this genus; but in the last stage of the life of the sperm there is the possibility of incompatibility between the sperm and the secretion of the glandular part of the receptaculum seminalis.

Observations on the natural control of the cabbage caterpillars (*Pieris* spp.), their biology and the interrelationships of the parasites have led J. E. Moss (*J. Animal Ecology*, **2**, 1933, 210–31) to the view that the introduction of the insect enemies of *P. rapæ* into New Zealand ought to be of real benefit in checking the ravages of this recently imported pest.

The attraction of some South American moths to light, human perspiration and damp sand has been compared by C. L. Collenette (*Entomologist*, **67**, 1934, 81–4).

HEMIPTERA.—V. B. Wigglesworth and J. D. Gillett (*J. Exp. Biol.*, **11**, 1934, 120–39) have shown that the olfactory and thermal senses used by *Rhodnius* in reaching its host reside chiefly in the antennæ which also perceive air currents and contact. Vision and vibration seem to be accessory factors. One of the authors' conclusions was based on false premises, but this conclusion has been later confirmed by further experiments (loc. cit., **11**, 1934, 408).

C. G. Johnson (*Trans. Soc. Brit. Ent.*, **1**, 1934, 1–32) has described the structure of the egg and the sub-opercular yolk-plug, the swelling of the egg and the hatching of *Notostira erratica* L. (Capsidæ).

The last and posthumous report of I. Kuwana concerning Japanese Diaspine Coccidæ has appeared (*Min. Agric. and Forestry, Japan*, Sci. Bull. 3, 1933, 51 pp.). It contains the seventh of the series of his studies, a key to the genera of Diaspinæ in Japan, and an index to the species recorded in the whole series of papers.

After investigating the density of population and sterility of the females in the Coccid *Lepidosaphes ulmi* L., E. Smirnov and W. Polejaeff (*J. Animal Ecology*, **3**, 1934, 29–40) have come to the conclusion that density of population may be considered as a factor regulating an excessive reproduction. A struggle for space and food is the probable cause of sterility under conditions of dense population.

A detailed study of the external anatomy of the red date scale *Phœnicococcus marlatti* Cockerell and its allies has been made by F. S. Stickney (*U.S. Dept. Agric.*, Tech. Bull. 404, 1934, 162 pp.).

HYMENOPTERA.—G. E. J. Nixon (*Ent. Mo. Mag.*, **70**, 1934, 87–9) has recorded the successful adoption of an orphaned brood of *Vespa vulgaris* L. by a queen of *V. germanica*.

B. B. Fulton (*Ann. Ent. Soc. America*, **26**, 1933, 536–53) describes the remarkable manner in which the eggs of the Chalcid *Habrocytus cerealella* Ashmead, a parasite of the Angoumois moth, are deposited. The width of the egg at its greatest diameter is at least twelve times the internal diameter of the ovipositor through which it passes without causing any appreciable swelling. The egg passes through the ovipositor like sand through an hour-glass, one end emerging below before the remainder has left the body.

In a paper describing the effects of refrigeration on the biology of *Trichogramma* by J. C. Schread and P. Garman (*J. New York Ent. Soc.*, **42**, 1934, 263–84), it is interesting to read that wing deformity, which may be taken to indicate a general weakening of the individuals, is directly proportional to the length of refrigeration. In addition, in the males it is greatly in excess of that of the females in both species studied. H. O. Lund, in a study of the temperature and humidity relations of two races of *Trichogramma minutum* Riley (*Ann. Ent. Soc. America*, **27**, 1934, 324–40) has succeeded in demonstrating under laboratory conditions a differential effect of physical factors upon host and parasite. This is a phenomenon that must be important in nature. The factors producing death of the parasites are not necessarily identical with those killing the host eggs, since the mortality of the parasitised eggs is much more variable with temperature and humidity than that of unparasitised

The natural development of a colony of *Aphelinus mali* has been studied by W. R. Thompson (*Parasitology*, **26**, 1934, 449–53). It was found that the actual percentage of parasitism was a good deal higher than the theoretical one. This suggested that the parasites had some tendency to avoid ovipositing in parasitised hosts. Salt (*Proc. Roy. Soc.*, B, **114**, 1934, 455–76) had previously shown this to be the case in *Trichogramma*.

Among other papers on parasites the following may be mentioned : one on the biology of *Anagrus atomus* (L.) Hal., a Mymarid parasite of the eggs of the leaf hopper, *Erythroneura pallidifrons* Edwards, by Elsie I. MacGill (*Parasitology*, **26**, 1934, 57–63) ; one on some Hymenopterous and a Dipterous parasite of Psyllidæ, by K. B. Lal (loc. cit., **26**, 1934, 325–34) ; another on the larval anatomy of certain larval parasites, both Hymenopterous and Dipterous, of *Cydia pomonella* L., by H. T. Rosenberg (*Bull. Ent. Res.*, **25**, 1934, 201–56) ; and another dealing with the Chalcid *Pirene graminea* Hal. and the Scelionids *Inostemma boscii* Tur. and *Sactogaster pisi* Först. by H. Kutter (*Mitteil. d. Schweizer Ent. Ges.*, **16**, 1934, 1–81). The Serphoid and Chalcidoid parasites of the Hessian Fly are the subject of a publication by A. B. Gahan (*U.S. Dept. Agric.*, Misc. Pub. 174, 1933, 147 pp.). Two lists of parasites of Lepidoptera have been published recently, one by C. Morley (*Trans. Roy. Ent. Soc. London*, **81**, 1933, 133–83), the other by J. V. Schaffner and C. L. Griswold (*U.S. Dept. Agric.*, Misc. Pub. 188, 1934, 160 pp.). The former deals with British Lepidoptera, the latter with Macrolepidoptera reared from field collecting in the north-eastern part of the U.S.A.

DIPTERA.—B. De Meillon (*Publ. South African Instit. Medic.*

Res., **6**, 1934, 195–248) in a study of *Anopheles funestus* and *A. gambiæ* has shown that the severity and distribution of malaria in the Transvaal is dependent on that of the insect vector and further the distribution of the insect is a function of the climate. *A. funestus* breeds in permanent streams whereas *A. gambiæ* prefers small pools of a temporary nature. The former therefore has a stable population and this is reflected by the intensity of malaria in regions where this species predominates and also by the absence of epidemics. This species shows a seasonal fluctuation which is entirely dependent on temperature. The latter species, *A. gambiæ*, on the other hand, shows in addition intra-seasonal fluctuations and is associated with epidemic malaria.

The biology of the black fly *Simulium armatum* Mg. has been investigated by J. Smart (*Proc. R. Phys. Soc. Edinb.*, **22**, 1934, 217–38) and includes a description of an apparatus successfully used for rearing *Simuliids* from eggs to adult. Work on the embryology of *Simulium* by Mrs. F. L. Gambrell has shown that it is similar to that of *Chironomus* (*Ann. Ent. Soc. America*, **26**, 1933, 641–71). Differences include the facts that the embryonic membranes form more rapidly in *Simulium* and the embryo makes but one rotation. This is the first embryological study on *Simulium* since 1866. The origin of the peritrophic membrane is also mentioned.

The origin of this membrane has also been studied by F. H. Butt (*Psyche*, **41**, 1934, 51–6) in *Sciara* and the honey bee. In *Sciara* this membrane appears to be a continuation of the stomodæal membrane and is ectodermal in structure. It arises from a group of large secretory cells lying around the œsophageal valve and attached to the upper edge of the mid-intestine.

V. C. Robinson (*Parasitology*, **25**, 1933, 285–95) has shown that the metamorphosis of *Sciara pullula* Winn. is delayed by the parasitism of a nematode, *Aproctonema entomophagum* Keilin, 1917. The consequence of this is that at least two generations of nematodes may be found in one host.

The structure, biology and systematic position of *Pantophthalmus tabaninus* have been studied by W. H. Thorpe (*Trans. Roy. Ent. Soc. London*, **82**, 1934, 5–22). This fly is one of the largest Diptera known and among the largest living insects. The larva feeds mainly on fermenting sap which accumulates in its burrows, and only a small proportion of the wood itself seems to be ingested. It is unique among the known Brachycera in the absence of true mandibles, the maxillæ being enormously developed. It also has tracheal gills.

A. E. Cameron (*Trans. Roy Soc. Edinburgh*, **58**, 1934, 211–50) has worked out the life history and structure of *Hæmatopota pluvialis* L. This is the first European Tabanid and the first species of this genus the life history of which has been traced from egg to adult. The flies are shown to have the habit of intermittent blood-feeding and periodic oviposition. This of course provides the opportunity for the development of parasitic micro-organisms in the internal organs of the fly and so biological transmission from an infested to healthy host becomes possible. *Trypanosoma theileri* of cattle has already been associated with a crithidial flagellate in the hind-gut of the cleg.

Mention is made of the successful use of dried silkworm pupæ as an attractant in traps for catching *Hylemyia cilicrura* Rondani in a study of this fly by C. Harukawa, R. Takato and S. Kumashiro (*Ber. Ohara Instituts f. Landwirts. Forsch. in Kuraschiki*, **5**, 1933, 457–78).

G. S. Graham-Smith has followed his detailed description of the structure and function of the proboscis in *Calliphora erythrocephala* (*Parasitology*, **22**, 1930, 47–115) by a paper which deals with the musculature of the alimentary canal from the pharynx onwards, and the structure and function of the proventriculus, Malpighian ducts, rectal valves and rectal papillæ (loc. cit., **26**, 1934, 176–248).

W. R. Thompson (*Parasitology*, **26**, 1934, 378–448) has given a comprehensive account of the larval morphology and biology of the Tachinid parasites of woodlice. Seven such parasites have been reared and identified as well as two undetermined species. *Porcellio scaber* L., *Oniscus asellus* L., *Metaponorthus pruinosus* Brandt. and *Armadillidium vulgare* Lat. were found to be attacked by one or more of the parasites.

Further observations, including studies of the tropisms, on the biology of the pigeon fly *Pseudolynchia maura* Bagot, have been made by Margaret J. Prouty and G. R. Coatney (*Parasitology*, **26**, 1934, 249–58).

OTHER ORDERS —The oogenesis of caddis flies has received little attention in the past. Recently R. A. R. Gresson (*Proc. Roy. Soc. Edinburgh*, **53**, 1933, 322–46) has studied this process in *Stenophylax stellatus* Curt. His findings concerning the growth and history of the ovarian elements agree fairly closely with Marshall's previous work (1907) on *Platyphylax designatus*.

Usually in Ephemerid nymphs where the gills move rapidly in metachronal rhythm, both members of each pair of gills beat together and their movements are co-phasedly synchronized. Thus there is an axial flow in the surrounding medium. L. E. S. Eastham (*Proc. Roy. Soc.*, B, **115**, 1934, 30–48) describes an exception to this rule in *Cœnis horaria.* In this insect the currents pass over the body from one side to the other. The gills beat in metachronal rhythm down each side of the body, but though the rhythms are synchronous there is a time phase difference between them.

J. W. Evans (*J. Counc. Sci. and Ind. Res.* (Australia), **7**, 1934, 61–9) has kept daily records of the thrips present in roses for two consecutive years. The accumulated data shows the close correlation between the autumn and spring thrips populations, and demonstrates how the increase and decrease of the population is directly dependent upon meteorological conditions. In this paper also it is shown how the incidence and amount of rainfall affects the population. In a previous paper (loc. cit., **6**, 1933, 145–59) the same author put forward the following hypothesis. That the number of individuals in the first spring generation determined the possibilities of a subsequent infestation and that the numbers present in this spring generation were dependent especially on the meteorological conditions of the previous four months. The data now brought forward does much to substantiate this hypothesis.

A complete account by J. Davidson of the Lucerne Flea, *Smynthurus viridis* L., in Australia and its reaction to different environments has now appeared (*Counc. Sci. and Ind. Res.* (Australia), Bull. 79, 1934, 66 pp.). W. M. Davies has started publishing a list of the Collembola of North Wales. The first contribution (*N. W. Naturalist*, **9**, 1934, 115–24) deals with the soil fauna of the mountain zone. Thirty distinct species are noted, six of which are new to the British list.

The development and homologies of the mouth-parts of the head louse have been re-investigated by W. Fernando (*Q.J.M.S.*, **76**, 1933, 231–41). This work shows that Cholodkovsky's interpretations of the homologies in the mouth-parts of *Pediculus* are incorrect, and it is in general agreement with Enderlein's views on the subject.

ARCHÆOLOGY. By E. N. Fallaize, B.A.

The Mesolithic Age.—Indications are not wanting that the increased attention which has been given recently by archæologists to the relations, typological and distributional, of the cultures of the Middle and early New Stone Ages may be expected to bear fruit in a more precise understanding of their arrangement and classification in chronological and developmental succession. Two recent contributions to the archæology of the British Isles, one in England and the other in Ireland, represent an important advance in this direction, while, in addition, the latter, if certain tentative conclusions are confirmed by subsequent investigation, may serve to throw light upon continental problems in the period of pre- and early-neolithic cultures by providing evidence in stratigraphy which is at present lacking. It is also interesting to note that in the English find of a mesolithic site at Broxbourne conclusions from typological evidence are fully corroborated by the results of independent pollen analysis, thus affording a substantial endorsement of the application to conditions in Britain of the results of the pollen analyses from north-central Europe which have been employed as a chronological standard.

The study of mesolithic cultures was given a fresh orientation by the publication in 1931 of an important paper by Professor V. Gordon Childe on the mesolithic Forest Cultures in the *Journal of the Royal Anthropological Institute*, vol. LXI, of which the implications as affecting Great Britain were brought out by Dr. J. G. D. Clark in *The Mesolithic Age in Britain* (1932). Dr. Clark showed that the distinctive mesolithic culture of south-east England was to be regarded as an extension of Professor Childe's Forest Cultures. A connection between the cultures of the Baltic and Britain had already been indicated by the discovery of harpoons of the Kunda type at Skipsea and Hornsea in Holderness and this is supported, as is now recognised, by the discovery of other harpoons of the same cycle.

Now Professor Childe has shown, in the paper to which reference has been made, that throughout Boreal time, and possibly extending back to pre-Boreal time, a generalised culture, equipped with a "heavy industry," of which Maglemose, Sjælland and Duvensee, near Lubeck, are to be regarded as special variations, extended probably from England to Finland. This tradition, he points out, persisted without break throughout Atlantic times, the kitchen-midden culture being a specialised variant adapted to a special form of food-supply, and, he goes on, "provided indeed in sub-Boreal time the background for the Neolithic cord-impressed pottery culture of the region."

For convenience of comparison Dr. Clark divides the span of this tradition into three stages, corresponding roughly to the climatic periods distinguished by pollen analysis. These are :—

Period A, correlating with the Boreal climatic period ;
Period B, correlating with the greater part of the Atlantic climatic period ; and
Period C, overlapping with the last phase of the Atlantic and the beginning of the sub-Boreal climatic period.

Culturally Period A includes Duvensee, Maglemose and probably Kunda ; B correlates with the kitchen-middens of the Littorina coasts; and C with the neolithic of the round-based pots, ornamented with cord impressions, comb marks and pits, with which Childe has related the "Peterborough" culture of Britain, as a westward extension of cord-impressed pottery cultures extending from the Baltic to the Black Sea. This in Britain, then, is Period C of the Forest Cultures; but of Period B there is no evidence, so far as connection with the Baltic area is concerned: its place is taken by what is to be regarded as a local development of Period A culture. This is represented by the Lower Halstow culture—assigned by Erdtman to the Atlantic climatic period—and other apparently related cultures in Sussex, such as Peacehaven, West Heath, etc., even though these show evidence of other contacts. For Period A there are sites such as, probably, Kelling and Thatcham; but Kelling is an open air site, while Thatcham was discovered before the technique of pollen analysis had been developed. The discovery of a sealed deposit of mesolithic age at Broxbourne is, therefore, of the first importance. It becomes the type site in Britain for the Forest Culture of Period A.

Broxbourne.—The site of the discovery is known as Rikof's Pit. It lies on the Lea marshes a few hundred yards to the east of Broxbourne railway station. The mesolithic implements were brought to the attention of Mr. Hazzledine Warren, who, however, did not see any *in situ* until May, 1932, when he discovered a small undisturbed floor, of which the principal area was not more than 15 ft. in diameter, while 10 to 15 ft. outside that area the industry disappeared. The floor was of leached sand, overlying gravel, and was itself overlain by peat. The underlying deposits belong to the Tundra stage that may be correlated with the Magdalenian epoch, while the overlying peat belongs to the Boreal Forest stage. Not only was the deposit undisturbed but the peat had protected it from later additions. The industry may, therefore, be regarded as being a sealed deposit. Mr. Hazzledine Warren describes the site and its geological conditions in *Journ. R. Anthrop. Inst.*, vol. LXIV (1934), and his account is followed by a description of the industry and a discussion of its affinities by Dr. J. G. D. Clark. Typologically, Dr. Clark regards the implements as of first-rate significance, and the site as one of the key mesolithic sites of Britain. The most numerous objects were flint flakes showing no sign of secondary work or use. They represent the result of knapping and indicate that the work was done on the spot. Some of the flints showed the effect of fire. There were a number of different forms of microliths, but the most numerous were the micro-burin, a by-product of the manufacture of microliths. Two chipped flint axes were of special importance. They are true core axes and one of them is comparable with the best of the Duvensee axes.

In a separate communication Dr. H. and Mrs. M. E. Godden report on a pollen analysis of material from the site and as a result assign the industry to Boreal time. Recent pollen investigation of the Skipsea peat and the "moorlog" associated with the North Sea harpoon, also dates the harpoons and, therefore, the Forest A culture of Britain to Boreal time. Broxbourne is thus the first complete industry of this tradition in Britain assignable to the Boreal period on the evidence of pollen analysis. The continental sites with which Broxbourne is most closely to be compared are Sværdborg, Mullerup, Holmgaard in Sjælland, and Duvensee near Lubeck, all of which have been dated on botanical evidence to the Boreal climatic period.

PRE-CAMPIGNIAN AND CAMPIGNIAN IN NORTH-EAST IRELAND.—Over thirty years ago the late W. J. Knowles, the well-known Irish archæologist and collector of Stone Age implements, pointed out the remarkable and somewhat anomalous character of certain of the early types of neolithic implements in northern Ireland; and Mr. C. Blake Whelan, who has given intense and extended study to the Stone Age industries of this area, has pointed to the possibility that as a peripheral area this part of Ireland may have afforded favourable opportunity for the survival of archaic types. Now, still having this peripheral character in mind, he has turned his attention to the question of "pre-Campignian" industries, and proposes to study intensively a number of sites on which, he is of the opinion, it will be possible to obtain evidence that, in the light of comparative study, may offer a solution of the problem of the affinities of the cultures which lead up to the Campignian industry and have provided Continental archæologists with some of the more obscure of their problems in later Stone Age affinities. Their difficulties arise, he has pointed out, from a lack of stratigraphic evidence which, he holds, Ireland may be able to supply.

In a study of the Campignian industry in north-east Ireland recently presented to the Royal Irish Academy (*Proceedings*, vol. XLII, Section C, No. 7), Mr. Whelan points out that there are in Ulster a number of parallel culture groups, phylogenetically distinct, developing in isolation, and interacting only in their latest stages. What is termed by Mr. Whelan "pre-Bann" is provisionally assigned to the late mesolithic in Ireland from its apparent association with a peat of Boreal-Atlantic age. From the bed of the lower Bann comes a series of microlithic retouched blades of evolved Creswellian facies, while the industry of the neighbouring submerged peat (Lough Neagh) appears to represent a forest culture of debased Upper Palæolithic tradition, as yet uninfluenced by Campignian immigration. Upon this early culture the great river ford civilisation of the Bann, with its admirable lithic industry and Scandinavian affinities, evidently followed. It cannot be older than the late Littorina climatic phase, during which the coastal raised beaches were laid down. The ultimate appearance of Campignian types in the Lower Bann, in company with tanged points for fishing forks and certain great stone axes, hitherto unpublished, suggest that the river ford fishing folk laid under contribution the Campignian culture of the coastal uplands; but this point is reserved for fuller investigation later, in co-operation with the Archæological Expedition to Ireland of Harvard University.

In the meantime Mr. Whelan, here also assisted by members of the Expedition, has been engaged in the examination of a Campignian Stone Age site at Ballynagard, Rathlin Island, Co. Antrim. The lithic material consists of both flint and a bluish fine-grained rock. In general terms the implements conform closely to Campigny in type and workmanship, while the pottery has been pronounced to be " Windmill Hill," thus bringing the site within the western European cultural complex. A second example of pottery from a ploughed area near one end of the sounding trench is pronounced to be " Peterborough." It would thus appear that Mr. Whelan has established his upland Campignian culture as the *terminus ad quem* to which further chronological and cultural research is to be directed.

THE STONE AGES OF NORTH AFRICA.—In reporting on the examination of the Abri Alain, near Oran, Algeria, M. Paul Pallary discusses the sequence of Stone Age cultures in North Africa (*Arch. de l'Institut de Paléontologie humaine, Mém.* 12). His views may be compared with those of M. R. Vaufrey (see SCIENCE PROGRESS, XXIX, 136–7). The site upon which the author has spent several seasons, and of which the excavation is not yet completed, had long been known as one of a number of shelters situated in ravines in the neighbourhood of Oran which had once been the abode of man. Microliths were discovered here by the author in 1906 and the shelter was photographed in 1907. Subsequently, however, it was destroyed by quarrying, but in 1927 an examination of the site was begun at the suggestion of M. Boule. Fortunately the author discovered a part of the original floor, which had not been disturbed and allowed a view of the original stratification of the deposits. This has afforded a key to interpreting the material found on the disturbed portions of the site. Of the four strata of deposits, the two lower are of a yellow earth, while the two upper are black and chocolate-brown respectively. The last-named is sealed with a calcareous deposit. The black, or second from the top, appears from the description to contain broken shells in such abundance as almost to resemble a shell-heap. In the microlithic industry which is characteristic of the site, little difference is to be observed between the yellow and the black strata of deposit.

In his discussion of the succession of palæolithic cultures in North Africa, M. Pallary agrees with the generally accepted view that the most ancient is the industry characterised by the great *coups de poing* of limestone, such as that found at Palikao, associated with the larger mammalia, to which must now be added the cave lion, recently discovered by the author. This was followed by an Acheulean fully comparable to that of Europe ; but the Aterian of Reygasse is not accepted as a separate industry to be distinguished from the Mousterian. Nor is the true Solutrean, as claimed by Reygasse, admitted. The industries following on the Mousterian are held to be the Getulian, extending from Mousterian to neolithic in the east and the Ibero-Maurusian in the west. In preferring the title Getulian to Capsian for the eastern industry, the author claims priority over de Morgan by whom the Capsian was so named, as well as greater conformity to scientific practice. The Ibero-Maurusian industry of the west, it is maintained, is to be clearly distinguished from the Getulian. Its principal sites are la Mouillah, Taza and Oran, though there are many others. But the point on which the author considers he is furthest removed from his archæological colleagues is in distinguishing a *berbéresque* industry, differentiated by pedunculate implements, which he considers to be neolithic, but which his colleagues regard as Mousterian. He bases this view on the fact that the pedunculate implement is never found in the lower levels of the caves, but only on the surface of the shell-heaps. In this view, he holds, he is confirmed by his recent investigations at the Abri Alain.

ARCHÆOLOGY IN THE NEAR EAST: LACHISH.—Among the reports on excavations in the Near and Middle East during the past season which have appeared in any detail, Tell Duweir (Lachish) has provided what is, perhaps, the most sensational find in the ewer with a painted inscription in a script which has still to be deciphered. Epigraphists, while differing in their readings, are as one in regarding it as of the first importance. The Wellcome Research Expedition has now completed two seasons' work on this site and at the time of writing is entering upon a third.

Judging by the results which have been obtained to date the site promises to be of great interest and importance for the prehistory of Palestine. In the early Metal Age the settlement was a centre of economic life of a size as yet unparalleled in the Shephelah. The Early Copper Age site, it has now been established, covers no less than 150 acres and includes the remains of a large dolmen. On a limestone ridge across the western valley flanking the side of the Tell, habitation caves of the early Bronze Age have been found. Some of them had been artificially enlarged. Many were used subsequently as burial-places. A gold bead found here is unique for this early period (pre- and early-dynastic of Egypt). Below on the side of the ridge was a cemetery, representing a later stage of the same culture. The normal type of tomb is an oval chamber approached by a narrow shaft. The burial was tightly contracted, and the dead were equipped with dagger or dart, a tall ovoid water jar, a squat two-handled food-vessel, and sometimes an open bowl. This cemetery equates with the Middle Old Kingdom in Egypt.

A remarkable find was a temple or sanctuary outside the city wall, which had been destroyed by fire not later than 1262 B.C. The date of its foundation has not yet been ascertained. It gives a remarkably complete view of the character of this type of place of worship and of the character of the ritual such as had not previously been attainable. Among the large number of objects of various kinds obtained here were a number of carved ivories, much calcined by fire, but preserving their artistic character and quality intact. The carvings were reminiscent of Tell el Amarna, and one of the objects, a perfume container, is figured in an Egyptian relief; but the art nevertheless is not Egyptian. It has been suggested that the carvings are the product of a localised school of art, of which a much later phase is, perhaps, to be seen in the ivory carvings of Samaria of the time of Ahab. A preliminary report by Mr. J. L. Starkey, the field director, has been published by the Palestine Exploration Fund, and brief reports appear in *Man* for July and *Discovery* for August.

NOTES

Soil Microbiology in the U.S.S.R. (H. N.)

This is the title of a paper-covered volume of 166 octavo pages published as the Soviet Section of the *Trans Third Comm. Int. Soc. Soil Sci.* Although dated 1933, the volume was not received at Rothamsted until October 1934. Six papers are comprised in the volume; of these, "Soil Microbiology in the U.S.S.R. (1917–1932)," by Professor E. E. Uspensky (Moscow); "Microbiological Evaluation of the Soil's requirement in Fertilizers," by Uspensky and A. P. Kriutchkova; and a review of work on root nodule bacteria at the Institute of Agricultural Microbiology, Leningrad, by M. P. Korsakova, are of most general interest. The first-mentioned, and longest, paper, is indeed challenging; it contains matter of considerable interest to scientists in many fields of work, and represents a substantial contribution to our knowledge of bacterial processes. Though written historically, Uspensky's paper accords prominence to very recent work concerning which little has been heard of outside Russia. A list of 337 papers, nearly all in Russian (translated titles are given), is appended, and few of these are older than 1925.

The isolation of Soviet science has resulted in independent work of which two examples, based upon this volume, follow. S. Winogradsky's discovery, in France, of the production of ammonia by the aerobic, free-living, nitrogen-fixing bacterium *Azotobacter*, was anticipated by several years by S. P. Kostychev *et al.*, working in Russia. H. G. Thornton and P. H. H. Gray at Rothamsted, and S. A. Korolev in Russia, independently and apparently simultaneously devised ratio methods for estimating the total numbers of bacteria in soil: the Rothamsted workers mixed indigo particles, and Korolev, yeast, with the soil, to avoid the almost insuperable difficulties presented by attempts to measure the weight or volume of the microscopic mass of soil examined. Both techniques are analogous to Almroth Wright's well-known clinical method of counting bacteria in presence of hæmocytes.

Russia appears to be the only country in which the "direct" (microscopic) method of estimating numbers of soil bacteria is normally used. The Russian technique may not always be Korolev's, and is possibly the more arbitrary one of T. N. Germanov, but it is noteworthy that Russian investigators correctly report bacterial numbers in soil in milliards and not in the millions recorded by workers with the plating method almost exclusively employed hitherto outside Russia. The only description of Germanov's method available to the writer is unintelligible, owing, probably, to the omission of an essential detail, while Korolev's method is given only in an obscure journal, not complete in any British library.

Processes of soil decomposition of paraffins and ring hydrocarbons are being actively studied by Russian workers headed by V. O. Tausson, and some advances have been made. It has been indicated that micro-organisms can oxidise polymethylenes and condensed ring compounds such as anthracene. Oxidations in soil do not always take courses familiar to chemical students; thus, naphthalene does not give rise to phthalic acid or naphthol.

The practical bearing of present work is emphasised by Professor Uspensky, who makes some amusingly cutting remarks about pre-Revolutionary studies of nitrification. Recent studies of associations of *Azotobacter* with higher plants have shown that there is an apparent symbiosis of plant and bacteria in tobacco, but a connection is less evident in growing mustard. Existence of root secretions is postulated to account for this type of association, and also for the action of bacteria in reducing nitrates to nitrite in the neighbourhood of roots. Regarding the puzzling questions of denitrification in soil, new views have emerged, which, while not clearing up the problem, represent a real contribution to its understanding.

Some peculiar cases of non-response to ordinary fertilizers have been investigated by S. I. Kusnetzov. A soil rich in chalk and in total P, but poor in total N, gave little or no increase of cotton unless "potash" saltpetre was used: application of ammonium sulphate or urea had the effect of decreasing the available P (T. N. Germanov). Kusnetzov also established in Central Asia some striking relationships between irrigation rates and mineral-nitrogen dynamics.

A branch of work almost unknown outside Russia is the correlation of pedology and microbiology: bacterial tests have been devised for determining soil type. A more familiar application of microbiological analysis—to determine a soil's requirement in fertilizers by the aid of *Azotobacter*—has been highly developed in methods specified in the second paper.

Korsakova's work on leguminous plants is essentially a valuable confirmation of what is already, though not sufficiently widely, known. Her statement of the reasons for the failure of seed "inoculation" to benefit plants grown on rich soil is clearly put. Such knowledge as is now possessed by Russian and British workers could undoubtedly be used to help growers of legumes in parts of the Empire where perplexities have arisen. Some of the apparent anomalies of behaviour of legumes could be avoided, were available information to be applied.

Antimony as a Poison (M. S.)

Up to recent years the physiological effects of antimony compounds attracted little attention, the only outstanding fact being the emetic property of potassium antimonyl tartrate, commonly known as tartar emetic. In 1925 a Ministry of Health report made reference to the use of antimony oxide in place of the more expensive tin oxide as an opacifying agent in the preparation of enamels for cooking utensils, and to possibilities of toxic effects. That the fears expressed in this report were far from groundless has been proved by a remarkable series of outbreaks of illness since that date. At Newcastle in 1928 no less than 56 employees of a local firm suffered from violent sickness after drinking lemonade prepared from a powder containing 18 per cent. tartaric acid, the lemonade having been stored in white enamelled buckets. The enamel on these vessels contained 3 per cent. antimony oxide and by interaction of this with the tartaric acid there was formed a solution of tartar emetic of a concentration 50 per cent. higher than the medicinal dose for emetic purposes. At Folkestone in 1929 the danger was again in evidence, the enamelled jugs on this occasion being guaranteed to be "perfectly safe in use and free from any injurious substances." Yet more recently cases of antimony poisoning occurred in a London hospital, citric acid present in fresh lemons being the solvent in this case.

At first the obvious remedy appears to be to prohibit the use of antimony compounds in enamels, although this would handicap industry. Yet the problem is more complex than it appears, as is shown by a recent Report of the Ministry of Health (*Antimony in Enamelled Hollow-ware*, H.M. Stationery Office, 1934, 4*d.* net), in which Dr. Monier-Williams of the Ministry's Chemical staff has reviewed the whole subject of toxicity of antimony compounds. The first important point is that antimony, unlike its chemical neighbour arsenic, does not exhibit poisonous properties in all its compounds. For example, potassium pyroantimonate, a reagent well known in chemical laboratories for detecting sodium compounds, has been administered to animals without ill effects ; tartar emetic on the other hand has proved fatal when administered regularly. Brunner in 1912 and Scheller in 1926 both arrived at the conclusion that the pentoxide of antimony and indeed all compounds in which antimony possesses a valency of five are non-poisonous, whereas soluble compounds of trivalent antimony cause symptoms of poisoning, especially those compounds which are soluble in dilute acids.

The mode of preparation of antimony enamels has an important bearing on the tendency of the enamel to dissolve and yield toxic solutions. Antimony trioxide may be added to the initial mixture before fusing or " fritting " in a furnace, the product being then ground, made into a suspension with water and china clay, and the suspension used for coating the hollow-ware before the latter passes to the enamelling furnace ; or the oxide may be added while grinding, and being thereby subjected only to the lower temperature of the enamelling furnace. The latter method is usual in practice. Furthermore, the proportion of silica comes into the problem, for a low-silica enamel does not require a high temperature for " fritting " and is cheaper to produce ; this form should be prohibited for ware which is to be used for cooking purposes A high-silica enamel is more resistant to the action of solvents, but it is more expensive to produce since a higher temperature of " fritting " is required.

From the experimental data obtained by Dr. Monier-Williams and his staff, there are various recommendations which could be made for safety purposes. There is the drastic step mentioned above, which would prohibit the use of all enamels containing antimony for manufacturing kitchen utensils, and specifying enamels containing tin oxide at four times the price. In support of this step is the contention of certain workers in physiology, that traces of antimony compounds are always adsorbed no matter what type of antimony derivative was used in preparing the enamel. A second possibility is to specify the use of compounds of pentavalent antimony, or even to limit these to the compound sodium metantimonate which appears to yield the most resistant enamels. This limitation would be useless without some proof that reduction of the antimony compound will not occur in the fritting furnace, and without some official solubility test comparable to the tests in use for lead glazes (Germany has already proposed such tests for antimony). The third possibility, consisting in marking utensils which are safe for general use, would not be successful in practice with an apathetic public.

The Scottish Meeting of the International Pre-Cambrian Association (G. W. T.)

The International Pre-Cambrian Association (*Réunion Internationale pour l'étude du Précambrien et des vieilles chaînes de montagnes*) was founded by the late Professor J. J. Sederholm of Helsingfors. A most successful first meeting and excursion took place in Finland during part of July and August 1931, at which Sederholm was elected president. It was then decided to establish the headquarters of the Association in Finland, and to hold triennial meetings and excursions, the next to take place in Scotland. A Scottish meeting was accordingly held between August 18 and September 2 of this year under the leadership of Professor E. B. Bailey (Glasgow), and was attended by 26 members representing 13 nationalities. Starting from Glasgow by motor-bus the party carried out a fortnight's exploration of the Highlands during which attention was concentrated mainly on the Dalradian formations of the Grampians, the permeation complexes of the Moine Series in Sutherland, and the overthrusts of the classic region of the north-west. A business meeting held at Lochinver on August 29 debated the aims and constitution of the Association. It was decided that its main function was to organise excursions to Pre-Cambrian regions, each excursion to fall within the year immediately following a meeting of the International Geological Congress, the president to be elected from the country in which the excursion is to be held. Thus the next meeting of the Association is to be held in Czecho-Slovakia in 1937 under the presidency of Professor R. Kettner of Praha. A full report of the Scottish excursion, and an account of the aims and constitution of the Association will be published shortly in the *Geological Magazine.*

Longitude Observations in Jan Mayen and Continental Drift (G. W. T.)

Last quarter an account of longitude observations in Greenland in their bearing on continental drift was published in these Notes (Oct. 1934, 326). Similar observations in Jan Mayen, an island in N. Lat. 71° to the north of Iceland, have recently been made by H. Tollner ("Astronomische Ortsbestimmungen auf Jan Mayen. Zur Frage der Kontinentverschiebungen A. Wegener's." Ergebnisse der Österreichischen Polarjahr-Unternehmung 1932–33 auf Jan Mayen, I. *Akad. Wiss. Wien, Math.-Nat. Kl. Sitzungsber.*, Abt. IIA, Bd. 143, 1934, 87–97). The longitude of a point in Jan Mayen was determined in 1883 by an Austrian expedition. It was redetermined by Tollner during the work of Austrian participants in the International Polar Year investigations of 1932–33. Comparison of the figures thus obtained discloses an apparent longitudinal shift of 2 min. 7·19 sec. in 50 years, Jan Mayen apparently being 1,270 metres farther west in 1933 than in 1883, or 1,400 metres if certain corrections are applied to the 1883 measurements. Thus the apparent drift of Jan Mayen is in the same direction as that of Greenland. On account of the uncertainties and errors inherent in the method of lunar culminations which was used by the astronomers of the 1883 expedition, Tollner concludes that his results cannot or should not be used as evidence in favour of Wegener's hypothesis of continental drift, and he considers that the same objections apply to the East Greenland observations on which Jelstrup based his opinion in favour of the hypothesis. If Jan Mayen drifts westward at the rate of about 2 metres a month, as the above result implies, it would be possible to detect such movement with complete certainty by modern geodetic methods within a period of three months. Tollner is careful to point out that his results do not prove that Jan Mayen has not moved to the west; it is merely that it is scientifically unsound to base conclusions either one way or the other on observations which include measurements so liable to serious error as those of 1883. Nevertheless, on the basis of longitude determinations by Jensen at Kornok in Godthaabsfjord (Greenland) in 1922 and their repetition by Sabel and Jörgensen in 1927, Tollner states that an absolutely real difference of longitude and therefore a real displacement of Greenland has been established.

The History of Science (D. McK.)

Following the example of the University of London, which more than ten years ago instituted a Department of the History of Science, the University of Oxford has now taken a first step towards recognising the importance of this subject by appointing Mr. R. T. Gunther, M.A., to a University Readership in the History of Science. Mr. Gunther delivered his Inaugural Lecture (*Oxford and the History of Science*, Oxford University Press, Humphrey Milford, 2*s*. net) in the Examination Schools on October 25, 1934, pointing out the immensity of his subject and its cultural value, stressing the part played by Oxford men in the development of science during the past 600 years, noting the contributions already made by members of the various Oxford colleges to the history of science from Ashmole, Rigaud and Daubeny onwards to the present day and urging as the essential needs for the proper study of the history of science a properly equipped Science Museum and a Departmental Library of the classics of science.

Miscellanea

The King has approved the following awards for this year of the two Royal Medals by the President and Council of the Royal Society: A Royal Medal to Prof. S. Chapman for his researches in the kinetic theory of gases, in terrestrial magnetism, and in the phenomena of the upper atmosphere; a Royal Medal to Prof. E. D. Adrian for his work on the physiology of nerve and its application to the problems of sensation. The following awards have also been made by the President and Council: The Copley Medal to Prof. J. S. Haldane in recognition of his discoveries in human physiology; the Rumford Medal to Prof. W. J. de Haas for his researches on the properties of bodies at low temperatures, and, in particular, for his recent work on cooling by the use of adiabatic demagnetisation; the Davy Medal to Prof. W. N. Haworth for his researches on the molecular structure of carbohydrates, the Darwin Medal to Prof. A. C. Seward in recognition of his work as a palæobotanist; the Sylvester Medal to Earl Russell for his distinguished work on the foundations of mathematics; the Hughes Medal to Prof K. M. G. Siegbahn in recognition of his work as a physicist and technician on long-wave X-rays.

Sir Arthur Balfour, Sir William Bragg and Lord Rayleigh have retired from the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research on completion of their terms of office. Sir John Cadnam and Sir James Jeans have been appointed members of the Council.

Prof. W. W. Watts, F.R.S., has been elected president of the British Association for the year 1935.

Prof. A. J. Clark of the University of Edinburgh and Prof. J. C. C. Ledingham, director of the Lister Institute, have been appointed to be members of the Medical Research Council in succession to Sir Charles Sherrington and Dr. A. J. Arkwright.

We have noted with great regret the announcements of the death of the following well-known men of science during the past quarter: Sir John Adams, until 1922 principal of London Day Training College; Mr. H. A. Allen, geologist; Prof. S. Ramón y Cajal, For. Mem. R.S., professor of histology in the University of Madrid; Mr. C. Carus-Wilson of "singing-sands" fame; Prof. W. C. Clinton, professor of electrical engineering at University College, London; Prof. B. J. Collingwood, O.B.E., professor of physiology in the University of London; Sir Edgeworth David, K.B.E., F.R.S., emeritus professor of geology in the University of

Sydney; Prof. G. Dreyer, C.B.E., F.R.S., professor of pathology in the University of Oxford; Prof. A. Fernau, director of the Institute of Radium Technology, Vienna; Mr. H. Glauert, F.R.S., mathematician; Prof. J. G. Gray, professor of applied physics in the University of Glasgow; Prof. W. M. Hicks, F.R.S., formerly professor of physics in the University of Sheffield; Dr. B. Laufer of Chicago, anthropologist; Prof. D. A. Murray, emeritus professor of mathematics in McGill University; Sir Thomas Muir, C.M.G., F.R.S., mathematician; Prof. W. McF. Orr, of University College, Dublin, mathematician; Prof. M. S. Pembrey, F.R.S., lately professor of physiology at Guy's Hospital; Sir Arthur Schuster, F.R.S., physicist.

The next meeting of the General Assembly of the International Astronomical Union will take place in Paris during the week July 10–17, and the Sixth International Botanical Congress will be held at Amsterdam on September 2–7.

In a letter to *Nature* (Oct. 6) Meyer, Schein and Stoll, writing from the Physical Institute, Zurich, state that they have detected a band of solar radiation extending from about 1900 Å to 2400 Å with a sharp maximum at 2100 Å. The absorption of ultra-violet light by ozone begins at 3100 Å, reaches a maximum at 2540 Å and decreases rapidly below this wave-length. Here, however, absorption by oxygen prevents any considerable quantity of energy reaching the earth's surface and only by working at high altitudes with a quartz monochromator and a very sensitive photon counter was it possible to detect it. The maximum occurs as would be expected where the resultant absorption of oxygen and ozone is least and its intensity on the Jungfraujoch (3460 m.) was about a thousand times greater than at Arosa (1860 m.).

The Report of the Building Research Board for the year 1933 deals for the most part with matters of trade interest. The Board is still pessimistic concerning the possibility of making brickwork waterproof by external treatment and goes so far as to record one case in which such treatment caused the formation of a hard skin on the surface of the brick which blistered and flaked so badly that it had to be removed. In any event the effective life of "waterproofers" seems to be limited to 18 months.

The Report states that solid concrete floors when laid on the ground are always liable to be damp. The factor which determines whether or not there is trouble, is the rate at which the moisture rising through the capillary channels in the concrete can be evaporated. Given good ventilation and a certain amount of heat, the moisture may be evaporated and conveyed out of the building as fast as it can penetrate the concrete. If ventilation is restricted —as, for instance, by storage of goods—then moisture will accumulate and the usual concomitants, fungus and mildew, may appear. Laboratory tests on Portland Stone have now been developed so far as to enable good resistant stone to be distinguished from very inferior stone, but it is not yet possible to grade the material more closely by this means.

An International Tin Research and Development Council, composed of delegates appointed by the Governments of the principal tin-producing countries, has been established for the purpose of acquiring and disseminating scientific and technical knowledge relating to tin, its alloys and chemical compounds, the processes involved in the production of these materials, and their applications. The objects of the researches and other activities of the Council are to discover and develop new industrial applications of tin, to improve the existing products and processes, and to assist tin consumers in overcoming technical difficulties and problems relating to tin.

Some fifteen technical papers have already been published. Among them is a 45-page pamphlet containing a very complete list of quantitative data relating to the properties of tin collated from papers and the standard Physical Tables. The original data are given and the user is left to make his own choice, *e.g.* between the Bureau of Standards value of 11·5 microhm-cm. for the resistivity of tin at 20° C. and the 12·853 microhm-cm. given by Pietenpol and Miley in the *Physical Review.* Another paper describes a rapid method for determining the thickness of tin coatings on steel by dissolving the metal from a known area with a solution of antimony trioxide in hydrochloric acid. (The thicknesses to be measured are of the order of 0·0001 in. to 0·0005 in.). A discovery of more general interest is that of the effect of small quantities (0·5 per cent. to 1·0 per cent.) of tin compounds such as tin oleate and tin tetraphenyl on lubricating oils. These compounds inhibit oxidation of the oil in air thus lessening the deposit of sludge and lengthening its useful life. The data given in the paper are based on results obtained with glass apparatus in the laboratory and would seem to need confirmation by tests under working conditions.

H. L. Curtis and R. W. Curtis have made a new determination of the ampere in absolute measure using the current balance employed in 1911 by Rosa, Dorsey and Miller for the same purpose. Their work is described in full detail in the *Bureau of Standards Journal of Research* (June 1934). Their final result showed that the Bureau of Standards international ampere = 0·999928 absolute ampere, a value two parts in a million greater than that obtained by Rosa and his colleagues. This agreement is, however, fortuitous since the difference between the two results is considerably less than the probable error in either and one set of observations gave 0·999990, a deviation of 62 parts in a million from the weighed mean.

The *Journal of the Bureau of Standards* for August 1934 contains an interesting paper by Wensel, Roeser, Barbrow and Caldwell on the comparison of carbon and tungsten filament lamp standards which gives some interesting details of the variation of the brightness of full radiators with temperature. At 1600° C. it is only 2·1 candles per sq. cm., at 2000° this figure has risen to 44·5, at 2200° to 137, at 2500° it is 533 and it reaches 1000 at about 2680°—data which emphasise the advantage of running lamps at the highest temperature compatible with a reasonable life. In the July issue of the same *Journal* Osborne and Meyers give revised tables of the saturation pressure of water vapour over the range − 5° C. to 374° C. calculated from the very remarkable equation

$$\log_{10} p = \mathrm{A} + \frac{\mathrm{B}}{\mathrm{T}} + \frac{\mathrm{C}x}{\mathrm{T}}(10^{\mathrm{D}x^2} - 1) + \mathrm{E}(10^{\mathrm{F}y^{5/4}})$$

in which $x = \mathrm{T}^2 - 293700$, $y = 374{\cdot}11 - t$ (374 11° C. is taken as the critical temperature) and A, B . . . F are constants ! Most of the data used in the computation have been published since 1932.

There is also a paper by L. W. Tilton which indicates that there is a correlation between the refractivity of air and the annual sunspot number. It has already been shown that the storminess of the earth's atmosphere increases with the sunspot number, and it is suggested that, when this number is low, the decrease in the average rate of stirring of the air permits denser components (*e.g.* associated molecules or isotopes) to settle near the surface and produce a mixture of slightly increased refraction. The standard value of $\mu - 1$ for dry CO_2-free air at S.T.P. is 0·0002925 and the variation is about 0·0000015—an amount sufficient to indicate that standard wave-lengths should be defined and used *in vacuo*.

ESSAY-REVIEWS

THE LIMITATIONS OF GENERAL RELATIVITY. By PROFESSOR E. A. MILNE, F.R.S., Rouse Ball Professor of Mathematics in the University of Oxford Being a review of **Relativity, Thermodynamics and Cosmology,** by RICHARD C. TOLMAN, Professor of Physical Chemistry and Mathematical Physics at the California Institute of Technology. [Pp. xvi + 502, with 13 figures.] (Oxford: at the Clarendon Press. 30*s*. net.)

IN this comprehensive treatise, Professor Tolman gives a sustained account, with ample mathematical detail, of the three subjects indicated in the title. It opens with a description of the " special " theory of relativity, and of the mechanics, in that theory, of a particle, a continuous medium and the electromagnetic field. It passes to an outline of thermodynamics, in the later sections of which it applies Einstein's mass-energy relation to the statistical equilibrium (under enclosure conditions) of hydrogen and helium, and of matter and radiation, with discussions of the results on the present ratios of hydrogen to helium or matter to radiation in the universe. The book then presents the fundamental ideas of " general " relativity and the relativistic theory of gravitation, and applies them in the well-known way to the essential gravitational phenomena. Next comes a body of theory devoted to the extensions of general relativity to electrodynamics and thermodynamics. This section partially anticipates in its discussion of the reversible motion of a fluid the substantial concluding chapter on " Applications to Cosmology." This last is divided into four sections embracing static and non-static cosmological models, their thermodynamic properties and the correlation of phenomena in the actual universe with the help of the results obtained.

The style of presentation is admirably clear and dispassionate—perhaps at times almost a little too apologetic for its own objectivity when comparatively meagre gains follow elaborate developments. The reader already familiar with the bases of the subject will have many happy hours following the ramifications unfolded by Professor Tolman. The work includes much original work by the author, published and unpublished, but it is no mere compilation. The sequence of topics is in fact most skilfully chosen, so that the reader is insensibly led to expect each new development. Moreover, the mathematics is almost all *verifiable* mathematics, that is to say, the reader is not left to grapple with mathematical obscurities for himself. One must admire the erudition and versatility of an author not only at home with the broad outlines of so many different branches of his subject but familiar with, and lucid concerning, the last mathematical details, even when the topic itself (for no fault of the author's) may be a little dreary. But this may convey a wrong impression, for the book arrests the attention almost everywhere. Further, the book may for some time to come well be an authority for references to standard and other formulæ, which are chronicled with encyclopædic thoroughness.

On fundamental matters, however, the work is not satisfying. Whilst all *consequential* mathematical investigations are given in full, the really important underlying ones are omitted and their results merely stated, either without proof or with only verification—a very different matter from proof. For example, the treatment either avoids facing fundamental issues, or omits the original, synthetic, path-finding derivations in the sections presenting (1) the establishment of the Lorentz transformation formulæ, (2) the relativistic transformation of Maxwell's field equations, (3) the demonstration of the existence of entropy as a function depending on the state and not the history of a system, (4) the fundamental Sackur-Tetrode expression for the "chemical constant" of a monatomic gas (why does h suddenly make its appearance here ?). Some of the missing derivations are to be found in the author's other works. But the calculation of dissociation equilibria, for example, turns essentially on the value of the chemical constant, and the curious reader is left with a sense of emptiness when he is asked to take this for granted. The author has not altogether succeeded in penetrating the subject from the outside. These are less criticisms of what is in the book than suggestions for future additions which would enormously enhance the degree of completeness achieved.

For if he made these additions the author would have to face issues which he in fact evades. It is true that in his introduction he expressly disclaims any intention of pursuing matters "so pleasant to the logician" as "the search for the smallest number of mutually independent and compatible postulates." But this is not the point. The reader wants to know exactly what the author means, or what he has assumed, in the derivations omitted. Take, for example, the Lorentz formulæ. The proof of these—the catalogue of what is assumed in their derivation—is the acid test of any work on relativity; the "general" theory of relativity is itself one possible generalisation of the results of "special" relativity and depends intimately on the Lorentz formulæ; in the author's original thermodynamic contributions the frequent appearance of factors of the type $\sqrt{1 - u^2/c^2}$ is a direct consequence of the Lorentz formulæ. Yet we are not shown how this surd originates, or what is involved in its derivation. The author implies throughout the existence of the rigid measuring-rod, yet does not enquire by what tests we are to recognise a distant measuring-rod as of the same "length" as one at the observer. How are we to be sure that its "length" is unaltered by transport from one position to another "stationary" with respect to the observer? Even if we could do this, what does "stationary" mean? Co-ordinate stationary? Or stationary as the result of adding rigid rod to rigid rod in chain fashion? Moreover, the army of clocks "distributed at convenient intervals throughout the system and moving with it," required by the author to make any progress at all in stating the meaning he attaches to the Lorentz formulæ,[1] implies a definition of stationariness. Again how are these clocks to be recognised as identical with the observer's own clock? How are they to be rated and synchronised as to time-zero so as to agree with the observer's clock and with one another? The answer to this question necessarily involves a definition of simultaneity of a distant event with an event at the observer, yet the "lack of any agreement as to [distant] simultaneity provided by the theory of relativity" (only mentioned incidentally on p. 108, long after the formulation of the Lorentz formulæ) stands as a looming shadow over the whole business. How can there be lack of agreement as to something not even defined? If we can define "distant simultaneity" for relatively stationary observers or clocks, why is it impossible to convey the same notion to relatively moving observers? The notion of distant simultaneity is involved in the very definition of velocity; for the velocity of a particle is the ratio of the distance it describes to the epoch-difference between two distant events, namely the events defining the beginning and end of its trajectory-arc; both these have to be assigned a time-coordinate by the given (distant) observer before he can legitimately employ the notion of velocity, *i.e.* before he can state what he means by a second observer in uniform motion relative to himself. Yet Professor Tolman assumes the notion of uniform relative velocity at the outset, before the difficulties of timing at a distance are referred to.

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[1] Actually this army of clocks is entirely unnecessary. It can be shown that the only clocks needed are two, one in the possession of each of the two relatively moving observers considered.

By taking sufficient trouble all these questions can be answered, and they are worth answering. The answers help to make clear an essential difference between " special " relativity and " general " relativity. " Special " relativity can be presented in a manner entirely devoid of a conceptual under-structure. " General " relativity, on the other hand, begins with the concept of a co-ordinate system. In " special " relativity, co-ordinates can be presented simply as *constructs* made out of given observations made before the co-ordinate system is mentioned. In general relativity a scheme expressed in conceptual co-ordinates can always be translated into the observations it implies ; but it has never been shown, and it is not in fact true, that any set of observations whatever can be described in the symbolism of " general " relativity. The latter statement is indeed obvious in the sense that what we mean by predicting a phenomenon by means of the theory is just the imposition of a possible restriction on observations. But the statement is true in the further sense that not all sets of observations compatible with the *principle* of relativity can necessarily be expressed in terms of the formalism of " general " relativity. The clothes of general relativity do not fit all figures The conceptual co-ordinate mesh-system used in general relativity is indeed of a special kind, namely Riemannian. But it is clearly possible to correlate any continuum of point-events with any continuum of 4-dimensional space, although only in special circumstances will the technically defined and observable " interval " between two neighbouring point-events be measured by the geometrical interval between the representative points. For the purposes of " general " relativity, a space is built up around the given distribution of matter-in-motion in such a way that (when the matter consists of free particles) the particles follow the " straightest " possible paths in the space or space-time, known as geodesics ; it is then taken as axiomatic that any additional free test-particle will also follow a geodesic. The condition that the free particles must describe geodesics in the space-time restricts the nature of the space-time, and imposes properties such as curvature on it. But this condition is only imposed by the analyst's choice. He can clearly make other choices for the curves to be followed by free particles, and then the resulting space-time, built up around them, will be different. For example, in the space commonly used in physics the space-curvature is zero and the paths of free particles in the corresponding space-time are not geodesics. It can be shown, at least in certain cases, that the analyst in describing a gravitational field can still employ a "flat" space, provided he uses suitable invariant factors to "weight" the elements of arc in the four-dimensional track. This was first made clear by S. R. Milner. The usage of "curved" space in general relativity treatments should not be allowed to mislead the reader into thinking that "Space" is *really* curved. It is better to regard "Space" as arbitrarily at the observer's choice, to regard the one chosen as a possible language for the description of the phenomena. *Some* language is necessary before description is possible at all. But to hold that the space employed in "general" relativity is some ultimate reality "Space" would be the same as to regard an Indo-European language as an ultimate reality for describing Aryan phenomena. Naturally, when the describer has fixed his choice of space, the "laws of nature" obeyed by phenomena as described by use of this space are relative to the space; different kinds of space may require different formulations of laws of behaviour of matter in motion, much as each language has its own idioms.

The upshot of these remarks is that we should beware of speaking of "physical space" as if it were a constituent of reality, but that instead we should speak of "the space used in ordinary physics" or "the space used in general relativity." This is at least a view to be tested by detailed enquiry. Professor Tolman, on the other hand, does not appear to say what he means by "space," which the reader is led to regard as some definite constituent of the external world with explorable properties. That objects possess spatial relations is undeniable, but it is a long step from this to assume that they are embedded in some entity "Space."

For example, in his remarks upon the origin of the Lorentz formulæ, Professor Tolman repeats a statement often made in this connection (used, in fact, by Einstein), namely, "making use of obvious assumptions as to the validity of Euclidian geometry and the homogeneity of space and time, etc." The "homogeneity of space and time" was used by Einstein as justifying the assumption of *linear* forms for the Lorentz formulæ when these are derived as substitutions compatible with the invariance of $c^2dt^2 - \Sigma dx^2$, but it is very difficult to attach any meaning to the assumption. To attach a meaning to it we should have to have some idea of inhomogeneous time, for example, and then exclude such inhomogeneities. Until space and time are properly defined there is no sense in predicating the property of homogeneity, whatever it may be. It is at any rate clear that this "homogeneity of space and time," as used in this context, is not the same thing as the homogeneity assumed or established for cosmological models, though we would have expected them to be good examples; for in these models the formulæ of descriptions of an event by two equivalent observers are not of Lorentz type. It may be objected that when deriving the Lorentz formulæ we are dealing with "free space." But what then of their applications to continuous media and electrodynamics? If the objection is put in the opposite way, that we are dealing with the absence of gravitational fields, then that ought to mean that we are dealing with the absence of all accelerations. Actually Professor Tolman frequently applies the Lorentz formulæ to accelerated motion—notably in the "clock paradox," where his neglect of the effect of acceleration on the accelerated clock seems to me illegitimate. Again, in his applications of special relativity, Professor Tolman devotes a wonderfully clear and explicit section to the relativistic definition of acceleration-causing force. He nowhere shows why the left-hand sides of his defining equations cannot be identified with gravitational forces. It is no use at this stage to answer by saying that gravitational forces "cause" a "curvature" of "space" or "space-time." For why have gravitational forces this effect, yet not electromagnetic forces? If mass is of purely electromagnetic origin, and if special relativity *in the absence of gravitation* is adequate for the description of electromagnetic fields by relatively moving observers, then special relativity should be adequate to deal with *all* properties of mass, including, of course, gravitation; if not, then mass is not of purely electromagnetic origin. The reader may be referred to the introduction to Professor Max Born's paper, at the recent International Conference on Physics, on the electromagnetic field. The fact is that "general" relativity is still an empirical method of dealing with gravitation. It is, of course, amazingly successful. It is less empirical than the Newtonian formulation, but still empirical. For example, it does not show why the Newtonian constant of gravitation is positive and not negative—why gravitation is attractive and not repulsive; the sign of "κ" is determined empirically by comparison with the Newtonian law. Again, the smallness of the cosmological constant λ is an empirical result—if indeed it exists as a natural constant.

These latter remarks are not, of course, criticisms of the book under review. But they are relevant to Professor Tolman's philosophical attitude. He quotes with approval the view that laws of nature are to be regarded as codifications of the results of experimental observations; but admits later that Einstein's theory was not the result of a mere attempt to account for small, existing, observational discrepancies with Newtonian theory; rather was it (in Tolman's happy phraseology) "the full flowering of a complicated theoretical structure, growing from fundamental principles whose *main* justification seemed to lie in their inherent qualities of reasonableness and generality" (our italics). He well adds "the extraordinary success of a theory, obtained by those methods of intellectualist approach whose dangers have been so evident since the time of Galileo well bespeaks the genius of its founder."

Actually, as has often been pointed out, laws of nature as recognised are rarely mere codifications of experience; we do not in fact apply completely the Baconian scientific ethic. If they were, and we declined to assent to expectations of future fulfilment, scientific law would be poorer than case-law; for it would not even set precedents. The object of theoretical scientific investigation is to represent more and more instances (observed or predicted) as examples of fewer and fewer general principles; to infer the widely differing bye-laws of nature from grander generalisations. Now usually there is some substratum of posited or postulated fact on which even the grandest generalisations are made to depend; that is to say, something assumed to be so-and-so in the world which might possibly be a different so-and-so. If we come to a fact beyond which we can get no deeper, then we have an ultimate irrationality, in Whitehead's phrase; there we find the finger of God. But can we be sure that the process of reducing the factual basis does not come to an end? May not the fundamental laws of nature in the last resort be, not mere projections of the mind as Eddington has suggested, but expressions of the logic of things, of the compatibility of observations, of the only phenomena capable of consistent observation by all observers? May not the ultimate observational basis be *nil*? Is it not possible that mathematical physicists will ultimately do for moving configurations of particles what the Greeks did for static configurations of points, namely deduce their properties by pure logic? From this point of view the present stage of physics may be comparable with the Egyptian stage of geometry, when geometrical facts (like the construction of a right-angle by a 3, 4, 5 triangle) were just empirical facts of experience.

It is for the extent to which Einstein's theory realises this ideal that Tolman rightly praises it, in the passage quoted. But it realises this ideal to an imperfect extent, as shown by its various empirical appeals to experience. The *principle* of relativity by itself is an almost perfect example realising the ideal; but that particular expression of it called the "general" theory of relativity may perhaps give place in time to some more fundamental expression of the ideas of relativity, containing a smaller basis of appeal to experience.

Professor Tolman's repeated insistence on the complete justification for " accepting " relations obtained by the theory of relativity as depending on the results of and confirmation by experience seems to pull the other way from his efforts to exhibit the " reasonableness " of the theory.

For, as Sir J. J. Thomson once said, a scientific theory is a policy, not a creed, and whilst a creed is something to be accepted or rejected, a policy is something to be pursued. There appears to the writer to be something dangerous in Professor Tolman's frequent references to " acceptable " scientific theory. " Acceptable " theory is own cousin to orthodoxy, which, though rightly a safeguard against thought-extravagances, is sometimes an obstacle to the emergence of new ways of thought, even in science. " General " relativity would in the long run suffer if it were ever regarded as beyond criticism. It is not impious for each new generation to think out its first principles for itself. No one should ever be asked whether they " accept " general relativity. It is legitimate to pursue all policies whilst accepting no theories ; hoping all things but believing no things.

Some trace of the influence of authority is found on p. 321 of Professor Tolman's treatise. He writes : " The possibility of a universe or cosmological model completely filled with an expanding gas was not considered from the classical point of view. It is this latter possibility that proves to be important for relativity. Classically, such models were not investigated . . . because it was known that the unmodified Newtonian theory of gravitation . . . was unable to carry through any unique treatment for such a non-static cosmological model." Actually, whilst Professor Tolman's book was in the press, this very problem was solved, for the case of an infinite space-filling dust-cloud, on strictly Newtonian principles, *i.e.* using the unmodified Newtonian theory of gravitation. This solution is adequate for all *present* astronomical purposes, and gives great insight into the meaning of the corresponding general relativity solutions. For example, what is described in the relativity solutions as an inherent curvature (positive or negative) of space, appears in the Newtonian solutions simply as the excess or deficiency of the square of the velocity of escape of a particle from its neighbours (reckoned by an observer on an arbitrary neighbour) over the square of the parabolic velocity of escape. Thus the mathematicians of the eighteenth century, on their principles, could have successfully accounted for the expansion of the universe if it had then been observed, or even predicted the relation between rate of expansion and mean density, and general relativity can now by no means claim to be indispensable in this respect; though of course it finds its own way of avoiding the infinite velocities predicted by the Newtonian models for small values of the " age " of the universe or at large distances. My point here is, however, chiefly that the problem was easily solved as soon as minds were cleared of the authoritarian view that general relativity was essentially necessary.

Professor Tolman's concluding section, on the relations between co-ordinate positions and observable quantities in cosmological models, is very clear and very useful. He shows how to eliminate the purely mathematical co-ordinates and obtain relations between observables only. An example is the relation between the expected numbers of nebulæ N out to a given limiting red-shift $\delta\lambda/\lambda$ and the bolometric luminosity l at this limit. This relation holds at any given epoch of observation. It would have been instructive, however, to consider simply the relation between any two of these quantities as the epoch of observation advances. For example, we could consider the predicted number of nebulæ $n\,ds\,d\omega$ inside a given solid angle $d\omega$ as observed at a given epoch of observation t to possess Doppler shift ratio $s (= (\lambda + \delta\lambda)/\lambda)$ lying between s and $s + ds$, say $n(t,s)ds\,d\omega$. If this policy is carried out it appears that the cosmological models of general relativity only achieve their object (of having every nebula surrounded by others) by the creation or annihilation of the necessary matter at the limit of observability for any given observer. The models predict that the observer would from time to time see new nebulæ born (or annihilated) in his field of vision. The new nebulæ are (in certain models) born receding with the velocity of light, just invisible, and then decelerate into visibility, ultimately fading again as increasing distance gets the upper hand. It is not that the analyst has *introduced* the creation of matter into his assumptions. The mathematics takes control, and creates the matter to fulfil the task imposed upon it by the analyst. If the condition be imposed that no matter shall be created or destroyed within the accessible temporal experience of the observer, then we have a criterion which compels the rejection of many cosmological models. Professor Tolman leaves us with a choice of many models. The suggested criterion might serve to distinguish between them, and remove the note of comparative pessimism on which Professor Tolman ends his scholarly treatise.

THE RELATIONSHIP OF GENETICS TO MEDICINE. By P. A. GORER, M.R.C.S., L.R.C.P., B.Sc. Being a review of **The Chances of Morbid Inheritance**, edited by C P. BLACKER. [Pp. xii + 494, with illustrations and charts.] (London: H. K. Lewis & Co., Ltd., 1934. 15*s*. net.)

THE relationship of heredity and environment in determining behaviour in general and disease in particular has been a contentious subject amongst doctors and sociologists for some time.

It cannot be claimed that the problem is solved, but geneticists are now in a position to state it far more clearly than was possible a few years ago. It would appear that the same is not true of the medical profession who, with few exceptions, appear not to have got beyond the stage of the subject at which Weismann left it. It must be remembered that Mendelism *alone* does not help to solve the general problem. If a condition is determined by a single gene, it does not mean that its appearance cannot be controlled by modifying the environment, although in some cases it may appear extremely improbable that medical science will find the way to do so within the next hundred years.

In applying knowledge medically, it is not always necessary to have a complete analysis of the processes involved; for example, we may state the probability of the offspring of a given marriage having or carrying a certain complaint without knowing the numbers of genes, etc., or the environmental factors concerned in its manifestation. Having given the figures, the question of the desirability of marriage or reproduction rests on the judgments of the patient and adviser. It is with the object of enabling the general practitioner to assess these chances that Dr. Blacker has edited this book.

As frequently happens in a composite work, the sections vary considerably in merit. Had all the authors kept within the limits laid down in Dr. Blacker's preface there would have been less to criticise. Unfortunately some of the contributors have wasted a great deal of space on what are, for the most part, extremely muddle-headed speculations on the relationship of heredity and environment.

The medical profession still seems obsessed with the idea of heredity *versus* environment and appear to regard a " hereditary cause " as something entirely different from an " environmental cause." It must be confessed that there is little in the chapter on Genetic Principles to elucidate the problem. The section is no more than a brief account of the theory of the gene, and has no special reference to the problems of clinical medicine or experimental pathology. It makes no mention of those genes whose phenotypic expression is influenced in a predictable way by the environment, as typified by the mutation "abnormal abdomen" in *D. melanogaster*. No mention is made throughout the book of a type of mutation studied extensively by Timoffeef-Ressovsky, in which there is not 100 per cent. manifestation of the gene with a constant genotype and a controlled environment. These genes also show a variable manifestation when heterozygous. It is probable that both these types are of great clinical importance and will help to explain many of the anomalous human pedigrees. It is doubtful if it will ever be possible to give a useful numerical answer to the question of the relative importance of heredity or environment except in a few special cases. The question is better set in a different form.

The medical man wants to know the following facts:

1. Is a given syndrome confined to a particular genotype ?
2. What are the conditions necessary for the production of the syndrome in the susceptible genotype ?
3. Is a syndrome reproducible in different genotypes by different stimuli or different intensities of the same stimulus ?
4. What is the genetic basis of the underlying diathesis ; is it a single gene ? If so is it dominant, recessive, etc.

There is one other aspect of genetics upon which there appears to be a good deal of confusion. On p. 143 a remark of Jennings is quoted to the effect that, "hundreds of genes are required to build a mind, even a feeble mind." This may be witty, but it betrays a fundamental misunderstanding of genetics. Genetics is not experimental embryology, the brain is not made out of genes. Pure genetics does not tell us how the brain is built. Genetics deals with differences ; when a gene mutates some physiological mechanism is modified. The consequent variation in the phenotype depends on the mechanism modified. For example, there are numerous mutations having a slight effect on the size of an animal. In the house mouse there is a single gene that when homozygous gives a sterile dwarf. Here there has been some interference with the mechanism of the pituitary gland and the animal's physiology profoundly altered. To avoid confusion it might be as well to follow the advice of Professor Fisher and to speak of a disease as being determined by a gene *substitution* rather than a single gene.

It is on the subject of mental disorder that eugenists have made their most extravagant claims, and it is here that most controversy remains. Genetic knowledge of this branch of medicine is unlikely to become precise till we have better and more objective methods of studying human behaviour. Very probably different genotypes respond differently to a similar social environment. But this does not tell us how far insanity is controllable by eugenic or other methods. On p. 93 Dr. Lewis says that nowadays the same *non-social* medical principles are used for the recognition of mental disorders as for physical diseases. On p. 122 he classifies agitators along with hysterics, swindlers, etc., as constituting a large portion of "the social problem group." It must depend to a large extent upon political ideas and social position as to whether one judges an imperialist, eugenic, or communist agitator as a social menace. The point is, though, not so much whether Dr. Lewis is inconsistent, but whether what he claims to do is desirable at all. Social behaviour only has relevance to a particular type of society, and the type of behaviour one wishes to produce must depend on the type of society one wishes to support.

The advice given in the chapter on mental defect is about as good as one can expect in the present state of our knowledge. The "primary amentias" still form a large mass of unsifted material. The questions set out earlier in this review still remain unanswerable. A great deal of the speculative matter in this chapter can be of no use to the general practitioner, and is on the whole too vague to be of value as a working hypothesis for the study of mental defect.

The chapter on diseases of the blood by Dr. L. J. Witts would be of value, if for no other reason than that in it are collected some of the contributions that the author himself has made to hæmatology. The data given on pernicious anæmia and the simple achlorhydric anæmia of Witts, bring out the importance of a thorough understanding of the pathology of certain conditions as a preliminary to a study of their genetic basis. In the case of both the diseases mentioned, our attention has been shifted from the blood-stream to the gastric mucosa, and it is here that we must look for the fundamental genetic modification.

Dr. M. Campbell, who writes on cardio-vascular diseases, has the distinction of being the only author to mention the mutations of the "abnormal abdomen" type. He gives an admirable account of the genetics of his own special subject and points out very clearly some of the difficulties of human genetics.

The section on skin diseases is deficient in many respects. Five common diseases are mentioned and four rare conditions of doubtful genetic basis picked more or less arbitrarily from the literature. Amongst the latter no mention is made of xeroderma pigmentosum (a recessive defect) or of the possibly allied porphyrinuria congenita. The account given of ichthyosis differs in many respects from that given by Cockayne. The special case of tylosis is said by the latter author to be due to a dominant gene on an analysis of forty-seven families. Dr. Forman gives Sieman's hypothesis that it is recessive, but gives no good evidence to support the contention. The most serious defect of all is that Cockayne's valuable book on the *Inherited Abnormalities of the Skin* and its appendages is not mentioned in the bibliography.

The chapter on tuberculosis is very disappointing. The relationship of genetics to infection is very complex. From a prospective parent's point of view, theoretical considerations do not greatly matter, but they may legitimately ask what are the risks of their children becoming infected if they themselves have suffered. The data obtained from the Grancher system in France and from the Papworth system in this country, are probably of more practical value than much of the evidence given in this book.

In the chapter on skeletal abnormalities, the accent is mainly on the embryological aspects of the problem, genetics being treated more or less *en passant*. The only definite eugenic prognosis given is for cleido-cranial dysostosis. One could equally well advise against the marriage of individuals with other deformities, even if they do tend to " die out " in later generations, as is said of " Lobster claw."

The other sections bring out points already dealt with; some of them, notably those on allergy and gastro-intestinal diseases, pointing out an interesting field for future research.

One point that emerges quite clearly from this book, is that it is seldom possible to give a eugenic prognosis for any particular clinical entity; it is nearly always necessary to consider the individual pedigree.

There are some curious omissions from this book; diabetes insipidus, xeroderma pigmentosum, alcaptonuria and a few other conditions whose genetic basis is known are not mentioned. From the point of view laid down in the preface the book is of value. It is in need of more co-ordination. Regarded as a treatise on pathological genetics it is already out of date.

REVIEWS

MATHEMATICS

Analytical Geometry of Three Dimensions. By D. M. Y. Sommerville, M.A., D.Sc., F.N.Z.Inst. [Pp. xvi + 416, with 49 figures.] (Cambridge: at the University Press, 1934 18*s*. net.)

This is a text-book for honours students at the universities and should prove most useful. There has been a tendency in elementary text-books to reduce three-dimensional geometry to a series of algebraic manipulations, and to obscure the geometrical ideas which form the essential part of the theory. In the book under review Professor Sommerville has avoided this mistake and emphasised at every step the importance of geometrical arguments. The book forms in some sense an introduction to Professor Baker's volumes on the *Principles of Geometry* reviewed on p. 347. In addition to the standard elements of the subject, there are chapters on tangential equations, curves and developables, invariants of a pair of quadrics, line geometry and algebraic surfaces.

E. M. W.

Differential Equations. By H. B. Phillips, Ph.D. Third edition. [Pp. vi + 125, with 17 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd, 1934. 10*s*. 6*d*. net.)

This text-book contains the theory of the simpler complete differential equations. The problems are mainly of the type occurring in mechanics and physics, and require the student to set up and integrate his own differential equation. With this in view, certain topics in mechanics and physics needed in groups of problems are discussed in the text.

E. M. W.

Numerical Integration of Differential Equations. By A. A. Bennett, W. E. Milne and H. Bateman. Bulletin of the National Research Council, No. 92. [Pp. 108.] (Washington: The National Research Council of the National Academy of Sciences, 1933. $1.00.)

This is the report of a committee of the Division of Physical Sciences of the National Research Council, and is intended for the student desirous of learning the theory of numerical integration of differential equations and for the practical computer. There are chapters on the interpolational polynomial, successive approximations and on step-by-step methods of integration. These three chapters deal with "topics probably essentially crystallised," but the subject of the last chapter, *viz*. methods for partial differential equations, provides many problems as yet unsolved, and it may be hoped that this report may call the attention of workers to this fruitful field of research.

E. M. W.

ASTRONOMY

Grundriss der Sphärischen und Praktischen Astronomie. By FRIEDRICH BECKER. [Pp. 167, with 59 figures.] (Berlin & Bonn: Ferd. Dümmler, 1934. R.M. 4.80, stiff paper covers; R.M. 6.50, bound.)

A KNOWLEDGE of the methods of spherical astronomy is a necessity to every astronomer, in whatever branch of the science he may choose to be engaged. This book supplies a clear and concise account of the general principles on which positional astronomy is based, and for its size it is remarkably comprehensive. Naturally the treatment is not so exhaustive as in larger text-books, but it is thoroughly up to date, and the importance of modern photographic methods has not been overlooked. The author does not aim at supplying detailed proofs of all the formulæ employed in the various reductions necessary in the preparation of a stellar catalogue from direct observations, but he brings the reader up to the point where he can supplement his knowledge by consulting specialised sources to which suitable references are given. Space has been found for illustrative examples of computation, and the numerous figures in the text are clear and well chosen. Brief accounts of the methods of interpolation, of Least Squares, and of map projections form an appendix of some thirty pages.

The last German text-book on this subject was written by De Ball and published in 1912. The obvious need of a more modern work has now been supplied by Dr. Becker, and his book, a masterpiece of compression, can be recommended not only to students, but also to lecturers who may wish to arrange a course in spherical astronomy. A translation into English should receive a welcome both here and in America.

R. W. W.

METEOROLOGY

Exploring the Upper Atmosphere. By DOROTHY FISK. With an Introduction by PROFESSOR H. L. BROSE. Illustrated by LEONARD STARBUCK. [Pp. 166, with 5 figures.] (London: Faber & Faber, Ltd., 1934. 6*s*. net.)

To those who would like the description of scientific observations on the constitution of the atmosphere, and related phenomena to take the form of an exceedingly pleasant narrative, this book can be heartily commended, even though its style may at times become somewhat poetic or even frivolous. It is suitable for readers of all ages, and can be specially recommended for boys and girls who are interested in science; they will find the chapters on ballooning and on polar lights most entertaining, for Miss Fisk appears to be an accurate and competent journalist.

L. F. B.

PHYSICS

Lehrbuch der Physik in elementarer Darstellung. By ARNOLD BERLINER. Fifth edition. [Pp. viii + 736, with 847 figures.] (Berlin: Julius Springer, 1934. R.M. 19.80, bound.)

THIS book may be unreservedly recommended to students and teachers of physics who are concerned with courses of instruction up to, and somewhat beyond, intermediate standard. It attempts to give an elementary picture of all important physical phenomena and succeeds in a most praiseworthy manner. It is excellently printed and bound; it is extremely well illustrated, and the use of coloured rays in some of the diagrams dealing with the formation of spectra, etc., are likely to be much appreciated; and it is to be hoped that the idea will take root with English publishers. The fact that the fifth edition is now before us is sufficient evidence of popularity and soundness.

The author pays much attention to the historical aspects of his subject, for he always endeavours to give the date when a particular definition was first used and by whom it was invented. Perhaps he tends to rely a little too much on German sources of information, *e.g.* the statement that the value of the mechanical equivalent of heat obtained by Jaeger and Steinwehr in 1915 is probably the most correct, will seem to British readers to be open to challenge. However, this is a minor fault in a book which, after all, is primarily designed for German students.

The reviewer particularly noted the simple descriptions of the Eötvos torsion balance and the Michelson-Morley experiment, and the references to the refractive indices of metals for visible radiations and also those of materials for X-rays. The book should specially appeal to students of physics who wish to perfect their knowledge of German, for its style is pleasing and straightforward, and there is a very marked absence of long, involved sentences. The price, R.M. 19 80, is really very modest, and one wonders what the circulation would be here if Britain were still on the gold standard.

L. F. B.

Physics. By PROFESSOR A. S. EVE, C.B.E., D Sc., F.R.S., McGill University, Montreal. Home University Library of Modern Knowledge. [Pp. 258, with 39 figures.] (London: Thornton Butterworth, Ltd., 1934. 2*s*. 6*d*. net.)

THIS is a very neat little book which will give an intelligent lay reader a very clear idea of the phenomena with which the physicist is concerned. It is written in a pleasant style with a marked economy of words. The author is very careful in defining the units in which physical quantities are measured. He covers a wide field, for sections on television and induced radioactivity are included in the book. It is felt, however, that the lists of references for further reading might be amended, particularly in the case of sound.

L. F. B.

Physical Constants. By W. H. J. CHILDS, B.Sc., Ph.D. Methuen's Monographs on Physical Subjects. [Pp. viii + 77, with 7 nomograms.] (London: Methuen & Co., Ltd., 1934. 2*s*. 6*d*. net.)

THE appearance of this little book will be cordially welcomed by all students, for it contains all the constants that are likely to be required in a normal degree course, it is convenient in size and shape, and its price is so modest that practically every student will be able to possess a copy. The information it contains is accurate and well set forth, and the compiler has saved a great deal of space, and obviously hopes to save much of his readers' time, by the use of nomograms, *e.g.* for capillary corrections, etc. In a future edition, however, it is imperative that a satisfactory index be provided, for the present list of contents is most inadequate. Incidentally, every reader will not remember the latitude of Greenwich, and that also might be included. The book is heartily recommended.

L. F. B.

Modern Acoustics. By A. H. Davis, D.Sc., Principal Scientific Officer in the Physics Department of the National Physical Laboratory. [Pp. xii + 345, with 8 plates and 104 figures.] (London: G. Bell & Sons, Ltd., 1934. 26s. net.)

A new impetus to the study of acoustics has been given by the technical requirements of the gramophone and radio industries, and the same developments as have made broadcasting and electrical reproduction possible have put new weapons into the hands of the scientific investigator. Most books written from the technical point of view in connection with new apparatus and processes tend to suffer from the propaganda habit, and to generalise from incomplete data and insufficiently controlled experiment. A book such as Dr. Davis's is thus of special value. Being the work of one who has spent many years at the National Physical Laboratory in carrying out and promoting investigation, and in the study of similar investigation elsewhere, he is qualified not only to compile the present knowledge of this subject, but also to examine it critically. In this volume, which covers a wide range of theory and experiment, he does both with success. He passes from introductory matter on general characteristics through chapters dealing with sources, measurements, hearing, building acoustics, etc., and the admirable summary he gives of the relative merits of the different reverberation formulæ is typical of his capacity to grasp and expound the essentials of a particular aspect.

The book is entitled Modern Acoustics and naturally makes no attempt to compete with, but omits much of what is contained in classical text-books on Sound since the underlying dynamical theory is unaltered by the advent of electrical sources and the methods of measurement.

It forms an impressive record of work done in the comparatively short period of about twenty years, and will make a strong appeal both to students and research workers who approach the subject from the scientific point of view, and to those engaged in industry who wish to make use of scientific research for technical ends.

The type is well set out and easy to read. Diagrams and photographs are excellently reproduced, and there is a most useful bibliography in the form of footnotes throughout the book.

G. A. Sutherland.

The Thermodynamics of Electrical Phenomena in Metals. By P. W. Bridgman. [Pp. vi + 200, with 33 figures.] (New York and London: Macmillan & Co., Ltd., 1934. 16s. net.)

Professor T. W. Bridgman is well known throughout the world from his experimental investigations of the properties of fluids in which use is made of the thermodynamic relations proved by Kelvin, Clapeyron, and others. It is perhaps not so well known that he has also concerned himself with the theoretical aspects, dating from the year 1919 when he published a critical thermodynamic discussion of the Volta, Thermoelectric and Thermionic effects. Since that year he has brought out at least sixteen papers, dealing largely with the theoretic connections between electrical properties and thermodynamics.

The book under review contains essentially the substance of these papers consolidated into a more or less coherent whole; some extensions and new relations have been added, and several important modifications or corrections have been made in some of the original formulæ. The final chapter on miscellanies contains much material which is not in the original papers, but is more or less common property and is included for completeness.

The added matter includes a very useful examination of fundamentals (pp. 5–38), mainly in connection with the Volta effect. This is followed by forty pages on thermoelectric phenomena, in which the peculiarities of the book become manifest. It must be stated that the author is not altogether conventional, and in his writings has struck out along lines of his own in an endeavour to surmount the difficulties which arise mainly in the application of thermodynamics. This, of course, in itself, is all to the good. Unfortunately, however, he apparently does not succeed in getting his friends to agree with him; nor does he convey the impression that he is himself assured that he has given a rigorous demonstration of his views (p. 57), and even puts forward a third possibility (p. 58) that the Peltier and Thomson effects are not *completely* reversible. In one point only does he speak with certainty. He considers that the incorrectness of the conventional point of view can be made absolutely certain if one considers thermo-electric phenomena "in single metal crystals of a non-cubic metal," (p. 60). (The description is his own.) To explain what happens he introduces a distinction between "the working E.M.F." and "the driving E.M F." This distinction on examination turns out to be apparently much the same as that between "potential difference" and "electromotive force" of the old-fashioned sort: a distinction which accounts for the phenomena equally well. However, the introduction of a second type of E.M.F. is supposed by him to remove all the difficulties (which even Kelvin felt at the beginning) connected with the application of the second law of thermodynamics to partially irreversible phenomena.

This short notice has kept to fundamentals. The main part of the volume deals with applications to thermionic phenomena, thermo-electric phenomena in crystals, transverse galvanomagnetic phenomena and connections with the electron theory of metals and photoelectric phenomena.

Teachers will find the volume contains numerous suggestive remarks which will be of value to them; but the dubious logic on which the author bases his subject will only tend to befog a student.

A. W. P.

The Electromagnetic Field. By H. F. Biggs, M.A., M.Sc. [Pp. viii + 158, with 38 figures.] (Oxford: at the Clarendon Press, 1934. 10*s*. 6*d*. net.)

This short book of 150 pages is written for students beginning an honours course in physics.

It opens with an introduction to the vector notation and this notation is used throughout the book. The subject matter is clearly and concisely put and the book may be described as a course of lectures prepared by a good teacher.

The material calls for little comment; there is no need for originality at this stage, but in the chapter on Dielectrics some departure from the usual treatment occurs. The author points out a difficulty which affects the usual treatment of the dielectric constant by means of removing a small sphere of the medium and avoids this method. In the space of this work the question cannot be adequately treated. A good feature is the introduction of a chapter on special relativity at the end. The author considers that the time has come when a knowledge of the electromagnetic field cannot be considered as complete without some appreciation of the synthesis given by the theory of relativity. Most teachers have appreciated this point and a course of lectures on the subject of the book usually includes some introduction to the special theory of relativity.

One notices a tendency nowadays to publish books of a condensed character on scientific subjects, usually with an apology that time is short and there is much to learn. But one wonders if students attempt to read the larger works especially by authors who are themselves pioneers. It will be a loss to scientific study if knowledge becomes superficial, or if one subject is studied merely because of its application to another. Short books can only be welcomed if they are very well written and if they encourage wider reading. The volume under review is one that we can strongly recommend on both grounds, and we regret that the death of the author has removed such an excellent teacher from among us.

H. T. F.

Electromagnetic Waves. By F W. G. White, M.Sc., Ph.D. Methuen's Monographs on Physical Subjects. [Pp. viii + 108, with 26 figures.] (London: Methuen & Co., Ltd, 1934. 3*s*. net)

In writing this monograph the author has set himself the task of giving an account of the propagation of electromagnetic waves and of introducing the reader to applications in the study of phenomena associated with the propagation of these waves in the upper atmosphere. The former part of this project covers the short space of eighty pages and the latter occupies the final chapter.

This is no simple task to undertake and in reading the opening pages of the work it is surprising to find with what elementary beginnings we set out. From such simple assumptions as the nature of the force between charges we pass in the second chapter to Maxwell's equations and, after a chapter on the theory of Lorentz, we arrive at the theory of dispersion. The steps are clearly set out and it is a tribute to the author that nowhere are we asked to make acts of faith and spring over difficulties with eyes closed.

The reader is expected to know something of the subject and the book is written for the advanced student. It is a logical, though necessarily condensed, account of the electromagnetic theory with especial stress on the theory of dispersion; the importance given to this aspect of the theory being due to the fact that the book is really an introduction to applications in the study of the upper atmosphere. The author has himself contributed to the knowledge of that branch of the subject, and is thus well qualified to guide the reader directly to it.

The student will find the first five chapters an excellent summary of some important deductions from the electromagnetic theory, and all who are interested in the important researches into the conditions in the upper atmosphere will welcome this excellent introduction to the subject.

H. T. F.

Ions, Electrons and Ionising Radiations. Sixth Edition. By J. A Crowther, Sc.D. F.Inst.P. [Pp. xi + 340, with 114 diagrams.] (London: Edward Arnold & Co., 1934. 12*s*. 6*d*. net.)

It is five years since the appearance of the fifth edition of Prof. Crowther's excellent book on the elements of modern physics. The sixth edition is now available, and, like its predecessors at their times of issue, it forms as good an introduction to present-day atomic physics as students need or teachers would desire them to possess. Like its forerunners, it is excellently bound, printed and illustrated.

Many important changes may be recognised at once in the new edition, in addition to minor alterations made throughout the text. Thus, Langevin's method for the measurement of the mobility of an ion disappears after many years and is replaced by the method of Tyndall and Grindley, while the chapter on ionisation by collision has been completely overhauled and condensed. Perhaps the description of determinations of e/m for an electron might have been amplified by the inclusion of one modern method, but this book is only a first introduction to the subject, and the author must be allowed some latitude in his choice of material. The packing effect is adequately treated in this edition. New sections on anomalous scattering and artificial disintegration appear in the chapter on α-particles, and β-ray spectra receive more attention than heretofore.

Neutrons, positrons and cosmic rays have a pleasant little chapter to themselves, and a special chapter on the structure of the nucleus deals with Gamow's theory of nuclear structure and the idea of the potential barrier in a very lucid manner. A chapter on the structure of the atom replaces the former chapter on the electron theory of matter, and students will find a most welcome addition in a number of examples, complete with answers, at the end of the book.

Prof. Crowther's book has a definite place in the English literature of physics which the new edition will enable it to consolidate.

L. F. B.

Müller-Pouillets Lehrbuch der Physik. Eleventh Edition. Vol. IV, Part 4. **Elektrische Eigenschaften der Metalle und Electrolyte; Magnetische Eigenschaften der Materie.** [Pp. xx + 906, with 400 figures.] (Braunschweig: Friedr. Vieweg & Sohn Akt.-Ges., 1934. R.M. 62, paper covers; R.M. 66, bound.)

This excellent treatment of the electrical properties of metals and electrolytes and of magnetic phenomena completes the fourth volume of this well-known text-book. For the experimental worker the subject matter is of special interest, since in addition to a full description of the experimental results, a valuable and concise account of the modern electron theory of metals has been included. It is true that the reader requires rather a greater mathematical equipment than was necessary for the study of earlier volumes of the work, but this was unavoidable if the account was to be of real use, and the contributor, Nordheim, is to be congratulated on the way in which he has performed his task.

The description of the electrical properties of metals, and more particularly of the thermoelectric properties, forms an important addition to our sources of information. The treatment of thermionics is concise and up-to-date. Some 270 pages are devoted to the properties of electrolytes, including a very good account of the behaviour and theory of strong electrolytes. The section on magnetism, by Auwers, is also worth the serious attention of teachers of physics, for he has taken great pains to make his account simple to understand, and has not avoided the discussion of certain points of general interest which are too often neglected. Incidentally, while quoting Coeterier's result for the gyromagnetic ratio of pyrrhotite Auwers might have quoted his result for iron. The treatment is fittingly completed by an account of the modern quantum theory of magnetism, by Nordheim. The whole work is excellently illustrated and can be unreservedly recommended.

L. F. B.

Alternating Currents. By Leo T. Agger, B.E. [Pp. viii + 203, with 146 figures.] (London: Macmillan & Co., Ltd., 1934. 5s.)

To deal satisfactorily with alternating currents, it is generally considered necessary to have at least an elementary knowledge of the differential and integral calculus. The author has written this book with a view to avoiding that requirement, and he has shown considerable ingenuity and attained no small measure of success in carrying out his task. Some of the methods adopted which are necessarily only approximate go a long way round to achieve the final result, and it is a debatable point whether such methods are really the simplest and best in the end.

The first chapter deals with the principles governing the generation of an alternating electromotive force, the current produced in a non-inductive circuit and matters connected with the wave-form. The author prefers at this stage to apply Faraday's law of electromagnetic induction from the point of view of the number of magnetic lines cut per second. Later on he finds it necessary to introduce the conception of a rate of change of flux linkages and one cannot help feeling that this should have been done in the first instance. In the opening chapter there is also a brief reference to vector representation of sinusoidal quantities. No mention is made here of the addition and subtraction of vectors in relation to the sine waves which they represent. This simple extension of the vector idea is discussed at the beginning of Chapter IV, but the separation into these two parts seems to be rather a disadvantage in that the subject tends to lack continuity. The usual scheme of development is adopted for the main theme of the book, namely the effects of resistance, inductance and capacitance in alternating current circuits. Chapters II to IX are devoted to a discussion of these matters, series and parallel circuits incorporating various resistive and reactive components being treated in turn. Chapters X and XI deal with polyphase currents and the measurement of power in polyphase circuits. Finally, the alternator, the transformer, the induction motor and alternating current measuring instruments are each allotted a chapter for brief consideration particularly in regard to the elementary principles governing their operation. There is a good selection of examples both arithmetical and graphical interspersed throughout the volume.

In giving us this book the author has no doubt filled a gap in the wide selection of works on the subject. He caters for the essentially non-mathematically minded student and in that field he has undoubtedly achieved success.

H. M. Barlow.

Fundamentals of Hydro- and Aeromechanics. By O. G. Tietjens, Ph.D. Translated by L. Rosenhead, Ph.D. [Pp. xvi + 270, with 186 figures.]

Applied Hydro- and Aeromechanics. By O. G. TIETJENS, Ph.D. Translated by J. P. DEN HARTOG, Ph.D. [Pp. xvi + 311, with 226 figures and 27 plates.]

Engineering Societies Monographs. (New York and London: McGraw-Hill Book Co., Inc., 1934. 24s. net each.)

DR. TIETJENS has rendered a signal service in making available Dr. Prandtl's lectures, on which these two volumes are based. The first volume opens with a detailed account of the statics of liquids and gases, atmospheric stability, including the formation of clouds, and the equilibrium of balloons. Then the fundamentals of fluid motion science are presented very clearly, sections on kinematics and inviscid flow being followed by a chapter on the equations of viscous motion. The articles dealing with the use of conformal transformation will be, perhaps, particularly useful. Indeed, the whole treatment sparkles, as one would expect, with a masterly individuality. Most practising engineers, for whom the book is written primarily, will occasionally wish that it were less brief—for instance, as to the manipulation of sources and sinks—but a foreword explains that various matters of detail or procedure appearing in other text-books and papers have been omitted deliberately to keep the presentation of fundamentals clear. Pertinent criticism may be made, however, of the widespread employment of vector notation. Without discussion of various questions arising—for example, whether tensor notation would not have been preferable, or whether either is justifiable in engineering—the facts remain that few English engineers are familiar with the notation and that there is little sign of their early education being rectified in this respect. However, this is a book well worth reading with pen and ink, in the course of which the "pocket-maps" may be "unfolded" once and for all, if desired.

The second volume is of a more technical nature, although still confining attention for the most part to fluid motion, and here Dr. Tietjens has gone beyond Dr. Prandtl's lecture courses. There are excellent chapters on flow in pipes and channels, boundary layers, drag and aerofoil theory, and a brief description of experimental methods. Of particular interest are two or three suggestive articles written by Prandtl himself. The whole forms an interesting and, one would think, a necessary commentary on Vol. I, while also collecting together the cream of modern semi-empiricism with which this prodigiously growing subject is being nourished. One or two criticisms may be made. Recent work with the ultra microscope throws serious doubt on any physical significance attaching to the laminar film of Art. 49, which must be regarded simply as an empirical expedient. The statement at the top of p. 89 is not correct and, in spite of the qualification at the foot of p. 91, the artificiality introduced cannot be thought worth while in view of the harm it might do in an obvious connection to a first reading of the subject; the cogency of Prandtl's boundary layer conception needs no emphasis. Finally, objection may be made to the frequent use of "partial vacuum" to describe the pressure reduction of aerodynamics. Density variation effects do occur, but they are essentially incidental and negligible until, in extreme circumstances so far as present-day aeronautics is concerned, they become an unmitigated (and insoluble) nuisance. This was a great point duly made in Vol. I, and it is a pity to raise the trouble in the mind of the reader all over again. The books have been carefully translated by Drs. Rosenhead and Den Hartog and are admirably produced and beautifully illustrated.

N. A. V. P.

A Text-book of Applied Hydraulics. By HERBERT ADDISON, M.Sc., Assoc.M.Inst.C.E. [Pp. xii + 409, with 315 figures.] (London: Chapman & Hall, Ltd., 1934. 21*s*. net.)

THIS is an attractive text-book which will appeal to Engineering students who do not intend to specialise in this subject. Practically every phase of Hydraulic Engineering is touched upon, though perhaps somewhat too superficially for those reading for University degrees. In general, the subject matter is that of the older English and American text-books of Hydraulics, though rewritten in a clear and refreshingly concise manner.

Text-books which offer simple outlines of complex subjects usually invite adverse criticism, and this one is no exception. The intention " . . . to present a compact summary of the fundamental principles of hydraulics," expressed in the preface, is hardly fulfilled, since much of the condensation is at the expense of necessary qualifications; in fact, so much so, that some of the treatments become misleading.

The whole subject of the specific speeds of turbines and pumps is introduced and discussed without appropriate reference to blade angles or to cavitation. The latter is certainly discussed independently in a short though good paragraph at the end of the chapter; but the connection between specific speed and blade angle does not appear even to be mentioned, and the reader might well obtain the erroneous impression that the specific speed of a turbine runner was primarily decided by its external proportions.

Practically no reference is made to the theoretical and experimental advances of the past twenty years which have done so much to remove hydraulics from the impasse it had then reached. This is a pity, since one feels that a text-book should at least give a student sufficient groundwork to enable him to appreciate the tendencies of modern design.

Despite these and other faults, and an occasional irritating suggestion of the schoolroom, the general clearness of conception and exposition will appeal to a wide range of students—possibly to a wider range than the author intended. His warning that it is intended as a basis for the study of more detailed treatises should not be overlooked.

Both author and publishers are to be congratulated upon the manner in which the algebraic treatment is set out, and also upon the excellence of the diagrams and illustrations.

C. M. W.

CHEMISTRY

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. Vol. XIII, Fe (Part II). By J. W. MELLOR, D.Sc., F.R.S. [Pp. x + 948, with 381 figures.] (London, New York, Toronto: Longmans, Green & Co., 1934. 63*s*. net.)

DR. MELLOR is approaching the end of his gigantic task, and but two volumes remain to complete a monument of which he and British chemists have every reason to be proud. The present volume continues the subject of iron and deals with the mechanical, thermal, optical, electrical and magnetic properties of iron and iron-carbon alloys. The chemical properties of iron, the corrosion of iron and steel, the valency and atomic weight of iron, passivity, alloys and intermetallic compounds of iron, oxides and hydrates of iron, the ferrites, perferrites and ferrates are also considered. The subjects are treated in the exhaustive manner now so well known. Some idea of

Dr. Mellor's thoroughness may be gained from the fact that this volume contains approximately 198 pages of references and a rough computation indicates that these pages contain between twenty-five and thirty thousand individual references.

The present volume should appeal to a class who may not be specially interested in the work as a whole, namely, those concerned in the manufacture and use of iron and steel in their many applications since it is a very complete index to the properties of these materials.

O. L. B.

Handbuch der Anorganischen Chemie. Abegg, Auerbach, Koppel. IV. Band, 3. Abteilung: **Die Elemente der 8. Gruppe des periodischen Systems. Zweiter Teil: Eisen und seine Verbindungen.** Edited by Dr. I. Koppel, Berlin. [Pp. xii + 222, with 96 figures.] (Leipzig: S. Hirzel, 1933. R.M. 21·00, paper covers.)

This volume includes articles on Passivity, by Dr. Veszi, Corrosion and Chemical behaviour, by Dr. Schikorr, the systems Iron oxygen, Iron carbon hydrogen, Iron hydrogen oxygen, Iron carbon oxygen, by Dr. Hofmann, and the application of the above systems to the metallurgy of iron by the general editor, Prof. I. Koppel.

Dr. Veszi's contribution on passivity appears at first sight to be somewhat brief, yet references are given to over four hundred and sixty papers, and evidently a great deal has been compressed into a small volume. Little discussion is given to the theories of this phenomenon, the author taking the view that since passivity is not confined to iron it is better treated in a more general way. Yet it must not be forgotten that the bulk of these theories have been put forward as the result of experimental work on iron and that one would naturally look in a work such as this for these theories. Moreover, it is true that in actuality we need only consider the oxide film theory, yet a critique of the other theories may even to-day play a useful part. What a tribute the survival of the film theory is to the genius of Faraday.

Dr. Schikorr's article on Corrosion is excellent, yet pride of place must be given by the reviewer to the treatment of the Iron, oxygen, hydrogen, carbon systems, by Dr. Hofmann, and the application of these systems to everyday practice in the blast furnace by Prof. Koppel. A perusal of these chapters shows the enormous amount of experimental data which has been determined, and how far the iron industry has advanced from the rule-of-thumb methods of only about a generation back.

In common with the earlier volumes of Abegg references to all the more important papers are given, and as far as these were checked were found to be accurate. This volume fully realises the high scientific ideals of its predecessors.

H. T.

Laboratory Manual of Colloid Chemistry. By H. N. Holmes. Third Edition. [Pp. xviii + 229, with 60 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 20s. net.)

In his preface the author states, rather quaintly, that the field of colloid chemistry has broadened greatly since 1922; hence this enlarged edition of his useful laboratory manual. Chapters have been added on adsorption, from solution and from vapour, and on catalysis; and the arrangement of the book has been improved so that it covers the whole subject in an orderly manner. As a text-book, it suffers from some lack of balance from two causes. Odd titbits of information are too often interpolated. And a certain American bias leads to literature references being given which are not fundamental but subsidiary papers or books. The chapter on viscosity illustrates this; it is inadequate and misleading. Bingham's work on pastes is given; Staudinger's name is not even mentioned; nor any of the characteristics of the viscosity of lyophilic colloids. The historic method of Devaux for examining surface films is described as a modification of the Langmuir trough method. The value of this book is, not, as the author supposes, for self-teaching since supervision on theoretical points is required, but to lecturers and demonstrators in physical and colloid chemistry. To them it should be of great value; 240 experiments are included, full practical details being given for a large number of them; and there are 60 figures. The binding is vermin- and water-proof.

A. S. C. Lawrence.

Introductory Colloid Chemistry. By H. N. Holmes. [Pp. xiv + 198, with 34 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 15*s*. 6*d*. net.)

There is a very real need for a good introductory text-book on colloids, but this cannot be so described. It is an interesting elementary book on colloids, but not an introduction to the theory since fundamental points and definitions are not dealt with adequately or, even in some cases, accurately. The Tyndall effect is wrongly described, nor is it mentioned that Faraday discovered it. The section on monomolecular films is prefaced by the remark that "such films have, of course, but one colloidal dimension." The order is not good. On p. 4 it is remarked that colloid chemistry might be called surface chemistry, but the properties of surfaces are not dealt with until three-quarters of the way through the book and surface tension not till p. 50. Nor is the Gibbs equation ever given although mentioned. Classification of colloids is not discussed until nearly half-way though—in the chapter on gels—which may account for the categorical misstatement, on p. 2, that colloids are two-phase systems. The writer has succeeded in writing an interesting book on colloids; it fails as a text-book for that reason. In the chapter on soaps, their use as lubricants in wire drawing receives as much space as the statement of McBain's fundamental researches. These characteristics may make the book appeal to those who want an interesting reference book on colloids rather than a sound introduction to the theory.

A. S. C. Lawrence.

The Kinetics of Chemical Change in Gaseous Systems. By C. N. Hinshelwood, M.A., F.R.S. Third Edition. [Pp. vi + 392, with 17 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1933. 15*s*. net.)

This is the third edition in seven years of Mr. Hinshelwood's widely read book. The first appeared in 1926 when the intensive experimental and theoretical study of gas reaction kinetics now so much a feature of chemical literature was just beginning, and an excellent way of getting an impression of the remarkable development since then is to compare the successive editions of this book. It must also be true that the really admirable clarity of Mr. Hinshelwood's writing has done much to foster this research activity, by exposing the fascination of the problems awaiting attack. The present edition completely supersedes its predecessor. It is much larger, but even so it has not been possible to present as complete a statement of the relevant experimental work. Equally, it would be impossible even if it were desirable to discuss all the detailed theoretical speculations attempted in recent years, although the reviewer regrets that the work of Pelzer and Wigner on the calculation (1931) of the absolute velocity of the ortho-para hydrogen transformation does not find any place, since this represents a fundamental advance in method.

The introductory chapter providing the necessary background of kinetic theory has been enlarged to include an excellent introduction to quantum mechanical principles. The other new material is concerned mainly with homogeneous catalysis of gas reactions, and with chain reactions, and forms an excellent summarizing discussion of the large amount of work published from Mr. Hinshelwood's laboratory. There can hardly be any need to recommend a book so well known as this to students of chemistry; for them it is indispensable. But also scientific workers in subjects allied to chemistry will find here an excellent guide to an understanding of at least some of the more important factors that decide how fast a chemical reaction takes place. The book is printed in large clear type on good paper, and its price seems very modest indeed, for these days.

B. T.

The Electronic Structure and Properties of Matter. By C. H. Douglas Clark, M.Sc., A.R.C.S., D.I.C. [Pp. xxvi + 374, with 33 figures.] (London: Chapman & Hall, Ltd., 1934. 21*s*. net.)

This volume consists of a general introduction to the electronic structure of matter (Part I), and a more specialised treatment of physical properties and molecular constitution (Part II). It is to be followed by volumes on X-rays and the Structure of Matter, Molecular Polarisation, Molecular Spectra, the Raman Effect, and an introduction to Quantum Mechanics. Others are in contemplation on matters of special chemical importance.

The present volume is in the main concerned with the periodic classification of the elements, and in Part I this is explained in terms of the electronic structure of the atom. In this section the evolution of the terminology of the quantum numbers and the present position are adequately dealt with. In Part II are chapters on melting and boiling points, on atomic and molecular volumes, on atomic and ionic radii, on electrical conductivity of pure substances, on magnetic susceptibility, and on cohesional and other properties. The author has in mind the explanation of all the above properties, as far as this is possible, in terms of electronic structure.

It is difficult to assess the value of the author's plan as a whole, but there is little doubt about the present volume. No other book in English covers the same ground, nor is there anything written from the same point of view. The author is very sparing of words, and a very large amount of information is compressed into a small space. The book is ideal for readers requiring to make a rapid survey of a particular field of research, for the author has very ably correlated his material. The work of correlation must have entailed a good deal of hard work. As a work of reference it is worth its price for the content of the tables alone. It is, nevertheless, not easy reading, for the author delves rather deeply into the theoretical basis of the subject at times, and is unable in his limited space to help a lame dog over a stile. He gives references to fuller treatment in Vols. 2 and 3, which have not yet appeared, so perhaps this fault is not so great as it would appear.

The scope of Part II reminds one of the classical work of Smiles on Physical Properties and Chemical Constitution, but the comparison brings to mind the remarkable changes that have occurred in point of view since that book was published in 1910.

It is undoubtedly a valuable work of reference and will be very useful to students. As a stimulation to research it is possibly the next best thing to a book written by a researcher himself.

W. E. G.

Asymmetric Synthesis and Asymmetric Induction. By PATRICK D. RITCHIE, B.Sc., Ph.D. [Pp. x + 155, with 3 figures.] (London: Humphrey Milford, Oxford University Press, 1933. 7*s*. 6*d*. net.)

THE literature dealing with asymmetric synthesis and asymmetric induction is scattered and, until the appearance of this book, no adequate account of these subjects has been available in English. Apart from the light asymmetric synthesis may throw on the intriguing problem of the origin of optically active organic compounds in nature, the subject is undoubtedly of great interest from the point of view of theoretical organic chemistry, since it suggests another method of approach for the study of the mechanism of organic reactions. Many of the experimental results obtained in certain well-known rearrangements where the starting materials are optically active suggest that the mechanisms of these rearrangements now generally adopted must be at fault. Recent work in this field does not seem to have received sufficient attention, and for this reason the present volume is particularly welcome.

The author has collected together practically all the material bearing on the two topics of his title, with ample references to the literature, and presents it in an attractive and readable manner. The book is more than a compilation, since the evidences are considered critically and, though all may not agree with the author on all points, his views and criticisms are worthy of consideration.

O. L. B.

Tautomerism. By JOHN WILLIAM BAKER, D.Sc., A.R.C.S., F.I.C. [Pp. viii + 332, with 4 figures.] (London: George Routledge & Sons, Ltd., 1934. 25*s*. net.)

THE title of this work is unfortunate as it conveys little idea of the contents. The book is, in fact, the first comprehensive account of the interpretation of the mechanism of organic chemical reactions on modern electronic views, the only notable omission being benzene substitution. Since, however, the notion of tautomerism is almost implicit in the electronic conception of reaction, the title is not incorrect if it be realised that it is not used in the restricted sense of classical tautomerism.

The need of such a book was obvious. In the first place the ideas with which it deals, though open to criticism in details, are patently a step in advance since they provide, for the first time, an explanation of experimental facts which the older organic chemist was content, or rather compelled, to accept without reason. Why, for example, tertiary butane could be directly nitrated or a tertiary alcohol lost water more readily than a primary one, why in the addition of halogen acids to propylene the halogen went to the β-carbon atom, why phenol was a stronger acid than alcohol and why the Crum Brown-Gibson rule worked and so on. The theory has now been applied extensively throughout organic chemistry and, apart from a few difficulties, with outstanding success; such difficulties as exist probably indicate that it is still in too simple a form rather than that it is fundamentally unsound.

Secondly, the electron theory as applied to organic chemistry has developed with great rapidity and, as a natural consequence, has undergone kaleidoscopic changes. The only available sources of information have been the original papers of those responsible for its development and the Annual Reports of the Chemical Society but as modifications of views have been frequent and the subject somewhat intricate it has been difficult to obtain a satisfactory picture of the position. A stage has now been reached, however, when most of the pitfalls have been appreciated and the theory has, in outline at least, reached a position of stability. Dr. Baker has enunciated it in considerable detail with all the important experimental work in support.

In so complicated a subject it is impossible for a reviewer to criticise the matter of the book, discussion of a single point would be apt to range over a considerable part of the domain of organic chemistry. The author seems to have been very successful in presenting the views of the authorities, but here again only one or two people are competent to express an opinion. The book is not an elementary exposition of the subject and requires careful reading, it would, perhaps, have been an advantage if the author had been a little more merciful on those not brought up in close contact with the electronic theory, and had devoted more space to explanation at the cost of some of the examples. The typography is at fault in that the formulæ which really matter, that is those indicating electron displacements, are too small, ordinary structural formulæ, strangely enough, are printed in larger type.

Dr. Baker, however, has earned the gratitude of organic chemists by undertaking a most difficult task with conspicuous success, and there is no doubt that his book will be in great demand by both research workers and advanced students of the subject.

O. L. B.

Organic Syntheses. Vol. XIV. W. W. Hartman, Editor-in-Chief. [Pp. viii + 100.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 10s. 6d. net.)

This new volume of a work now well known to all organic chemists describes tested methods for the preparation of aceto-*p*-cymene, benzanthrone, benzopinacol, β-benzo-pinacoline, *o*-bromophenol, β-chloroethyl methyl sulphide, decamethylene glycol, diazoaminobenzene, β-diethylaminoethyl alcohol, 2 : 6-dimethylpyridine, diphenyl sulphide, ethyl acetosuccinate, gallacetophenone, glycerol α-γ-dibromhydrin, glycine ethyl ester hydrochloride, *p*-hydroxybenzoic acid, 2-hydroxy-3 : 5-diiodobenzoic acid, β-hydroxyethyl methyl sulphide, *dl*-methionine, 5-methylfurfural, *p*-nitrodiphenyl ether, nitromesitylene, *p*-nitrophenyl isocyanate, nitrothiophene and *dl*-β-phenylalanine.

In an appendix later references to preparations in the preceding volumes are given and a number of additions and corrections. The index contains references to material in vols. X to XIV, that is to all that published since the collective volume.

O. L. B.

A Practical Chemistry for Higher School Certificate and Intermediate Students. By G. P. McHugh, M.Sc., Ph.D.(Lond.), M.Ed.(Dunelm.). [Pp. viii + 117, with 17 figures.] (London, New York, Toronto: Longmans, Green & Co., 1934. 2*s*. 9*d*. net.)

This is a small laboratory text-book for elementary students. Section I contains the usual reactions of the common metals and acids, and a scheme of analysis of simple mixtures. The working instructions are good, and the theory of the group separations is found opposite each table, which leads to a better understanding of analytical procedure and discourages a merely mechanical routine. Section II on Volumetric Analysis is adequate, but not every teacher would adopt the methods of calculation employed. In Section III a few inorganic and organic preparations, and the tests for elements in organic compounds are given, together with three quantitative exercises which seem rather misplaced in this section. The sensitivity of Smith's phosphate separation (p. 39) is obviously understated; the test for nitrate in presence of nitrite (p. 52) will always give a positive result as the equation shows; the nitrate-halide test (p. 53) is ambiguous, and the double formula of potassium permanganate is obsolete. The book is moderately priced, its contents are well arranged and presented and it will doubtless hold its own against many similar publications.

J. N. S.

The Chemistry of Petroleum Derivatives. By Carleton Ellis. [Pp. 1285, with 48 figures.] (New York: The Chemical Catalog Co., Inc., 1934. $18.00.)

This remarkable volume appears at a particularly interesting time, when the chemist is becoming increasingly successful in treating the constituents of petroleum as raw materials for chemical reactions, instead of dealing with the whole mixture in the more crude and brutal fashion only as a source of fuel, lubricants, and constructional material.

The completeness with which the Author (and his collaborators) has fulfilled the title of the book is claimed in the following sentence from his preface: "Every effort has been made to make it complete and to include in it references to all published material of both purely scientific and industrial interest as well as to the vast amount of information contained in patents on the subject." An examination of this claim shows that very little has escaped him.

The whole tremendous subject is dealt with in fifty chapters, logically developed; and the reader has a pleasant feeling that the author's main object is to help him through the complication of the details supplied, rather than to do justice to the subject as such. All matters are simply and clearly described, and the 162 tables and numerous summaries are of continual assistance.

The author interprets his title widely, and covers the chemistry of production as well as the chemistry of use, whether this be of scientific importance or of practical value. Probably the most prominent amongst the interesting examples that can be quoted are those of the production of synthetic rubber, gum-formation in gasoline, and the production of new solvents.

The delay in the detailed scientific attack on petroleum has been the result of the great difficulty in the separation of the components in pure form. For this reason it is of great interest to have brought together the highly refined methods of laboratory fractionation and of gas analysis, amongst the other methods of examination and analysis that are given.

The richness of the whole book is augmented by the collection of references that approach 10,000, and cover work done in over fifteen "research countries." It is a remarkable tribute to the production of the book that no misprints have been noticed; and one feels almost apologetic in pointing out a single error (so far discovered) in the Index, where *Carbenes*, p. 671, has been omitted.

This book is an outstanding production, a most valuable conspectus of a vast and complex subject.

P. E. S.

The Hardness of Metals and Its Measurement. By Hugh O'Neill, D.Sc., M.Met. [Pp. xiv + 292, with 119 figures, including 24 plates.] (London: Chapman & Hall, Ltd, 1934. 25*s*. net.)

Dr. O'Neill has devoted many years to the study of the hardness of metals. It is therefore appropriate that he should have written this book. Hardness, like strength, toughness and ductility, is difficult to define, for like these it is not a simple property of metals but a term used to denote a particular aspect of their response to applied force. In general, it means response to a force applied in such a way as to produce indentation, and hardness tests are methods of making and measuring indentations. When force is applied in other ways different measurements are made, but as all the quantities determined by tests ultimately depend on the same fundamental attributes of metals, the results obtained by one method are not independent of those obtained by others. Thus the consideration of hardness leads to the general consideration of the relations between stress and elastic and plastic deformation and thence to the other manifestations of these relations, namely, tensile strength and ductility.

Of the nine chapters in the book three deal with the methods of measuring hardness, five with the relations between hardness on the one hand and general mechanical properties, treatment, composition, constitution and structure on the other. One deals with abrasion, machinability and cutting hardness. A large amount of useful information is brought together for the first time and this book should take its place along with the other monographs published in recent years for the use of those who are seriously interested in metals.

J. M. R.

Industrial Radiography. By Ancel St. John, Ph.D., and Herbert R. Isenburger. [Pp. x + 232, with 70 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 21*s*. 6*d*. net.)

This is a well illustrated account of the use of X-rays and radium in the examination of various structures and articles and particularly those pertaining to Engineering. Some common defects in castings, forgings and welds are described by the aid of radiographs or exographs as the authors of this book call them. There is no doubt that the development of radiography has placed a searching and potent weapon in the hands of the engineering inspector; moreover, the method is non-destructive and non-harmful. If this book has a fault it is that not sufficient emphasis is given to the limitations of the method. It must not be imagined that all faults to which metals are liable can be positively detected by radiography For example, hair line cracks which have very serious importance in steel structures are not generally detectable by this method. Further, although the method of shift or stereoscopic radiography has very valuable use in localising a flaw it must not be regarded as possessing absolute accuracy in all cases. Perhaps one of the most important applications of radiography is in the examination of welding, but here again perhaps the most serious defect to which welds are liable, that is non-penetration, may or may not be detected, depending upon the accessibility of the weld, its thickness, shape and so on.

Considerable space is devoted to a discussion of theoretical matters connected with X-rays and X-ray apparatus, which though they may have value and interest for the student who is beginning the study of radiography, they will not interest the engineer whose chief concern is to know what this new science can teach him concerning the faults and lesions in his materials and structures and how far he may rely upon its findings. An excellent bibliography of the subject is provided.

V. E. P.

A Manual of Foundry Practice. By J. Laing and R. T. Rolfe. [Pp. viii + 276, with 150 figures.] (London: Chapman & Hall, Ltd, 1934. 15*s.* net.)

This book, as its title indicates, deals with the subject of the art and practice of founding metals to shapes. A very praiseworthy effort has been made by the joint authors to contribute a manual for those interested in the founding of metals, in which both the science and the art underlying the successful production of castings is expounded and presented in a concise and lucid manner. Modern foundry practice is no longer wholly dependent for success upon the rule-of-thumb methods of past decades, although, admittedly, many of the best of such methods are still retained as being eminently practicable. Science, as a result of intensive research work, is continually yielding up many of its metallurgical secrets, and day by day scientific principles and control are being introduced to the foundry to the mutual advantage of both the supplier and the user of metal castings. It is from this aspect that the manual under review has been written.

The text is comprised of eleven chapters: I, The Principles of Moulding and Core-Making; II, Core-Making; III, Moulding Tackle; IV, Moulding Technique; V, Plate and Machine Moulding; VI, Moulding and Running in Relation to Design; VII, Loam Moulding; VIII, The Metallurgy of Cast Iron; IX, The Melting of Cast Iron; X, Special Methods of Production—Chill and Malleable Cast Iron; XI, Non-Ferrous Founding.

As will be seen from a glance at the foregoing contents, the authors have attempted to cover a very wide field—too wide, in fact. The founding of metals is too encyclopædic a subject to be adequately covered within the pages of one small volume. This is made apparent when one discovers, for instance, that the important subject of non-ferrous founding is dismissed in a single chapter of fifteen pages.

The manual may be regarded as a text-book for the foundry apprentice, and likewise for the assistants to the foundry metallurgist. The foundry craftsman will find much that is familiar to him in the daily routine of foundry practice, but will derive pleasure and profit from a careful perusal of this book.

W. L.

GEOLOGY

La Géologie et les Mines de la France d'outre-mer. By many authors under the auspices of the Bureau d'Études géologiques et minières coloniales. [Pp. viii + 604, with 38 figures and 2 plates.] (Paris : Société d'Éditions Géographiques, Maritimes et Coloniales, 1932.)

THIS finely produced and comprehensive work contains a full up-to-date summary of the geology and economic mineral deposits of all the French colonies and dependencies. It represents the first-fruits of a combined movement by the French Government and certain powerful industrial interests towards the fuller development of the mineral resources of French overseas possessions. A syndicate entitled *Comité d'études minières pour la France d'outre mer* was founded, and this in turn set up a *Bureau d'études géologiques et minières coloniales* which was charged with the investigations under review, in which Prof. A. Lacroix, the authority *par excellence* on the geology and mineralogy of the French colonies, has played the most important part.

The substance of this book was given in a course of lectures during the winter 1931–32 by its compilers It does for the French colonies what Reed's well-known book did for the geology of the British Empire, and brings together an immense amount of valuable but scattered information, especially in regard to the less well-known French possessions. It is good to have these compact accounts of the geology and mineral deposits of Algeria and Tunis (128 pp.), Morocco (52 pp.), French Indo-China (60 pp.), French West and Equatorial Africa (80 pp), Madagascar (50 pp.), and so on. Each chapter gives a geographical sketch of the region dealt with, followed by stratigraphy, igneous rocks, tectonics, and mineral deposits, closing with a useful bibliography. Included in this volume is a chapter on petroleum in French possessions (53 pp.). The two plates are geological maps in two colours of Northern Africa and of French Africa respectively, and most of the text figures are well-drawn geological maps and sections. Full subject and geographical indexes have been provided. This book represents a most valuable contribution to general regional geology as it provides information hitherto not readily obtainable regarding some geologically little-known countries.

G. W. T.

Les Ressources minérales de la France d'outre-mer. I. Le Charbon. II. Le Fer. By many authors under the auspices of the Bureau d'Études géologiques et minières coloniales. [Part I, pp. vi + 245, with 33 figures ; Part II, pp. iv + 436, with 59 figures.] (Paris : Société d'Éditions Géographiques, Maritimes et Coloniales. Part I, 1933, Frs. 24 ; Part II, 1934, Frs. 36.)

THESE volumes represent the first two parts of a work in four parts which is designed as a sequel to *La Géologie et les Mines de la France d'outre mer*, which is reviewed above. The substance of these compilations was given by the authors concerned in a series of lectures delivered at the Musée d'Histoire Naturelle in Paris during the winter 1932–33 under the auspices of Prof. Lacroix. In the first volume of this series, which was devoted mainly to geology, the accounts of mineral resources were necessarily condensed. In the volumes under review the information regarding mineral resources is greatly expanded in respect to their descriptive and economic aspects. The general plan of each contribution is as follows: character of the mineral deposits; forms in which they are usually found; deposits in the French colonies and in the world; metallurgy; uses; statistical data of production, consumption and markets.

As regards coal Algeria, Morocco and Indo-China are the chief centres of production, but coal also occurs in Madagascar and New Caledonia, although its economic possibilities have not yet been ascertained.

The second volume surveys ores of iron and of elements used in iron metallurgy such as manganese, chromium, nickel, tin, tungsten, graphite, beryllium, molybdenum, cobalt, titanium, and vanadium.

In both volumes an immense amount of valuable and out-of-the-way information hitherto scattered in numerous publications has been brought together in compact form. Economic geologists will find in them a rich quarry of material both for academic and industrial purposes.

G. W. T.

Outlines of Physical Geology. By C. R. LONGWELL, A. KNOPF and R F FLINT. [Pp. v + 356, with frontispiece and 297 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd, 1934. 18*s*. 6*d*. net.)

THE parent book, *Physical Geology*, from which this shorter version has been prepared, was reviewed in SCIENCE PROGRESS (Jan. 1934, 556). While this book is simpler and less comprehensive than the longer and more detailed work, some sections have been amplified so as to ensure greater clarity in treatment, and technical terms have been used much less freely. The number of chapters has also been reduced by combining subsurface waters, lakes and swamps in a single discussion, and by omitting a chapter on the interior of the earth, although some aspects of this subject are dealt with in connection with earthquakes and mountains. As in the larger work the illustrations are numerous, well-selected, and excellently reproduced. Many of them are quite new, having been specially made for this book. The new weapon of aerial photography which is so effective in geological illustration has been frequently employed to good purpose. The book is very well written and can be recommended to geological students as one of the best of recent texts.

G. W. T.

Dip and Strike Problems, mathematically surveyed. By K. W. EARLE, D.Sc., F.G.S. [Pp. x + 126, with 122 figures and 1 folding plate.] (London: Thomas Murby & Co., 1934. 12*s*. 6*d*. net.)

DIP and strike problems may be attacked by graphical or geometrical constructions, by trigonometrical methods, and by tables constructed from trigonometrical formulæ. The first method has been mainly used by practical geologists, but as the author of this book points out, this method involves a different construction and figure for each problem, and is susceptible of only an approximation to accuracy. Hence Dr. Earle has turned to the somewhat neglected trigonometrical methods of which the formulæ can be applied to each and every problem of the same kind. The book therefore provides mathematical solutions of various problems, including the calculation of true dip, the thicknesses of strata, dip and strike of underground seams encountered in bore-holes, lengths of headings and adits, axial plane directions of anticlines, throw, heave and hade of faults from surface and underground observations and from the displacement of surface outcrops in anticlines and synclines. Glancing through the work it seems to the reviewer that some of the formulæ are more complex than is necessary in dealing with variable data that are only roughly amenable to mathematical treatment. The author concedes that the formulæ, notwithstanding their exactitude, can only give approximate accuracy, and has attempted to eliminate error by smoothing out the variables in the data. Apart from inherent limitations which are frankly confessed by the author, the treatment is excellent, and we are particularly impressed with the value of the chapters on faults and faulted anticlines and synclines. The work concludes with a good glossary of structural terms, and a bibliography which might, perhaps, have included Bailey and Robin Willis's *Geologic Structures*.

G. W. T.

BOTANY

The Families of Flowering Plants. II: Monocotyledons. By J. HUTCHINSON, F.L.S. [Pp. xiii + 243, with 107 figures.] (London: Macmillan & Co., Ltd., 1934. 20*s*. net.)

IN this volume Dr. Hutchinson presents the Monocotyledonous families in a considerably altered arrangement approximating more closely to the modern concepts of affinity. The most drastic modification is the regrouping of the Liliales and the Amaryllidales with the Alstroemeriales as a separate cohort. The Amaryllidales are held to be characterised by the possession of an umbellate inflorescence, rather than by an inferior ovary, and, in accord with this concept, the Agapantheæ, the Allieæ and the Gilliesieæ are included in this group. The Alstroemeriaceæ are placed with the Petermanniaceæ and the Philesiaceæ. Great importance is attached to the differentiation of the perianth into calyx and corolla and the Butomaceæ are held to be primitive representatives of the series characterised by a homogeneous perianth, whilst the Alismatales constitute the original stock from which the types with a heterogeneous perianth are held to have been derived. The Orchidales are here related to the Hæmodorales directly and to the Burmanniales indirectly.

As in the previous volume the author has increased the number of families by subdivision, but since the presumed relationships are clearly indicated the disadvantages of the multiplication of groups is largely overcome. The numerous figures include about a dozen distribution maps together with habit drawings and dissections of characteristic representatives of many of the families.

The book is a thoughtful and original contribution to the taxonomy of the monocotyledons, and although exception will doubtless be taken to some of the author's innovations, students cannot fail to find the work both helpful and stimulating.

E. J. S.

The Gramineæ. A Study of Cereals, Bamboo, and Grass. By AGNES ARBER, M.A., D.Sc. [Pp. xvii + 480, with coloured frontispiece and 212 figures.] (Cambridge: at the University Press, 1934. 30s. net.)

IT is with anticipatory pleasure that we open a new work from the pen of Mrs. Arber, and these pages are written with her customary lucidity and charm of style.

There are, at present known, over 8,000 species of wild grasses, whilst of the cultivated cereals it has been estimated that the hereditary strains of wheat number millions, and over 2,000 varieties of rice have been recognised from the Philippines alone.

With such a plethora of material it is scarcely surprising that, despite a distinctive and not very variable vegetative morphology, the grasses as a whole present a range of anatomical structure, of reproductive morphology and biological equipment that involves a large proportion of the problems which the higher plants offer for solution. Indeed, the grasses constitute so important a group, whether considered from their botanical or economic aspects, that any treatment of them within the compass of a single volume must necessarily be selective in its scope.

Beginning with an account of the cultivated cereals the reader is here led on to that "Pooh Bah" amongst the grasses of economic importance, the Bamboo, to which no less than four chapters are allocated. In this connection the author's views on trees and shrubs, published in 1928, are here repeated as Chapter V. The view expressed that the essential feature of the tree "is not so much its size as the woodiness or fibrousness that means strength" is open to considerable objection. In our view the fundamental quality of trees, which they share with shrubs, is the perennation of their aerial shoots. Woodiness is a quality found alike in trees and herbs, though in the latter in very varying degrees. It is the perennation, combined with increase of size, that creates the essential physiological problems of the tree habit, problems that are only partially shared by the giant herbs of the tropics such as the Banana.

The argument for the derivation of trees from herbs based on the Leguminosæ appears to us equally fallacious. Mrs. Arber would have us believe that the less-specialised flowers of many woody representatives of this family are the consequence of a lag in the rate of evolution as compared with that of the herbaceous genera with their earlier maturity and more frequent generations. But Mrs. Arber nevertheless assumes that these same woody types, which have been so laggard in floral evolution, have not been prevented from evolving the woody habit from herbaceous ancestors. But such a lag should affect all types of evolution. Little as we know concerning the rate of this process, the comparative constancy of anatomical characteristics and frequent variability of floral morphology within a group suggest that the rate of evolution of the latter is more rapid than of the former. Furthermore, the validity of this assumption of an evolutionary lag in arboreal types is itself open to some doubt since evolution depends not merely on the number of new types but on their successful establishment and dispersal. If a tree takes longer to attain maturity and produce offspring than a herb, which is not always the case, such a difference is perhaps more than compensated for by the vastness of its productivity. One would require evidence that annuals were, in general, more specialised than related herbaceous types with delayed maturation, before one could concede any significant importance to this factor.

If the Bamboos have a primitive floral organisation owing to a slow rate of evolution, their woodiness might well be due to the same cause, if we concede the primitiveness of the fundamental physiological qualities which are involved in the perennation of large aerial shoots to which certain anatomical peculiarities are incidental.

In these pages the more ecological aspects of the Gramineæ have been intentionally omitted and little is included respecting their physiology, of which not the least remarkable feature is a capacity for absorbing and secreting such large amounts of silica. How and in what form this silica is absorbed and trans-located offers one of the most interesting problems in plant physiology.

A great deal of the text is devoted to the features of reproductive morphology which the grasses present in such variety. Mrs. Arber evidently has a passion for the microtome and, both actually and metaphorically, she often presents to us a succession of two-dimensional slices when we should have welcomed a three-dimensioned whole. The work throughout is profusely illustrated. The 212 figures which occupy some 160 pages actually comprise a very much greater number of separate drawings, but it is no exaggeration to say that over 60 per cent. of these are representations of microtome or other sections. The three-dimensional figures are not only comparatively few in number but regrettably small.

The work is limited both as to its scope and mode of treatment, but within these limitations Mrs. Arber has provided an extremely readable account which all can peruse with profit and interest.

E. J. SALISBURY.

Hooton Pagnell. The Agricultural Evolution of a Yorkshire Village. By ARTHUR G. RUSTON, D.Sc., B.A., and DENIS WITNEY, B.Com. [Pp. viii + 459, with 12 plates and 58 figures.] (London: Edward Arnold & Co., 1934. 25*s*. net.)

DR. RUSTON and Mr. Denis Witney have done well to put together the great mass of material they have accumulated in regard to the little village of Hooton Pagnell in Yorkshire. Many people have never heard of it: a discerning few know it as the home of Sir Geoffrey Luterel who, in the early part of the fourteenth century, arranged for the production of the beautiful Luterel Psalter which is one of the treasures of the British Museum and one of the best sources of information about medieval agricultural implements and cultivations. Apart from this the village has no special interest; it has played no part in history and so, according to the cynic, ought to have had a happy life—which probably it has. Certainly it has had a long life: it was already somewhat important at the time of the Doomsday survey and it has never greatly changed its status. It is a good representative English village, with its long continuous history, its gradual evolution and its marked power of survival through all the changing circumstances of the passing years. The Saxon settlements have had remarkable vitality; a large number of our towns and villages bear Saxon names, and the enormous growth of population in the eighteenth, nineteenth and twentieth centuries usually meant only an expansion and joining up of ancient settlements, not the creation of new ones. It is this feature of continuity that gives the English village its character: its solidity and durability stand as an enduring memorial to the humble folk who lived there, and as a faithful expression of their character.

Townspeople coming to live in the village do not always recognise this fact. They are made to feel that they are foreigners, visitants from some outside world, and this impression persists till they have aligned themselves with the general current when they, in turn, will regard the new-comer as in some sense an intruder.

Dr. Ruston's assiduity in economic investigations is well known, and it has never been better exemplified than in this painstaking study which he and Mr. Witney give us of the village. Its course is traced from Norman times to the present day: even the changes in field boundaries are recorded. Particulars are given with maps, showing the various enclosures and the evolution of the farm. There are some interesting curves setting forth the relation of price of wheat to enclosure from 1780 to 1820, and the relation between prices of produce, wages, and rent. These show that the agricultural worker is considerably better off now than ever before, for his wages stand at 90 per cent. above the pre-war level, while agricultural prices have risen only 5 per cent. The landowner got nothing out of the period of high prices and has had to carry out his various obligations at enhanced cost.

The Land Tenure and Tenant Right problems of the village are traced from their beginnings. The book closes with a long chapter on Tithe and Glebe which is of particular interest in that Hooton Pagnell is free from tithe troubles that occasionally flare up in the eastern and southern counties: the authors show how this has come about.

The authors have wisely given long extracts from contemporary documents; in consequence the book has value as an example of the sources from which village history is drawn. No small part of its merit lies in the fact that it shows students of economic history, of geography and of agriculture how to set about exploring the history of any village in which they may be interested.

E. J. Russell.

ZOOLOGY

Anatomy of Animal Types. For Students of Zoology. By E. A. Briggs, D.Sc. [Pp. xx + 250.] (Sydney: Angus & Robertson, Ltd.; London: The Australian Book Co., 1934. 10*s*. 6*d*. net.)

This volume consists of careful descriptions and instructions for the dissection of a number of types of animals, ranging from Amœba to the rabbit. While this selection is no doubt admirable for Australian zoologists, their British colleagues will have less opportunity of deriving profit from this book since, of all the types with which it deals, only about half a dozen are to be found in this country.

The absence of any member of the Annelida is surprising, as is the omission of any reference to Nephridia. The absence of any figures may be useful in the long run, by forcing the student to make his own drawings and not to copy from those of the book, but it makes the morphological relations more difficult for the beginner to grasp.

G. R. de B.

The Anatomy of the Rhesus Monkey (Macaca mulatta). By numerous authors. Edited by CARL G. HARTMAN and WILLIAM L. STRAUS, Jr. [Pp. x + 383, with frontispiece and 128 figures, including 6 plates.] (London: Baillière, Tindall & Cox, 1933. 27*s*. net.)

THIS attractively produced and beautifully illustrated book is nothing less than a monograph on the anatomy of the Rhesus monkey, by a team of nineteen authors, compiled as a handbook to meet the increasing use of this type as a laboratory animal in America. This careful study of an interesting member of the human family is a valuable addition to the small existing list of special anatomies, particularly as the morphological aspect is kept well in mind.

G. R. DE B.

Modes of Behavioral Adaptation in Chimpanzee to Multiple-Choice Problems. By ROBERT M. YERKES. Comparative Psychology Monographs, No. 47. [Pp. 108, with 8 figures.] (Baltimore: The Johns Hopkins Press, 1934. $1.50.)

A Field Study of the Behavior and Social Relations of Howling Monkeys. By C. R. CARPENTER Comparative Psychology Monographs, No. 48. [Pp. 168, with 3 figures and 16 plates.] (Baltimore: The Johns Hopkins Press, 1934. $2.25.)

THESE are two contributions from the Yale Laboratories of Comparative Psychobiology, the first being carried out at the Experiment Station in Florida, the other on the Barro Colorado Island in Lake Gatun, Panama. It is convenient to consider both together, for, as Yerkes points out, field studies and experimental researches in animal behaviour should go hand in hand.

Yerkes' contribution is a long delayed sequel to his well-known and classical study of the mental life of monkeys and apes published in 1916. He shares with Kohler the honour of having initiated a new and fruitful line in the investigation of anthropoid behaviour. Experiments have been carried out by the " multiple-choice " method with success on several species of birds and mammals, and it has certain advantages for the study of " relational learning." The general plan of such experiments is as follows. A number of identical compartments are provided, one of which is the way out to reward and freedom. The correct compartment is not the same in consecutive tests, but has a definite place in a group, and the group itself is not fixed. Thus it may be the right-hand one of any set of compartments, or the middle one, or the second from the left, and so on. It will be seen that the problems set are difficult ones, and it is no surprise to find that even chimpanzees take a considerable time to solve them and not infrequently fail. Success gives evidence of the animals' ability to grasp in practice abstract relations. Yerkes' experiments were carried out with four young chimpanzees, and altogether six different problems were given them. He found that in nearly 50 per cent. of the successful trials the solution came by sudden " discovery " of the essential relation, and was fully retained in control trials. He holds that discovery is not necessarily present in learning, but that the solution of relational problems by the chimpanzee is not usually achieved without it, even in cases where the solution appears to come gradually. In this power of discovery he sees the germ of " those expressions of human curiosity and originality which we call invention and research." He found the behaviour of his chimpanzees curiously unpredictable, and he noted that affective dispositions were all-important conditions of success. The monograph is important, and deserves careful study by the professional student of behaviour; in particular, the discussion of the various processes and activities at present lumped under the name of "learning" is worthy of close attention.

Dr. Carpenter's interesting study of the natural behaviour in the field of the Howling Monkey (*Alouatta palliata*) is the third in a series of outdoor studies of primates in the wild, following upon Dr. Nissen's study of chimpanzees in French Guinea and Dr. Bingham's paper on the gorilla in the Belgian Congo.

The Howlers are arboreal, inhabiting a definite ecological niche or norm, namely the dense, tall primary forest; they rarely descend to the ground. They live in small groups or clans, each having a fairly definite territory of considerable extent, through which they wander by regular arboreal pathways, from one favourite spot for feeding or sleep to another. The clans interfere very little with one another. They vary considerably in numbers of individuals—from four to thirty-five—and consist of males, females with young and females without young. The females greatly outnumber the males. The clan leads a peaceful communistic life; fighting among the adult males is unknown, and they co-operate in leading the clan from one place to another. There is apparently no competition for sexual dominance, and mating seems to be communal. In these respects the Howlers differ considerably from other monkey societies that have been described.

Carpenter gives a very interesting and detailed account of the behaviour of the young, the mothers, and the other members of the clan. Though relatively indifferent to the young, the adult males assist in protecting and retrieving them. At least nine different kinds of cries can be clearly distinguished; the situations eliciting each, and the responses made are summed up in a Table on pp. 108–9, which gives a vivid picture of the Howlers' daily life.

E. S. Russell.

Adam's Ancestors, An up-to-date outline of what is known about the origin of Man. By L. S B. Leakey, M.A., Ph.D., F.S.A., F.R.A.I. With a foreword by Sir F. Gowland Hopkins. [Pp. xix + 244, with 12 plates and 30 figures.] (London: Methuen & Co., Ltd., 1934. 7*s*. 6*d*. net.)

In this little book Mr. Leakey has attempted to cover the whole field of early prehistoric archæology, including both the cultural and the physical side. In addition he gives briefly various personal reminiscences in regard to his discoveries in East Africa. These are all too brief and form by no means the least interesting part of the book. The archæological section is of particular value. Mr. Leakey discusses the whole question of the implements of early man, both from the point of view of chronology and also of identification. He stresses the importance and necessity of actually working in flint because, as Mr. Henry Balfour has pointed out, it is impossible to have a proper understanding of flint implements without understanding the properties of the flint itself. The sketches which illustrate this section are specially to the point. The chronology embodies all his researches into this complicated matter. It is a pity in many respects that Mr. Leakey did not confine himself to this side. It is here that he has had practical experience and can speak with authority. His discussion of the physical side is less happy, and there are a number of statements which are either given without any evidence at all to support them, or else need considerable qualification. What evidence, for instance, can Mr. Leakey produce to show that there is ever mating between man and the anthropoid apes? Or again he says, "The *shape* of the skull is not regarded as of any great importance from the point of view of deciding whether a skull is of *Homo sapiens* type or not." By shape in this connection he appears from the rest of the paragraph, to mean cephalic index, but this after all is a crude way of measuring shape and if the altitudinal index, which he does not mention, is taken into account the difference is very marked. Again in discussing Piltdown man he only mentions, in a footnote, that a tooth was found in 1917. He does not attempt to say what this tooth is or its importance. In the text he postpones most cautiously and wisely the questions of reconstruction, but gives us a plate showing a reconstruction without any comment. In fact in contradistinction to the clear and valuable annotation of the plates and drawings in the archæological part those in this part are not well documented. Plate IV, for instance, has the legend "the *canine fossa* in *homo sapiens* and its absence in other types" without as far as can be seen any indication what skulls are figured. Here of course we are up against the fundamental difficulty of the popularisation of modern science, the whole matter is becoming so specialised that each branch needs covering by an expert, whereas what is wanted in a popular book is exactly what Mr. Leakey has put before himself to accomplish, namely a wide and comprehensive exposition of the whole subject. Mr. Leakey promises us a more specialised work on his discoveries in East Africa which we shall look forward to with great interest.

L. H. D. B.

The Ancestry of the Long-Lived. By RAYMOND PEARL and RUTH DE WITT PEARL. [Pp. xiv + 168, with frontispiece and 20 figures.] (Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. 13*s*. 6*d*. net.)

FOR this study the authors consider those who live to be 90 years as long-lived, and are concerned with the problem as to why certain persons live to this age. In considering the ancestry they use the "Total Immediate Ancestral Longevity" (abbreviated to T I A L) which is obtained by taking the sum of the ages at death of the six immediate ancestors of an individual, that is to say of the two parents and the four grandparents.

The data used come from two sets of material: (1) A collection of case histories of extreme longevity (Long. Series) collected by the Department of Biology of the School of Hygiene and Public Health of the Johns Hopkins University, and (2) the Family History Records (F H R Series) collected by the same department.

In the first series those were included as subjects who could give the age at death of their six immediate ancestors, and since the subjects were at least 90 the ancestors must have been dead at the time of the enquiry; there were 365 such cases.

In the second series, the F H R, all families in which both parents and all four grandparents were dead and had the age at death recorded were selected. The subject or *propositus* was the oldest living individual in the sibship; 136, and in some tables 143, persons were studied. The ages of the subjects in the two series never overlap, the range in the F H R series being from 10 to 90 years. In Chapter IX the authors point out that in the F H R series when families are chosen at random as regards age, but with the requirement that all six immediate ancestors must be dead, then the age at death of the parents is not random, but in a proportion of cases parents must have died young for their families to be included. In the Long series there will be no selection, since all ancestors have had an equal chance of dying, but in the F H R series when the subject is young, only those can be included whose parents had died young and there is selection.

The authors discuss the influence of birthplace and race stocks, and of alcoholic habits of the group of long-lived persons; the numbers are too few to give any information as to longevity among those who were foreign born, since there are only 43 of both sexes divided among 14 different countries. With regard to the association of longevity and the taking of alcohol the authors find no significant difference in the percentage of "moderate" and "very moderate" drinkers in the two series, but that there are fewer total abstainers and more heavy drinkers in the F H R series.

The distributions of T I A L given in Chapter VI are interesting, showing the wide range of years that the summed duration of the lives of one's parents and grandparents may take; the mean T I A L in the Long. series is 446·14 years, and in the F H R series is 385·43 years, but how much of this difference is due to the method of selection in the F H R series, and how much is due to the inheritance of longevity it is impossible to say. It is difficult to see how these "purely arithmetical consequences of the selection" can be overcome; in the case of grandparents the trouble is less, but in the case of parents the method of selection seems to us to make any conclusions as to the inheritance of longevity impossible. From Table 16 it will be seen that each of the grandparents of the long-lived subjects live on the average 8 years longer than those of the F H R series, and though possibly a year or two must be allowed for the method of selection in the F H R series, there seems here some evidence of the inheritance of longevity.

The parent-offspring correlation in the Long. series is low, and since the subject is not used in obtaining the coefficients given in Table 27 the low value cannot be attributed to the stringent selection of nonagenarians. In the F H R series it is not surprising to find low and even one negative though insignificant correlation, owing to the way the data were chosen. The same applies to the matings that produced the long-lived. There were over seven times as many matings of the type short-lived by short-lived involved in the production of the propositi in the F H R series as in the production of the propositi in the Long. series but such is to be expected since the majority of the propositi, probably all in the first two age groups, could not have been included in the data studied unless both their parents had been short-lived.

Though some of the conclusions reached may eventually be found to be correct, we do not consider that all are proven by the material provided in this book.

E. M. E.

Report on the North Sea Cod. By MICHAEL GRAHAM, M.A., Naturalist on the Staff of the Ministry of Agriculture and Fisheries. Fishery Investigations, Series II, Vol. XIII, No. 4, 1933. [Pp. 160, with 33 figures.] (London: H.M. Stationery Office, 1934. 7*s*. net.)

THE North Sea Cod Fishery has been specially studied in the Fisheries Department for some years, and the present report is a general survey of all that has been done. It contains much interesting and important information, including details of the methods employed, studies in growth of young and old fish, assessment of broods, fluctuations in brood strength and in stock, the life cycle of the cod in the North Sea and predictions of the cod fishery.

The English cod fishery in the North Sea is worth rather more than a million pounds annually. In describing the fishery, it is shown that for many purposes we can accept the statistics of Grimsby as representing a fair sample of the whole North Sea stock. With regard to the main economic question, it is found that the proportion of comparatively large fish caught is greater in the cod than in the haddock, probably owing to the fact that the cod is naturally partly protected for a period before maturity. The reason being chiefly their relative disperse distribution, but also the partial sanctuary afforded to feeding cod in the rough ground where it is difficult to fish, or possibly to their following the herring, on which they feed largely, into the upper waters out of reach of the trawl.

It is concluded that the protection of spawning cod would have no effect on the subsequent yield of their descendants, and that the stock is in no apparent danger of further diminution. Predictions as to the average yield of the fishery may now be made with some precision.

M. V. L.

The Generic Names of British Insects. Prepared by the Committee on General Nomenclature of the Royal Entomological Society of London, with the assistance of the Department of Entomology of the British Museum (Natural History). **Part I: Recommendations relating to the Publication of the Committee's Reports. Part II: The Generic Names of the British Rhopalocera,** with a check list of the species, by FRANCIS HEMMING, C.B.E. (London: Royal Entomological Society of London, 1934. Part I (pp. 6), 6*d*.; Part II (pp. 40), 3*s*. 6*d*.)

NOMENCLATURE in Biology is a special field in which those who work seem to claim an independence of other lines of research. They rightly say that it is the foundation of the Science, but as they are always rearranging the units, in blind obedience to the rule of priority in nomenclature, regardless of the complications resulting in the literature or of the inconvenience caused to those who are not systematists, the foundation is so unstable that attempts have been made from outside the ranks of the systematists to throw over the rule of priority and fix names once and for all. In 1928 the British Zoologists, at their Annual Meeting, with two dissentients, passed a resolution "that this meeting of British Zoologists is in favour of a substantial increase of the lists of *nomina conservanda*," but there the matter ended. In 1933 the same subject was brought up again before the Association of British Zoologists and also before the Royal Entomological Society of London. In both cases the matter was referred to the respective Councils and both Councils appointed a Committee to go into the matter with a view to seeing what could be done. The

Committee of the British Zoologists, perhaps too heavily weighed down under the sacred rule of priority, reported to their Council that the matter was not urgent except in Entomology, while the Entomological Committee laid before their Council a scheme upon which, they suggested, useful work could be carried out. *The Generic Names of British Insects*, of which Parts 1 and 2 appeared early this year, is the result of the scheme approved by the Council, a scheme in which the Department of Entomology of the British Museum (Natural History) is assisting and which has involved the setting up of a special Organising Committee and of sub-committees for each Order of insects including representative experts on the different groups.

It was at first intended to produce lists of species only where it was thought to be of special advantage, but as the Association of British Zoologists is at the present time preparing a work upon the British Fauna, which, it is hoped, will be published by the Trustees of the British Museum (Natural History), it was felt that odd lists published by the Royal Entomological Society, probably by the same specialists as are producing the lists for the other work, would be absurd and therefore an arrangement was come to between the two bodies by which the Association of British Zoologists agreed to omit Entomology from their syllabus on the understanding that the publication of the Royal Entomological Society would include lists wherever possible. Part I under review is entitled *Recommendations relating to the Publication of the Committee's Reports* and is a series of rules drawn up by the Committee for the guidance of authors contributing to the succeeding parts. Part II, on the British Butterflies, is the first part of the actual work and Mr. Hemming has set an example of the way in which such work should be carried out. The object of the work is to fix the names of genera and genotypes (and the list will also fix the names of species), and it has been recognised by the Committee that only upon certain lines is there any hope of such names being generally accepted. These lines are first, that the rule of priority must be followed where any consequent change of name does not seriously affect the literature, and secondly, that where a name, which is not the original one, has long been in use, a proposal for the suspension of the law of priority and the fixation of the name as a *nomen conservandum* shall be made through the Council of the Society to the International Commission on Zoological Nomenclature.

That the work should be universally accepted without criticism is not to be expected, but it is hoped that evidence of the thoroughness of the work of the author, chosen because he is an authority and not a crank, will induce those systematists endowed with a modicum of common sense to accept the decisions, backed as they are by the Royal Entomological Society and the Department of Entomology of the National Museum. It may deprive a few systematists of the pleasure of further juggling with names, but it will be an immense boon to the large majority of Entomologists all over the world to have a standard work upon entomological nomenclature.

FRANK BALFOUR BROWNE.

MEDICINE

The Doctor and His Patients. By ALBERT KRECKE. [Pp. vi + 301, with 1 plate.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1934. 10*s*. 6*d*. net.)

THE essential character for a good doctor is a "good man." The sincere tribute by Dr. Fritz Lange appended to this book is the clue to the chapters which precede it. Kreoke was a "good man." The sound precepts expressed for the moral and professional guidance of physicians and surgeons in their relations with patients and colleagues, though many may appear as simple and obvious platitudes, cannot be disregarded by any who have respect for the honour and high standing of the healing profession. To lay readers who are potential patients, the book should be of interest and value, and should help to create a much desired better understanding between them and their doctor. The dominant notes should always be human kindness and mutual sympathy; the doctor's first thought the welfare of his patient.

The book is a translation from the German second edition of 1932 by Margaret M. Green and has an easy fluency. A tendency to repetition is perhaps unavoidable in a collection of scattered thoughts, gathered it would seem from the commonplace book of a very occupied surgeon, whose life was wrapt in intense enthusiasm for his work, but this will only serve to point the principle on which he would insist.

In the present day when the "general public" expresses so often a strange ignorance of common ethics, and an amused contempt for what it calls "professional etiquette," it is to be hoped that many will read this book and learn to become "good men and women."

P. J.

The Principles of Heating and Ventilation. By H. M. VERNON, M.A., M.D. [Pp. viii + 232, with 52 figures.] (London: Edward Arnold & Co., 1934. 14*s*. net.)

DR. H. M. VERNON has written a useful book on the principles of heating and ventilation, the practice being dealt with so far as necessary to illustrate the principles. In considering heat loss from the body, the use of the kata-thermometer is made evident, as a measure of cooling from and velocity of air movement, and the practical application of this instrument illustrated by observations made by the author and his co-workers in factories.

Dr. Vernon says "It is a pure chance that the rate of cooling of the instrument happens to yield a fairly good index of the sensations experienced by human beings," but as the cooling powers selected as standard ones, were in the first case fixed by the inventor (Sir Leonard Hill) in conditions which were acknowledged to be comfortable and fresh, there was no chance in the matter. The combination of the three factors, air temperature humidity and air movement into a scale of "effective temperatures," has been worked out in the Research Laboratory, Pittsburg. The author, by his own studies, has reached the conclusion that the cooling power of the air as estimated by the dry kata-thermometer generally affords a more accurate index of comfort than the effective temperature scale.

The kata-thermometer, he says, appears to take too much account of air movement. While the effective temperature scale tends to take too much account of air temperature. The effects of clothing are next dealt with, and in still air thick winter clothing is found to have a heat retaining power equivalent to 22–23° of air temperature. In indoor work with little air-movement it is most important, to prevent over-heating, to reduce air-temperature and clothing. The Eupathioscope and the Globe thermometer are considered as gauges which take into account the important factor—radiant heat. By combined use of wet and dry bulb thermometers, the kata-thermometer and the globe thermometer the factors which influence body heat loss are evaluated. The importance of the character of the rays in affecting skin sensations, and reflexly the nasal mucous membrane, has recently been emphasised, but the author does not touch on this. He deals in successive chapters with the loss of heat from buildings, heating by open fires and gas and electric fires, hot water and steam radiators, low temperature sources of radiant heat, and low temperature heating by electricity. The author says that the use of the last for continuous heating, even when electricity is bought at the non-peak period for as little as 0·27*d.* per unit, does not compare favourably, on a fuel basis alone, with coke or oil. With a price as low as 0·12*d.* a unit there would be no difficulty in justifying the use of electricity instead of any other fuel. This price is far above the ordinary present price paid by the householder. The considerations of panel ceiling heating, and under floor heating, as in Liverpool Cathedral, are of particular interest.

In the case of small high temperature electric heaters placed in the ceiling, the warming effect is found to be chiefly due to convection; therefore, when the windows are kept open in a school so warmed, the fingers of the children tend to become too cold for them to do their work. In the final chapters Dr. Vernon considers ventilation, and discusses natural and mechanical means. He deals with the control of humidity and conditioning of air, and with the special ventilation required in industry to remove harmful dust. This book will be a great assistance to architects, factory inspectors, medical officers of health and sanitary officials, and will interest those of the general public who seek to secure the best conditions in home and work rooms.

L. H.

PHILOSOPHY AND HISTORY OF SCIENCE

The Organism of the Mind. An Introduction to Analytical Psychotherapy. By GUSTAV RICHARD HEYER, M.D. Translated by EDEN and CEDAR PAUL. [Pp. xiii + 271, with 37 illustrations.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1933. 15*s.* net.)

THIS volume consists of a series of lectures, apparently delivered to mixed audiences, and deals with a number of " organ neuroses " and methods of psychotherapeutic treatment. Its aim, as stated by the author in his preface, is a modest one. He wishes to indicate " what psychotherapy is, why and how it is practised, and to give pointers along the roads of psychotherapeutic thought." Dr Heyer is in the main an " analytical " psychologist, though he borrows also from other schools and, while critical of some of their teaching, shows himself always to be sympathetic with them.

The chapters of Part I of the book treat of the vegetative, animal and " pneumatic " cycles of life in a highly original way, and show how the " mental vital cycle " may influence the lower functions, chiefly by way of autosuggestion, " imagination " and abreaction. This illustrates the power that the mind has over the body. Part II is concerned with psychotherapeutic procedures; and in it the theories of Freud, Adler and Jung are examined in some little detail. There is a final section in which thirty-seven plates—" accurate reproductions of pictures drawn by the subjects' unconscious "—are presented, together with explanations on the lines of symbolic interpretation. These are interesting but, it must be said, somewhat far-fetched.

The Organism of the Mind is suggestive, erudite and well written; though, in common with much of this kind of literature, it loses convincingness because of the wealth of metaphor, allegory and (though the author dislikes the term) mysticism with which it abounds. But the sort of empirical method employed in such studies naturally lends itself to treatment of this kind. Indeed, Dr. Heyer believes that "the symbol is far more convincing than the shrewdest of deductions and logical demonstrations." One might wish for a less dangerous (because so full of possible pitfalls) avenue of approach to an art which ultimately must be based on the soundest of scientific foundations.

The translation, as one might expect, is in excellent English and style.

F. A.

The Endless Quest. Three Thousand Years of Science. By F. W. Westaway. [Pp. xix + 1080, with Frontispiece, 50 other plates (3 coloured) and 193 figures.] (London and Glasgow: Blackie & Son, Ltd., 1934. 21*s*. net.)

This is a history of science for the general reader; and it is a critical history in which the reader is enabled to assess the evidence on which scientific theory is built, with some pertinent remarks on the quicksands of relativity and those who have been lured into them. But it is primarily a history of science, and as such it will appeal not only to the general reader, for whom it was written, but also to many scientific readers, since we are all nowadays specialists in one science and general readers in others. Moreover, it shows to some extent how essential the history of science is to history in the broader sense, especially to the history of civilisation.

Mr. Westaway begins with the empiric knowledge of the earliest civilisations, passes on to Greek and Roman times and the Renaissance, describes the rise of modern science from the sixteenth century onwards, and then outlines the various developments that bring his narrative up to the present day. The book is written in a very interesting way with adequate biographical detail, well illustrated and thoroughly indexed. A list of books for further and more specialised reading is given at the end of each chapter.

Among other points, *Skeptical Chymist* (p. 279 and elsewhere) should read *Sceptical Chymist*; the account of Priestley's methods (p. 299) is erroneous (see this journal, 1933, **28**, 17); Dalton's analyses of olefiant gas and marsh gas did not play the part suggested (p. 313) in the formulation of the Atomic Theory; "hydrochlorine" is misprinted for "hydrochloric" on p. 658; and the reference (p. 834) to Plate 44 (portraits of four distinguished scientists, one still living) as consisting of photographs of four of McGregor's restorations of fossil men is unfortunate, even if the details of the paragraph provide the attentive reader with some unexpected amusement.

Mr. Westaway's book is to be recommended; it should certainly be in every school and college library to enable the intelligent student of history to obtain some knowledge of his subject, some acquaintance with the history of civilisation instead of mere political history.

D. McKie.

Combustion from Heracleitos to Lavoisier. By Joshua C. Gregory, B.Sc., F.I.C. [Pp. vii + 231.] (London: Edward Arnold & Co., 1934. 10*s*. 6*d*. net.)

Mr. Gregory, already known as a writer of historical works on chemistry, here presents a study of the history of human thought and ideas on the problem of fire and flame. He begins with a detailed account of the premature notions of the Greeks (pp. 51); and the four chapters that are devoted to this will be found of considerable use by those who wish to obtain a proper understanding of early chemical theories. Then he passes on to the development of the idea of an inflammable principle, to the history of the study of the gain in weight of metals on calcination, the first work on combustion in closed vessels, the rise of the phlogiston theory and, finally, to the work of Black, Priestley, Scheele, Lavoisier and Cavendish.

As a study of the various explanations of fire advanced through the ages, this book is an interesting contribution to the history of science. Mr. Gregory rightly claims that his survey "displays the constant interplay in which preconceptions are applied to data and data to preconceptions. It exhibits the significant rôle of the preconception as a directive of thought —a rôle in which it may be a misleading prepossession or an insistent notion or a fertile guiding hypothesis."

Boyle's *Sceptical Chymist* (p. 60) is wrongly stated to have been first published anonymously: and the title of Boerhaave's classic should read *Elementa Chemiæ* (p. 66 and elsewhere).

But—if only Mr. Gregory would write differently! "Weighty argumenters," he says, "were then preferred to lighteners of the metal which weighted the calx by withdrawn levity" (pp. 115–16); and there is more of it. Nobody *thinks* in this way: why therefore *write* in it?

D. McKie.

Whither Medicine? By Josef Loebel, Dr. Med. Translated by L. Marie Sieveking and Ian Morrow. [Pp. xii + 296.] (London: Sidgwick & Jackson, Ltd., 1934. 7*s*. 6*d*. net.)

This book is an English version of Dr. Loebel's *Medizin oder dem Manne kann geholfen werden*, an historical record of the progress of medical science and art through the ages from the primitive times of black magic to the aseptic surgery, immunology and psychoanalysis of the present day. It is intended for lay readers and is written in modern journalistic style, technical and scientific terms and controversy are for the most part avoided, being replaced by illustrative, sometimes rather forced analogies, and quotations from medical and general literature. The field covered by the sixteen chapters is very comprehensive, but a cunning plan of linking the different sections into a continuous story emphasises the evolutional progress and serves to maintain the interest and unity of the subject. By practising the principle that "only the half truth is simple, the whole truth is the most complicated thing in the world," the author has provided an easily digested dish from very rich food. The chapters seem to us uneven in value; perhaps "pathology" and "physiology" will make the greatest appeal and be most interesting and profitable. The answer to the title "Whither Medicine?" and the whole moral of the tale is "The Science of Man." It is a book for which there should be a wide demand, and should attract and interest a large number of non-medical readers.

P. J.

Keith Lucas. By various contributors. [Pp. 131, with portrait-frontispiece and 1 text-illustration.] (Cambridge: W. Heffer & Sons, Ltd., 1934. 5*s*. net.)

Keith Lucas died in 1916, at the age of 36, as the result of an aeroplane accident, and Sir Walter Fletcher soon afterwards began to collect material for a book about him, and about his work in the two spheres of physiology and of aviation. Unfortunately, Sir Walter found no time, in his subsequent busy life, to edit this material and to publish it, and the book under review is composed of these unedited notes on different phases of the life of Keith Lucas, together with a section on his more strictly physiological work, written by his friend, Prof. E. D. Adrian.

The various parts of the book are, therefore, related, but are not, probably in the final form in which Sir Walter Fletcher would have cast them, had he lived to enjoy greater leisure.

Even with this qualification, however, the book is most welcome. The younger physiologists know of the work of Keith Lucas, and respect the clarity of expression and of design which it possesses; they can also see what an advance it was on that which preceded it. But the very scientific austerity which ensured these effects in his published work necessarily concealed somewhat the personal picture of the man himself, and it remained the duty of his more intimate friends to reveal this side, and so to complete the story.

Personality is an intangible, transient thing, which must be captured and set up in print, if it is to be given a chance of survival; even then, it is not too easy to communicate it adequately to future generations. The eight contributors, who have attempted the task in the case of Keith Lucas, have laid under their debt both the younger physiologists of to-day and the historian of physiology, while those who are interested in the progress of aviation will find that a number of very important advances were due to the same talents, which before 1914 had so richly served physiology.

K. J. F.

MISCELLANEOUS

General Sociology. By Henry Pratt Fairchild. Wiley Social Science Series. [Pp. x + 634, with 8 charts.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 23s. net.)

Professor Fairchild is frank enough to admit that the launching of one more sociology text-book upon a congested academic sea requires explanation. And this he does by pointing out that his aim was not the presentation of new subject-matter, but the treatment of what is generally understood by Sociology, from an angle that would make more readily comprehensible its basic principles. In this task he has admirably succeeded. Without omitting difficult problems or unduly simplifying their solution, Prof. Fairchild has here produced a book that should be of great value to the beginner as well as to the more advanced student.

In line with the founders of modern Sociology, Comte and Spencer, the author regards Sociology as ' 'the study of man and his human environment in their relations with each other " and its goal as the discovery of " the true and indigenous laws and processes of social relations." From this approach it logically follows that the prediction of social phenomena is an integral part of the science. But Sociology attempts not only to understand Society but also to control and change it, and in this connection Prof. Fairchild develops the useful distinction between social reform and social engineering, concepts which owe much to Giddings. He rightly emphasises the economic factors in social life " those basic and determinate relations of men to their human environment " and his analysis of the material existence of society into four basic factors—land, population, standard of living and economic culture, is ably conceived. These subjects, it may be added, Prof. Fairchild has made his own in his previous writings.

Other matters that receive illuminating treatment are: the nature and variety of social groups and their classification on the basis of volition; the nature of social causation and the substitution of the mechanical conception by a view which regards social relations as reciprocal and rotatory; the psychological forces of society and the classification of interests; the meaning of applied Sociology and the concepts normality and abnormality; and finally the significance of ultimate values and their axiomatic character.

It was no doubt impossible in a book of this scope to discuss everything adequately, and Prof. Fairchild's remarks on Socialism, Marxism, Capitalism and on social institutions require both amplification and revision. It is misleading to write as he does that "the State is inherent in human nature" or to argue that ancestor-worship is the main root of religion.

The concluding section of the book is devoted to questions, study topics, and a useful bibliography. There is in addition a detailed index.

J. Rumney.

Science and the Police Officer. By Henry T. F. Rhodes. [Pp. xvi + 180.] (London: The Police Chronicle, 1934. 5*s*. net.)

This book is a collection of lectures to police officers dealing partly with the application of scientific technique to and partly with the employment of scientific method in the detection of crime. They are addressed to the layman and not to the expert, and quite apart from their value to the policeman, they make a book of unusual interest. Many who follow the more erudite paths of science find relaxation in detective fiction which, in its best form, employs mental processes akin to their own, and such will find this book pleasant reading whilst the large army of authors of such works will find it invaluable.

The author rightly stresses the importance of the scientific outlook in the treatment of evidence, and on the technical side deals with the uses of photography, microphotography, ultraviolet light and chemical tests in connection with various aspects of crime. In many instances the employment of the various methods is illustrated by reference to actual cases. Finger-prints, foot-prints, calligraphy, bullets and firearms and toxicology all come in for treatment. The scientific details are accurately and clearly put in an elementary manner, the intention being rather that the reader shall be able to appreciate the work of the expert than that he should be able to replace him.

O. L. B.

The Progress of Science. By J. G. Crowther. [Pp x + 304, with frontispiece, 11 plates and 31 figures.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1934. 12*s*. 6*d*. net.)

Dr. Crowther here presents an account of recent fundamental researches in physics, chemistry and biology. The first two chapters deal with the latest achievements of the Cavendish and Copenhagen laboratories, including the discovery of the neutron. The third chapter gives a very interesting and informative description of Soviet physics, more especially of the work in progress at the Physico-Technical Institute, Kharkov. Chapters IV and

V summarise the current theories of the expansion of the universe and the latest researches on the penetrating or cosmic rays and include an account of the discovery of the positron. Chapter VI deals with heavy hydrogen. Chapter VII is concerned with "The Chemistry of Evolution" and provides a very valuable summary of the work of Needham and others on chemical embryology. In Chapter VIII, which deals with human heredity, Dr. Crowther has much to say about the nonsense talked by certain advocates of wholesale sterilization, and invokes the aid of Haldane and Hogben in a thorough-going attack on the clap-trap that too frequently in this problem passes for scientific evidence among politically-minded enthusiasts. The final chapter describes the recent advances in the treatment of pernicious anæmia.

Dr. Crowther has written many admirable popular works on science: this is by far the best. Though intended primarily for the intelligent general reader, it is well suited to the needs of the average specialist in science, who is usually a layman outside the range of his own particular study.

D. McKie.

The Method and Theory of Ethnology. By Paul Radin. [Pp. xv + 278.] (London and New York: McGraw-Hill Publishing Co., Ltd., 1933. 15*s*. net.)

Dr. Radin shows himself a stern critic of past and present schools of thought in ethnology. Each one in turn—Tylor and the evolutionary school, Elliot Smith, Perry and the diffusionists and Malinowski and his followers in "functional anthropology"—is weighed in the balance and found wanting. Nor does Dr. Radin spare his colleagues in the United States. Dr. F. Boas and those who have elaborated the theory of culture areas, while given credit for achievement in certain directions, are shown to have been misled by the false analogy of natural science into applying analytical methods and the quantitative treatment of unitary characters to material to which they are not applicable and in a subject with the aim of which they are not consonant.

Dr. Radin's own view is that the purpose of ethnology being the description of aboriginal cultures, it should be purely descriptive, concrete, and individual. Whether he regards ethnology understood in this sense as a science Dr. Radin does not say.

E. N. F.

Adult Education in Practice. Edited by Robert Peers, M.A. [Pp. xiv + 301.] (London: Macmillan & Co., Ltd., 1934. 7*s*. 6*d*. net.)

This book, which replaces the much more modest *Tutor's Manual* prepared long ago under the editorship of Mr. G. D. H. Cole, is, as the publishers claim, the first complete survey of the English Adult Education movement from the practical standpoint. Prof. Peers has played an important part in the developments which have brought the movement to its present stabilised (if not standardised) form, and he has been able to enlist the services of a number of other notable teachers and administrators. The result of their collaboration is a handbook, dealing briefly but adequately with all those aspects of adult education upon which tutors and others concerned ought to be informed. It passes from a brief but illuminating study of the aims, background and present organisation of the movement to chapters dealing with the several types of adult students; their needs and the types of class and courses designed to meet them; the work of organising tutors, including that of the pioneer breaking new ground; methods of teaching (an excellent chapter); library facilities; activities outside the class; the opportunities available for full time students; and lastly the very important but admittedly difficult problem of the training of tutors for the teaching of adults. A long series of appendices includes the regulations of the Board of Education and other official documents together with brief accounts, supplementary to the main part of the work, dealing with the special features of adult education in Scotland and Wales. The book ends with a very useful classified bibliography. The whole work is pervaded by an atmosphere of reality which must make it welcome, not merely to young tutors, but also to experienced teachers and organisers. To say that it will be indispensable is not to repeat a formula but to state a fact.

T. P. N.

The Subject Index to Periodicals, 1933. [Pp. x + 546 cols.] (London: The Library Association, 1934. £3 10*s*.)

In the preface to this new volume of the annual Subject Index the Library Association quite justifiably proclaims its gratification at the appearance of the volume only eight months after the close of the year. To have collected more than 25,000 entries, checked them, made the corrections which the reduction to uniformity of the work of numbers of contributors inevitably involves, and read the proofs in so short a time is an achievement of which the editor, Mr. T. Rowland Powel, may well be proud.

The value of the Index is probably greater to journalists, and the writers of the more popular type of book than to the specialist, since the material covered by certain of the scientific abstracting journals is as a rule omitted, but the individual who uses the book with this fact in mind must find it of the greatest use to have in one comprehensive index articles which have appeared in such journals as *Nature*, *Science Progress*, *The Philosophical Magazine*, and *The Journal of the Franklin Institute*, to name only a few. He must be grateful also for having his attention drawn to scientific papers published in the less well-known journals all over the world. The inclusion of references to a larger number of foreign periodicals would make the debt of scientific workers to the Library Association still greater.

J. A. W.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Numerical Studies in Differential Equations. Vol. I. By H. Levy, M.A., D.Sc., F.R.S.E., Professor of Mathematics at the Imperial College of Science, University of London, and E. A. Baggott, M.Sc., A.R.C.S., D.I.C., Lecturer in Mathematics, The Polytechnic, Regent Street, London. London: Watts & Co., 1934. (Pp. viii + 238, with 14 figures.) 12*s*. 6*d*. net.

Differential and Integral Calculus. Vol. I. By R. Courant, Göttingen—New York. Translated by E. F. McShane, Princeton. London and Glasgow: Blackie & Son, Ltd., 1934. (Pp. xiv + 568, with 136 figures.) 20*s*. net.

The Differential Invariants of Generalized Spaces. By Tracy Yerkes Thomas, Associate Professor of Mathematics in Princeton University. Cambridge: at the University Press, 1934. (Pp. x + 241.) 21*s*. net.

Lectures on Matrices. By J. H. M. Wedderburn. American Mathematical Society Colloquium Publications, Vol. XVII. New York: American Mathematical Society, 1934. (Pp. viii + 200.) $3.00.

Five Place Table of Natural Trigonometric Functions to Hundredths of a Degree. Compiled by Amelia De Lella, Central Station Engineering Department, General Electric Co., New York. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. 52.) 6*s*. net.

General Astronomy. By H. Spencer Jones, M.A., Sc.D., F.R.S., Astronomer Royal. Second edition. London: Edward Arnold & Co., 1934. (Pp. viii + 437, with 28 plates and 113 figures.) 12*s*. 6*d*. net.

Through Space and Time. By Sir James Jeans, M.A., D.Sc., Sc.D., LL.D., F.R.S. Based on the Royal Institution Lectures, Christmas 1933. Cambridge: at the University Press, 1934. (Pp. xiv + 224, with 53 plates.) 8*s*. 6*d*. net.

Radio Round the World. By A. W. Haslett, sometime Scholar of King's College, Cambridge. Cambridge: at the University Press, 1934. (Pp. viii + 196, with 5 plates and 22 figures.) 5*s*. net.

Generation of the Universe and "Design for Living." By Percy A. Campbell. East Cleveland, Ohio, 1934. (Pp. vi + 101.) $1.50.

Theoretical Physics. By Georg Joos, Professor of Physics at the University of Jena. Translated from the first German edition by Ira M. Freeman, Ph.D., Chicago. London and Glasgow: Blackie & Son, Ltd., 1934. (Pp. xxiv + 748, with 3 plates and numerous figures.) 25*s*. net.

The Physical Basis of Things. By John A. Eldridge, Professor of Physics, University of Iowa. International Series in Physics. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xiv + 407, with frontispiece and 136 figures.) *22s. 6d.* net.

Physics. By A. S. Eve, C.B.E., D.Sc., LL.D., F.R.S., Macdonald Professor of Physics and Dean of the Faculty of Graduate Studies and Research, McGill University, Montreal. The Home University Library of Modern Knowledge. London: Thornton Butterworth, Ltd., 1934. (Pp. 258, with 39 figures.) *2s. 6d.* net.

Elementary Quantum Mechanics. By R. W. Gurney, M.A., Ph.D., Research Associate in the University of Bristol. Cambridge: at the University Press, 1934. (Pp. vi + 160, with 67 figures.) *8s. 6d.* net.

Magnetism and Matter. By Edmund C. Stoner, Ph.D. (Cambridge), Reader in Physics at the University of Leeds. London: Methuen & Co., Ltd., 1934. (Pp. xvi + 575, with 87 figures.) *21s.* net.

A Guide to Electricity. For Home and School. By Charles F. Smith, D.Sc., M.I.E.E., lately one of H.M. Inspectors of Schools. London: Oxford University Press, 1934. (Pp. x + 73, with 17 figures.) *2s. 6d.* net.

Ions, Electrons, and Ionizing Radiations. By James Arnold Crowther, Sc.D., F.Inst.P., Professor of Physics in the University of Reading. Sixth edition. London: Edward Arnold & Co., 1934. (Pp. xii + 340, with 114 figures, including 4 plates.) *12s. 6d.* net.

Atomic Structure and Spectral Lines. By Arnold Sommerfeld, Professor of Theoretical Physics at the University of Munich. Translated from the Fifth German Edition by Henry L. Brose, M.A., D.Phil., D.Sc., Lancashire-Spencer Professor of Physics, University College, Nottingham. Vol. I. Third edition. London: Methuen & Co., Ltd., 1934. (Pp. xii + 675, with 151 figures.) *35s.* net.

The Mysteries of the Atom. By H. A. Wilson, M.A., M.Sc., F.R.S., Professor of Physics, Rice Institute. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1934. (Pp. x + 146, with 40 figures.) *10s. 6d.* net.

A Study of Crystal Structure and its Applications. By Wheeler P. Davey, Ph.D., Research Professor of Physics and Chemistry, School of Chemistry and Physics, The Pennsylvania State College. International Series in Physics. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xii + 695, with 255 figures.) *45s.* net.

Crystals and the Polarising Microscope. A Handbook for Chemists and Others. By N. H. Hartshorne, Ph.D., M.Sc., Lecturer in Inorganic and Physical Chemistry, and A. Stuart, M.Sc., F.G.S., Lecturer in Geology, University College of Swansea. With a Foreword by Professor G. T. Morgan, O.B.E., D.Sc., LL.D., F.R.S., F.I.C. London: Edward Arnold & Co., 1934. (Pp. viii + 272, with 217 figures.) *16s.* net.

The Electronic Theory of Chemistry. An Introductory Account. By Robert Fergus Hunter, D.Sc., Ph.D., D.I.C., A.R.C.S., Nizam Professor of Chemistry and Director of the Chemical Laboratories of the Muslim University, Aligarh, India. With a Foreword by C. K. Ingold, D.Sc., F.R.S. London: Edward Arnold & Co., 1934. (Pp. viii + 125.) *8s. 6d.* net.

Dipole Moments. A General Discussion held by The Faraday Society, September, 1934. London : Gurney & Jackson for The Faraday Society, 1934. (Pp. iv + 314, with numerous figures.) 21*s.* net.

Physical Chemistry. By A. J. Mee, M.A., B.Sc., Senior Chemistry Master, Cheltenham Grammar School. London : William Heinemann, Ltd., 1934. (Pp. xx + 780, with 208 figures and 5 plates.) 15*s.*

Electrolytes. By Hans Falkenhagen, Professor in the University of Köln. Translated by R. P. Bell, Fellow of Balliol College, Oxford. The International Series of Monographs on Physics. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1934. (Pp. xvi + 347, with 105 figures.) 25*s.* net.

The Spirit of Chemistry. An Introduction to Chemistry for Students of the Liberal Arts. By Alexander Findlay, Professor of Chemistry, University of Aberdeen. Second edition. London, New York, Toronto : Longmans, Green & Co., 1934. (Pp. xvi + 510, with 152 figures.) 10*s.* 6*d.* net.

A Manual of Biochemistry. By J. F. McClendon, Professor of Physiological Chemistry, University of Minnesota Medical School. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1934. (Pp. viii + 381, with 58 figures.) 31*s.* net.

An Introduction to Practical Organic Chemistry. By W. A. Waters, M.A., Ph.D. (Cantab.), Gordon Wigan Prizeman of Cambridge University, Lecturer in Chemistry in the University of Durham. London : Edward Arnold & Co., 1934. (Pp. x + 92, with 19 figures.) 3*s.* 6*d.*

Arsenical and Argentiferous Copper. By J. L. Gregg, Metallurgist, Battelle Memorial Institute. American Chemical Society Monograph Series. New York : The Chemical Catalog Co., Inc., 1934. (Pp. 189, with 56 figures.) $4.00.

The Principles of Motor Fuel Preparation and Application. Vol. I. By Alfred W. Nash, M.Sc., M.I.Mech.E., F.C.S., F.Inst. Fuel, Professor of Petroleum Technology, University of Birmingham, and Donald A. Howes, B.Sc., Ph.D., A.M.I.P.T., Anglo-Persian Oil Co., Ltd. London : Chapman & Hall, Ltd., 1934. (Pp. xiv + 538, with 125 figures, including 9 plates.) 30*s.* net.

Industrial Furnaces. Vol. I. By W. Trinks, Professor of Mechanical Engineering, Carnegie Institute of Technology. Third edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1934. (Pp. x + 456, with 359 figures.) 37*s.* 6*d.* net.

Dust. By S. Cyril Blacktin, B.Sc., M.Sc., Ph.D., A.I.C. London : Chapman & Hall, Ltd., 1934. (Pp. xii + 296, with 4 figures, including 2 plates.) 18*s.* net.

Firedamp Explosions : The Projection of Flame. Part III : The Effect of Branch Galleries and of Bends. By M. J. Burgess. Safety in Mines Research Board Paper No. 83. London : H.M. Stationery Office, 1934. (Pp. 20, with 8 figures.) 6*d.* net.

Stemming Materials. By J. A. S. Ritson and H. Stafford. Safety in Mines Research Board Paper No. 84. London : H.M. Stationery Office, 1934. (Pp. 30, with 1 figure.) 6*d.* net.

Simultaneous Shot-Firing. A Report by the Shot-Firing Sub-Committee of the Explosives in Mines Research Committee. Safety in Mines Research Board Paper No. 85. London: H.M. Stationery Office, 1934. (Pp. 14, with 1 figure.) 6*d*. net.

An Automatic Firedamp Recorder. By H. Lloyd. Safety in Mines Research Board Paper No. 86. London: H.M. Stationery Office, 1934. (Pp. 16, with 14 figures and 3 plates.) 6*d*. net.

The Routine Method for Determining the Inflammability of Mine Dusts: A Modified Form of the Test. By A. L. Godbert. Safety in Mines Research Board Paper No. 87. London: H.M. Stationery Office, 1934. (Pp. 12, with 2 figures and 3 plates.) 6*d*. net.

Twelfth Annual Report of the Safety in Mines Research Board, including a Report of Matters dealt with by the Health Advisory Committee, 1933. London: H.M. Stationery Office, 1934. (Pp. 129, with 48 figures, including 19 plates.) 2*s*. net.

Geologic Structures. By Bailey Willis and Robin Willis. Third edition. New York and London: McGraw-Hill Book Company, Inc., 1934. (Pp. xviii + 544, with frontispiece, 12 plates and 202 figures.) 24*s*. net.

The Geology of Egypt. Vol. II: The Fundamental Pre-Cambrian Rocks of Egypt and the Soudan; Their Distribution, Age and Character. Part I: The Metamorphic Rocks. By W. F. Hume, D.Sc., late Director of the Geological Survey of Egypt. Cairo: Survey of Egypt, Government Press, 1934. (Pp. lxv + 424, with 96 plates.) Piastres 300.

Geologic History at a Glance. Compiled by L. W. Richards and G. L. Richards, Jr. Stanford University, California: Stanford University Press; London: Humphrey Milford, 1934. (2 plates.) 3*s*. 6*d*. net.

An Introduction to Plant Biochemistry. By Catherine Cassels Steele, M.A., B.Sc., Ph.D. (St. Andrews). London: G. Bell & Sons, Ltd., 1934. (Pp. viii + 356, with 12 figures.) 15*s*. net.

The Gramineæ. A Study of Cereal, Bamboo and Grass. By Agnes Arber, M.A., D.Sc., sometime Fellow of Newnham College, Cambridge. Cambridge: at the University Press, 1934. (Pp. xviii + 480, with frontispiece and 212 figures.) 30*s*. net.

Researches on Fungi. Vol. VI. By A. H. Reginald Buller, D.Sc., Ph.D., F.R.S., Professor of Botany at the University of Manitoba. London, New York, Toronto: Longmans, Green & Co., 1934. (Pp. xii + 513, with 231 figures.) 28*s*. net.

Hooton Pagnell. The Agricultural Evolution of a Yorkshire Village. By Arthur G. Ruston, D.Sc., B.A., Lecturer in Agricultural Economics and Advisory Economist, the University of Leeds, and Denis Witney, B.Com., Advisory Economist, the University of Edinburgh. London: Edward Arnold & Co., 1934. (Pp. viii + 459, with 12 plates and 58 figures.) 25*s*. net.

Weed Suppression by Fertilizers and Chemicals. By H. C. Long, B.Sc.(Agric.), of the Ministry of Agriculture and Fisheries. Published by H. C. Long, The Birkins, Orchard Road, Hook, Surbiton, 1934. (Pp. 57, with 17 plates and 5 line drawings.) 2*s*. net.

Theory and Practice of Silviculture. By Frederick S. Baker, F.E., Associate Professor of Forestry, University of California. American Forestry Series. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xiv + 502, with 87 figures.) 30*s*. net.

Identification of the Commercial Timbers of the United States. By H. P. Brown, Ph.D., Professor of Wood Technology, and A. J. Panshin, Ph.D., Research Assistant, New York State College of Forestry. American Forestry Series. New York and London: McGraw-Hill Book Co., Inc., 1934. (Pp. xxvi + 223, with 60 plates and 6 figures.) 18*s*. net.

Everyday Botany. By L. J. F. Brimble, B.Sc. (London and Reading), Associate of University College, Reading. London: Macmillan & Co., Ltd., 1934. (Pp. viii + 589, with 340 figures.) 7*s*. 6*d*.

An Index to the Genera and Species of the Diatomaceæ and their Synonyms. 1816–1932. Compiled by Frederick Wm. Mills, F.L.S., F.R.M.S. London: Wheldon & Wesley, Ltd., 1934. Part XVI: Na–Ni. (Pp. 1162–1240.) Part XVII: Ni–Pl. (Pp. 1241–1320.) 10*s*. each.

The Anatomy of the Salamander. By Eric T. B. Francis, B.Sc. Lond., Ph.D. Reading. With an Historical Introduction by Professor F. J. Cole, D.Sc., F.R.S. Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. (Pp. xxxii + 381, with frontispiece, 25 plates and 3 figures.) 25*s*. net.

Sea Trout of the Montrose District. The Bervie, the North Esk, the South Esk, and Coast Nets. Part I: The District and its Sea Trout. By G. Herbert Nall, M.A., F.R.M.S. Fishery Board for Scotland, Salmon Fisheries, 1934, No. III. Edinburgh: H.M. Stationery Office, 1934. (Pp. 63, with 2 graphs and 7 plates.) 3*s*. 6*d*. net.

The Natural History of the Herring of the Southern North Sea. Being the Buckland Lectures for 1933. By William C. Hodgson, D.Sc., Naturalist on the Staff of the Ministry of Agriculture and Fisheries. London: Edward Arnold & Co., 1934. (Pp. 120, with frontispiece and 10 figures.) 3*s*. 6*d*. net.

The Herring Industry. First Report of the Sea-Fish Commission for the United Kingdom. London: H.M. Stationery Office, 1934. (Pp. 51.) 9*d*. net.

Insect Physiology. By V. B. Wigglesworth, M.A., M.D., Lecturer and Milner Research Fellow in Medical Entomology, London School of Hygiene and Tropical Medicine. Methuen's Monographs on Biological Subjects. London: Methuen & Co., Ltd., 1934. (Pp. x + 134, with 13 figures.) 3*s*. 6*d*. net.

The Locust Outbreak in Africa and Western Asia in 1933. Survey prepared by B. P. Uvarov, Senior Assistant, Imperial Institute of Entomology. Economic Advisory Council, Committee on Locust Control. London: H.M. Stationery Office, 1934. (Pp. 66, with 9 maps.) 2*s*. net.

Sixth Report of the Economic Advisory Council Committee on Locust Control. London: H.M. Stationery Office, 1934. (Pp. 55.) 1*s*. net.

Working Dogs. By Elliott Humphrey and Lucien Warner. With a Foreword by Raymond Pearl. Baltimore: The Johns Hopkins Press; London: Humphrey Milford, 1934. (Pp. xiv + 253, with 52 figures.) 16*s*. net.

Veterinary Helminthology and Entomology. The Diseases of Domesticated Animals caused by Helminth and Arthropod Parasites. By H. O. Mönnig, Dr.Phil., B.V.Sc., Professor of Parasitology, Faculty of Veterinary Science, University of Pretoria. London: Baillière, Tindall & Cox, 1934. (Pp. xvi + 402, with 264 figures, including 12 plates.) 30*s.* net.

Pathologie der Mitose. By Georg Politzer, Privatdozent der Embryologie an der Wiener Universität. Protoplasma-Monographien, Vol. VII. Berlin: Gebruder Borntraeger, 1934. (Pp. viii + 238, with 113 figures.) R.M. 16.20.

Bacteriology and Protozoology: in Medicine and Public Health. By W. R. Logan, M.D., F.R.C.P.E., D.P.H. Fourth edition. Part I. Catechism Series. Edinburgh: E. & S. Livingstone, 1934. (Pp. iv + 75.) 1*s.* 6*d.* net.

Practical Bacteriology. An Introductory Course for Students of Agriculture. By Andrew Cunningham, D.Sc., Advisory Bacteriologist, Edinburgh and East of Scotland College of Agriculture. Second edition. Edinburgh and London: Oliver & Boyd, Ltd., 1934. (Pp. viii + 203, with 26 figures.) 7*s.* 6*d.* net.

Illustrations of Regional Anatomy. By E. B. Jamieson, M.D., Senior Demonstrator and Lecturer, Anatomy Department, University, Edinburgh. Edinburgh: E. & S. Livingstone, 1934. Section I: Central Nervous System. (48 plates.) 7*s.* net. Section II: Head and Neck. (61 plates.) 10*s.* net. Section III: Abdomen. (37 plates.) 5*s.* 6*d.* net. Section IV: Pelvis. (30 plates.) 3*s.* 6*d.* net. Section V: Thorax. (27 plates.) 4*s.* net. Complete set, 30*s.* net.

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SCIENCE PROGRESS

PROBLEMS OF SEX IN THE HIGHER FUNGI

By H. C I. GWYNNE-VAUGHAN, G.B.E., D.Sc.
Professor of Botany in the University of London, Birkbeck College

Among the lower plants reproduction may take place either by the development of non-sexual spores, each of which germinates without fusion to give rise to a structure like that on which it grew, or by the formation of sexual cells the union of which results in a new plant. In either case a stimulus to growth is involved, and the advantage of the sexual process presumably lies in the opportunity it affords of bringing together cells from different individuals so that fresh combinations of characters occur. Among aquatic forms, many of which are free swimming or liberate free-swimming cells, the union of sexual elements from distinct individuals offers no insuperable problem, but, with the invasion of the land, a more difficult situation has to be faced. In green plants it is met partly by the persistence of the motile male cells, though this demands the presence of external, fluid water at the time of reproduction, and partly by the formation, as the ultimate product of fertilisation, of vast quantities of spores, each capable of giving rise to a new sexual plant. Frequent failure can be disregarded when success is thus increased by many hundredfold. Spores of this type are produced in groups of four in spore mother cells and their development is associated with the reduction division or meiotic phase, by which the number of chromosomes, doubled in fertilisation, is returned to the original figure. The chromosomes, transmitted from cell to cell throughout the organism, carry the heredity characters.

The fungi differ from other plants in being without chlorophyll and, like animals, are therefore dependent for their organic food on the substances manufactured by green plants. Some, such as the white and blue moulds, infect plant or animal remains, others develop as parasites on green plants, or, more rarely, on animals, and form the rusts, smuts, mildews and other diseases which trouble the farmer. Among fungi, as among green plants, the device of multiplying the effect of fertilisation by means of spores is widely employed, but the motile male cell disappears while the vegetative plant is still aquatic, and the motile spore is discarded soon after adaptation to subaerial conditions has begun. Thus the higher fungi, Ascomycetes and Basidiomycetes, entirely lack motile cells and their only power of movement is by the directive growth of their filaments.

In these groups the vegetative body consists of a mass of branching threads, the hyphæ, which together constitute the mycelium, and may grow singly or be interwoven to form a solid structure as in toadstools and bracket fungi. The hyphæ are septate, each cell containing one or more nuclei, but the walls are formed by the ingrowth of a ring of material so that, in young filaments, the centre of the wall is incomplete, and cytoplasm, vacuoles and nuclei can pass unhindered from cell to cell. At a later stage, when the central pore is more nearly closed, vacuoles may be seen to squeeze through, changing their shape as they go, or may be retained, as in a backwater, against the completed sides of the wall. Another means of communication (Fig. 1) is by the fusion of a hypha with another part of the cell from which it arose, with a separate branch of the same mycelium, or with a mycelium from a different source. Sometimes germ tubes from three or four spores will unite in this way so that, in the resulting hyphæ, the same cell contains nuclei of different origin and carrying different hereditary characters. The vegetative intermingling of distinct individuals thus achieved offers an interesting physiological problem. Multiplication by wind-borne spores known as conidia, unconnected with the sexual process, is common, especially among Ascomycetes and, while some species show normal sexual reproduction, in very many others the sexual organs have disappeared and an advanced stage of apogamy has been reached.

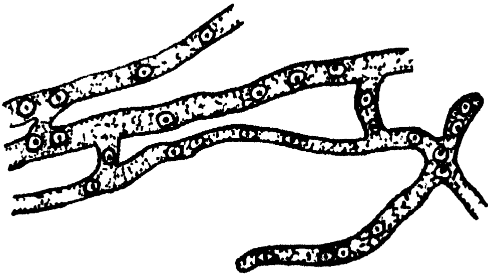


FIG. 1.—*Ascophanus Aurora*, group of hyphæ between which fusion has taken place. × 1,600.

Among Ascomycetes *Pericystis apis*, the cause of chalk brood disease in bees, is diœcious, having separate male and female plants (Fig. 2) which differ in vegetative characters, the male being larger than the female. When they meet, sexual branches are formed. Diœcism has also been recorded in the Laboulbeniaceæ, a family of external parasites on beetles and other insects. In these non-motile species the diœcious condition involves the risk that elements of different sex will fail to make contact and that reproduction, therefore, will not take place. In the Laboulbeniaceæ this difficulty is largely overcome by the motility of the insect hosts, interchange of the sexual elements of the fungus occurring when the insects pair or when they are crowded together.

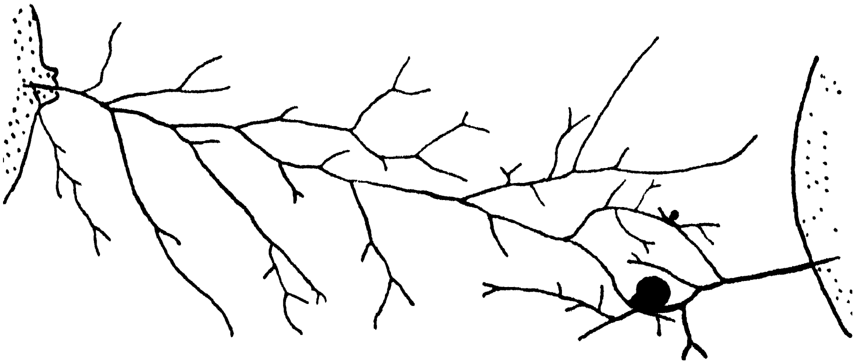


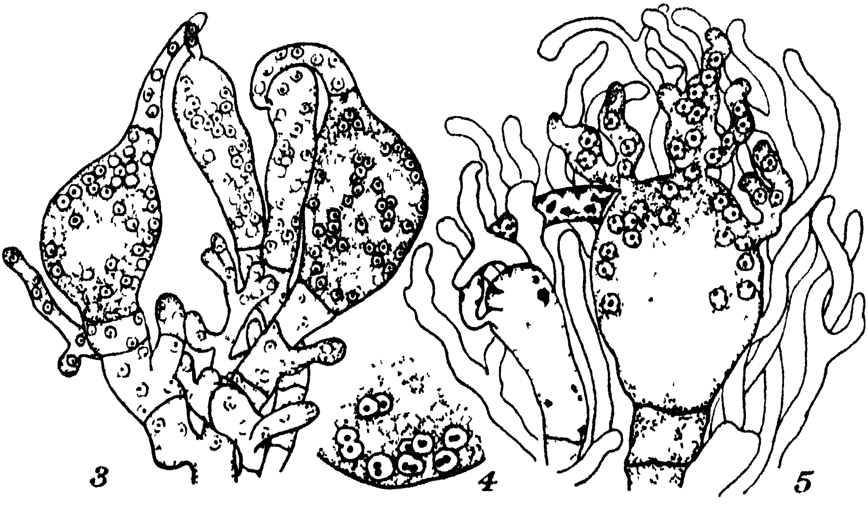
FIG. 2.—*Pericystis apis*, long branch of a male plant meeting short branch of a female plant of the same age.

The black dots indicate the position of sexual organs where they have met. Slightly enlarged (*after* Claussen).

Monœcious forms, in which the same plant bears both male and female organs, have a far better chance of fertilisation, but, if union takes place between the products of the same vegetative body, no fresh grouping of characters can be expected. When an organism is well suited to a stable environment this may be actually an advantage, but it limits the possibility of adaptation should the environment change.

On patches of burnt ground a little, pink, ascomycetous species, known as *Pyronema confluens*, is sometimes to be seen which reproduces only by the sexual method. Male and female organs (Fig. 3) are borne on branches of the same mycelium. The male organ, or antheridium, is an oblong sac containing 100 to 200 nuclei ; the oogonium, or female organ, is stouter, but the number of nuclei is about the same, and, from the distal end, a relatively narrow branch, the trichogyne, grows out and makes contact with the antheridium. The intervening walls are dissolved, and the male nuclei travel through the trichogyne to the oogonium where they are seen to fuse (Fig. 4) in pairs with the female nuclei. After fertilisation numerous branches, the ascogenous hyphæ, bud out from the oogonium (Fig. 5); at their ends the asci, or spore mother cells are developed, reduction divisions take place, and spores are produced. Nuclei which fail to fuse remain in the oogonium and there disintegrate.

The life history of this and related forms is complicated by the occurrence of a second fusion of nuclei in the young ascus, making the nucleus of the spore mother cell tetraploid (with four sets of chromosomes instead of the usual two), and by a correlated second reduction, which ensures the presence of a single set of chromosomes in the spore. This state of affairs has the curious consequence that, after sexuality is lost, meiosis, the ultimate stage of the sexual cycle, may be retained in relation to the fusion in the ascus. Such is the case, for example, in *Pyronema domesticum*, found on damp walls and ceilings. Here some of the female nuclei in the oogonium unite with the entering male nuclei, but others, apparently stimulated by changes associated with the presence of the latter, pass unfused into the ascogenous hyphæ, so that, before the fusion



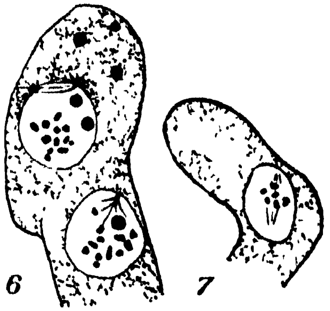
FIGS. 3, 4, 5.—*Pyronema confluens.*

Fig 3 shows two pairs of sexual organs From each oogonium a trichogyne has grown out and has made contact with the antheridium × 800

In Fig 4 is seen a small part of an oogonium at the time of fertilisation, showing male and female nuclei in process of fusion × 1,300

Fig 5 shows an oogonium after fertilisation with ascogenous hyphae growing upwards, the nearly empty antheridium can be seen on the left still in continuity with the trichogyne, the contents of which are beginning to disintegrate and are deeply stained The branches shown in outline are those forming the sheath of the fructification × 800 in the ascus, some cells have diploid nuclei, with two sets of chromosomes, and some haploid nuclei with a single set.

The occurrence of two fusions in the same life history is so unusual that many investigators have been led to doubt whether both really occur. The fusion in the ascus is easily recognised and all observers agree that this takes place. The fusion in the oogoniun seems to possess a more sensitive mechanism, for it can only be seen in material killed in a condition of health and full activity. Since a hundred or more nuclei may fuse simultaneously in a single oogonium, whereas the ascus fusions take place singly, the sexual fusion is not only far more striking when found, but far more easily missed. Certain authors, who have recorded the entry of the male nuclei into the oogonium, but have not observed fusion, have suggested that male and female nuclei travel in pairs up the ascogenous hyphæ and are united by the fusion in the ascus. Colour is given to this hypothesis by the fact that many cells of the ascogenous hyphæ are binucleate, and by the attractive comparison instituted to the binucleate cells of Basidiomycetes shown in Fig. 28.



FIGS. 6 and 7 —*Pyronema confluens.*

Fig. 6 shows the tip of an ascogenous hypha in which two nuclei are preparing for division, in each are 12 darkly-stained dots—the chromosomes, the larger, round objects are nucleoli × 2,600.

Fig 7 shows the tip of a vegetative hypha before fertilisation; the nucleus is at a stage similar to those in Fig 6, but shows only 6 chromosomes, the haploid number. × 2,600.

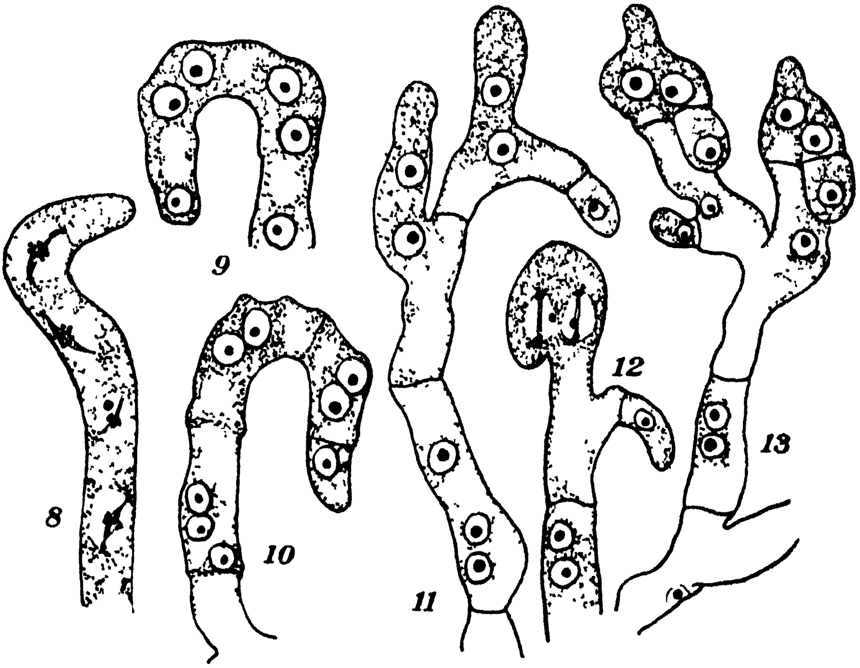
Where, as in Pyronema, the oogonial fusion has been described, the evidence rests on three main criteria:

(i) On the observation of the entry of male nuclei and of male and female nuclei in process of fusion (Fig. 4).

(ii) On chromosome counts, the chromosomes in the nuclei of the ascogenous hyphæ (Fig. 6) being twice as numerous as in the nuclei of virgin oogonia and other cells (Fig. 7) of the sexual plant.

(iii) On changes in the number of nuclei in the oogonia. This is best elucidated by an example. In *Pyronema confluens* the average number of nuclei for 18 virgin oogonia was 150, for 18 oogonia after the male nuclei had entered 276, and for 18 oogonia from which ascogenous hyphæ were beginning to bud (a stage rather earlier than that in Fig. 5) 163. This means that an average of 126 male nuclei entered the oogonium and that an average of 113 of these fused with female nuclei, leaving a few of both sexes unpaired, so that the total number of nuclei was first increased by the entry of the male elements, then diminished by their fusion.

Such observations have now been made in a number of species. The ascogenous hyphæ are variable in appearance, those first put out being sometimes nearly as wide as the oogonium from which they develop, but sooner or later narrow branches are formed (Fig. 8) in which the nuclei must lie in single file. Nuclear division takes place simultaneously at first, when there are no walls, throughout the mass of hyphæ, later throughout each branch, walls being laid down at right-angles to the nuclear spindles. When division is complete a vacuole marks the position of the old nuclear area (Fig. 9) and it is across this (Fig. 10) that the wall appears. As a result the sister nuclei lie in different cells (Fig. 10), the distal daughter nucleus of the distal division in a cell by itself, the proximal daughter of the distal division with the distal daughter of the penultimate division, and so on, each cell, except those at the ends of the series, containing two nuclei which are not sisters and may not be related. The binucleate cells bud out (Fig. 11), their nuclei



FIGS. 8–13.—*Pyronema confluens*, ascogenous hyphæ.

In Fig 8 four nuclei are in process of division, their spindles more or less parallel to the long axis of the hypha

Figs 9 and 10 show the formation of binucleate cells with a uninucleate (or trinucleate) cell at each end of the row

In Fig 11 the binucleate cells are budding out, in Fig 12 the nuclei of a branch from a binucleate cell are dividing, and in Fig 13 the division is complete, each branch being curved over and consisting now of three cells, that in the middle is binucleate, those at the ends contain one nucleus each. In Figs. 11, 12 and 13, the uninucleate distal cell of the original ascogenous hypha can still be seen. × 1,900.

pass into the branch and there divide (Fig. 12), so that two uninucleate cells are produced with a binucleate cell between. This binucleate cell may form the ascus, its two nuclei, which are descendants of the fusion nuclei of the oogonium, undergoing a second fusion, or it may branch again and again before ascus formation takes place. When the ascus is about to be produced, the branch usually curves over (Fig. 13), constituting the familiar crozier. Its two uninucleate cells may join forces and grow out as a new, ascus-forming branch. The method of division and cell

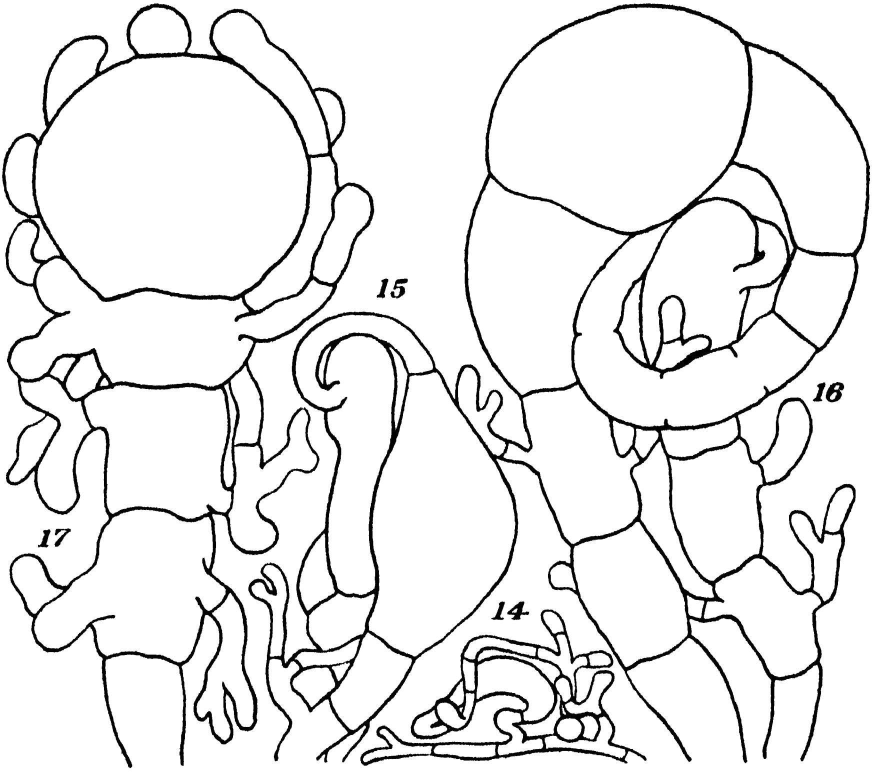


FIG. 14.—*Ascophanus Aurora*.

Oogonium and trichogyne lying almost horizontally, the rounded antheridium is in contact with the trichogyne. × 1,000

FIG. 15.—*Pyronema confluens*.

Oogonium, trichogyne and antheridium × 1,000.

FIG. 16.—*Ascobolus magnificus*.

[illegible] stalk ; from it the trichogyne curves downwards on the right and is in continuity with the antheridium Near the tip of the trichogyne the walls have been pierced by the passage of the male nuclei. × 1,000

FIG. 17.—*Humaria granulata*.

An oogonium mounted on a stalk, there is no trichogyne and no antheridium ; a few hyphæ of the sheath have grown up round the oogonium. × 1,000.

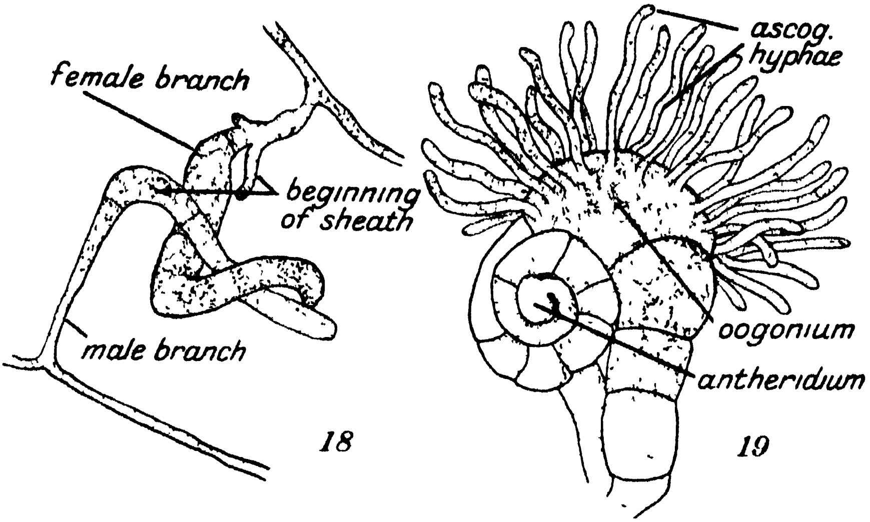
formation at this stage has long been known, but it is comparatively lately that it has been correlated with the earlier cell divisions of the ascogenous hyphæ.

A minute, greenish species found on sheep pellets, *Ascophanus Aurora*, shows a type of sexual apparatus very like that of Pyronema, but the oogonium is twisted and has 8 to 20 nuclei, while the antheridium is globular or drumstick shaped and contains only 4 to 10. Figs 14 to 17 show the relative size of this and other forms shortly before fertilisation. After fertilisation the superfluous female nuclei remain and disintegrate in the oogonium. The small, globular antheridium is of interest since it is often broken off from its stalk and may be a forerunner of the detachable antheridium, known as a spermatium, which is found in many of the fungi and appears to be carried to its destination by the wind or insects. In the parasitic rust fungi insect visitors are encouraged by the bright colour and sometimes by the scent of the infected areas as well as by the secretion of a sugary juice.

Another monœcious form on dung with normal fertilisation is *Ascodesmis nigricans*. Here, however, the trichogyne is not a later outgrowth of the oogonium but a distal cell cut off from the female branch in the course of development.

A trichogyne of the Ascodesmis type, but divided into many cells, is also found in *Ascobolus magnificus*, a species with reddish-brown spores discovered in the neighbourhood of New York. The sexual branches become twisted round one another (Fig. 18), the antheridium and trichogyne make contact, the oogonium swells and the male nuclei travel from cell to cell through the trichogyne till they reach the female organ. The last wall is pierced, they pass through and fuse in pairs with the female nuclei. Throughout their journey and at the time of fusion male nuclei can be distinguished by the fact that they contain two chromatin masses instead of the single mass seen in the female nuclei. Ascogenous hyphæ (Fig. 19) and asci are formed as in Pyronema.

In all these monœcious fungi fruits appear in single spore culture, but, in *Ascobolus magnificus*, single spore cultures are persistently sterile. Rudiments of sexual branches are formed, but they die too soon for their sex to be determined. It is only when appropriate plants are brought together that the sexual organs continue to develop and fertilisation occurs. The plants of Ascobolus thus fall into two categories which are indistinguishable in appearance and may be conveniently referred to as A and B. Two A or two B mycelia in contact are sterile, but, when A and B meet, male organs mature on the younger branches of each and pair with female organs from the older branches of the other, or complementary strain. *Ascobolus magnificus*, though monœcious, bearing male and female elements on the same plant, is self-sterile, in this respect resembling certain monœcious cherries and other trees. It has not reverted to diœcism, but has obtained the same result, the intermingling of characters from two individuals, in a new way. It may perhaps be surmised that, in a changing environment, the limited variability associated with constant self-fertilisation became harmful and, since an evolutionary step can apparently never be retraced, survival lay with those forms which, by a fresh device, ensured the combination of characters from different individuals.



Figs 18 and 19.—*Ascobolus magnificus.*

Fig. 18 shows a narrow, male branch growing upwards and a stouter, female branch in contact with it, the terminal cell of the male branch is the antheridium, the tip of the female branch will form the trichogyne × 350

In Fig 19 is seen an oogonium from which ascogenous hyphæ are growing out, the coiled trichogyne and the antheridium round which it is twisted are empty. × 350

In the fungi so far considered the normal union of male and female elements is retained and is available in *Ascobolus magnificus* as a means of bringing together the nuclei of complementary strains. But in very many fungi one or both of the sexual organs have disappeared. This may be regarded as the natural consequence of continued self-fertilisation ; fresh characters are no longer-brought together and the process has lost its chief importance. In *Humaria granulata*, an orange species very common on cow dung, the mycelium gives rise to oogonia (Fig. 20) but antheridia are never seen. In single spore culture or when two A or two B mycelia meet (Fig. 21) the oogonia soon die, but, if two complementary mycelia

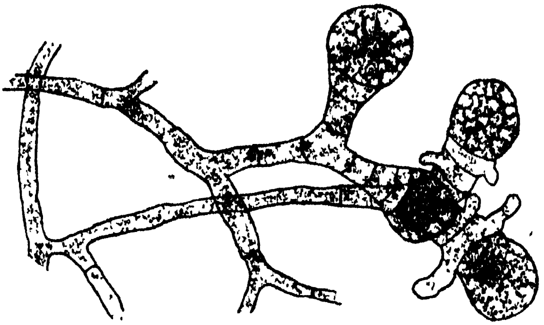


FIG 20.—*Humaria granulata*, young oogonia. × 300.

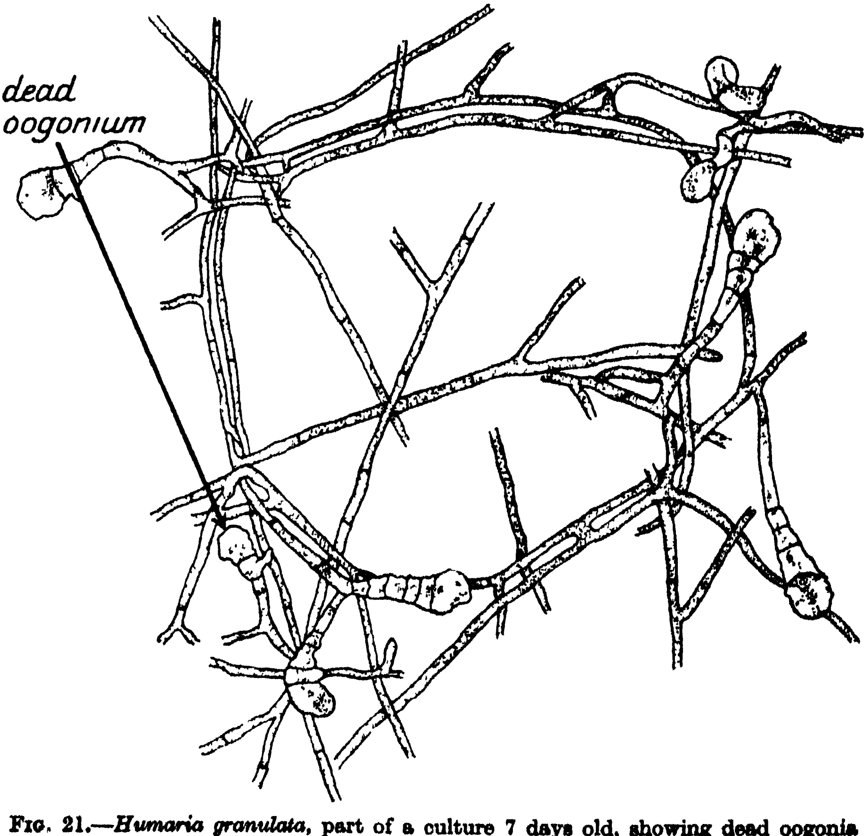


FIG. 21.—*Humaria granulata*, part of a culture 7 days old, showing dead oogonia where two B mycelia are in contact. × 140.

are present (Fig. 22), many of the oogonia put out ascogenous hyphæ and give rise to ascus-bearing fruits. This development depends on the fusion of the hypha on which the oogonium is borne with a branch from a complementary strain (Fig. 23). The co-operation of the two strains is demonstrated by the fact that the resultant asci contain both A and B spores.

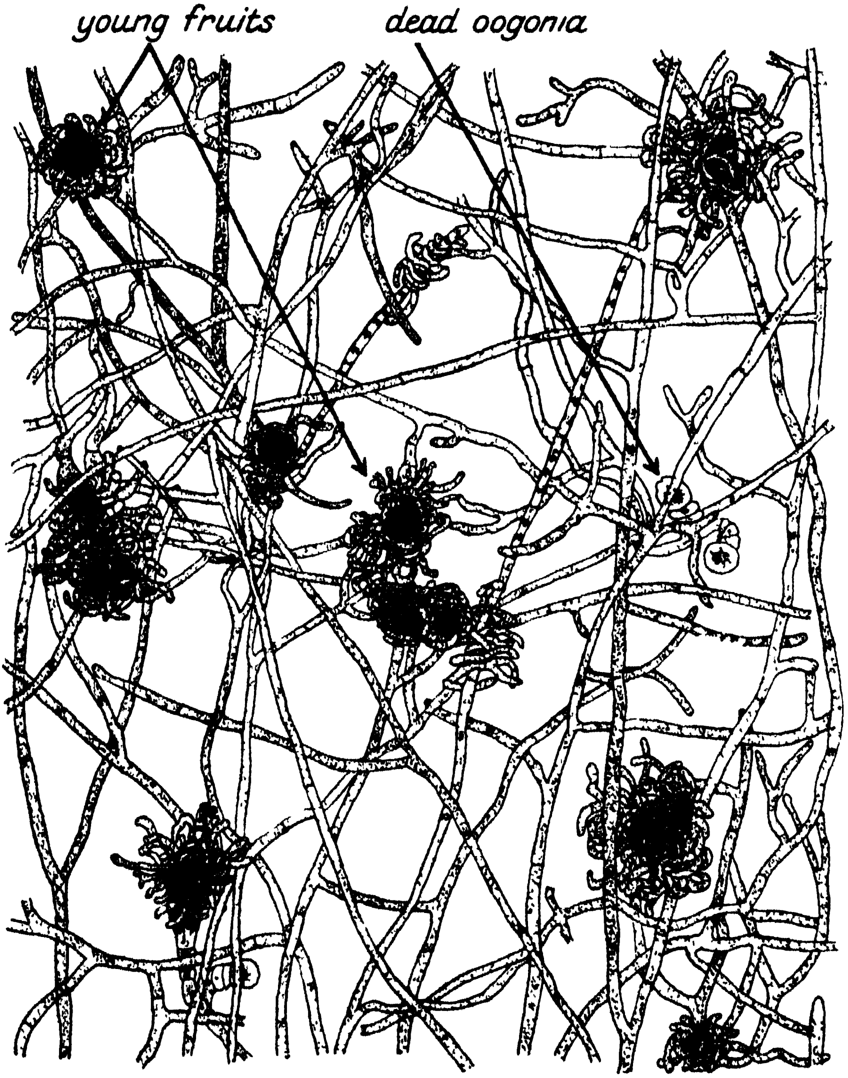


FIG. 22.—*Humaria granulata*, part of a culture 7 days old, showing young fruits where an A and a B mycelium are in contact; two or three dead, unsuccessful oogonia may also be seen. × 140.

A very similar condition is found in Neurospora, the red bread mould, a troublesome pest in bakeries and elsewhere. Here, as in Humaria, the antheridium is lacking and the female branch develops in relation to a mycelial fusion, but, whereas the union of nuclei in the oogonium is still retained in Humaria, in the investigated species of Neurospora it has wholly disappeared. Two species are of special interest. In *N. sitophila* the ascus contains eight spores, uninucleate at their first formation, four of which give rise to A and four to B mycelia. In *N. tetrasperma* similar A and B spores are sometimes obtained, but more commonly the ascus contains four large spores each of which receives an A and a B nucleus. The mycelium from such an AB spore is fertile in single spore culture. The convenient term homothallic is applied to these mycelia, and the term heterothallic to A and B strains like those of *N. sitophila, Humaria granulata* and *Ascobolus magnificus*. A good many fungi are now known which possess both large, homothallic and small, heterothallic spores. In Neurospora a successful study has been made of the inheritance not only of the A and B factors

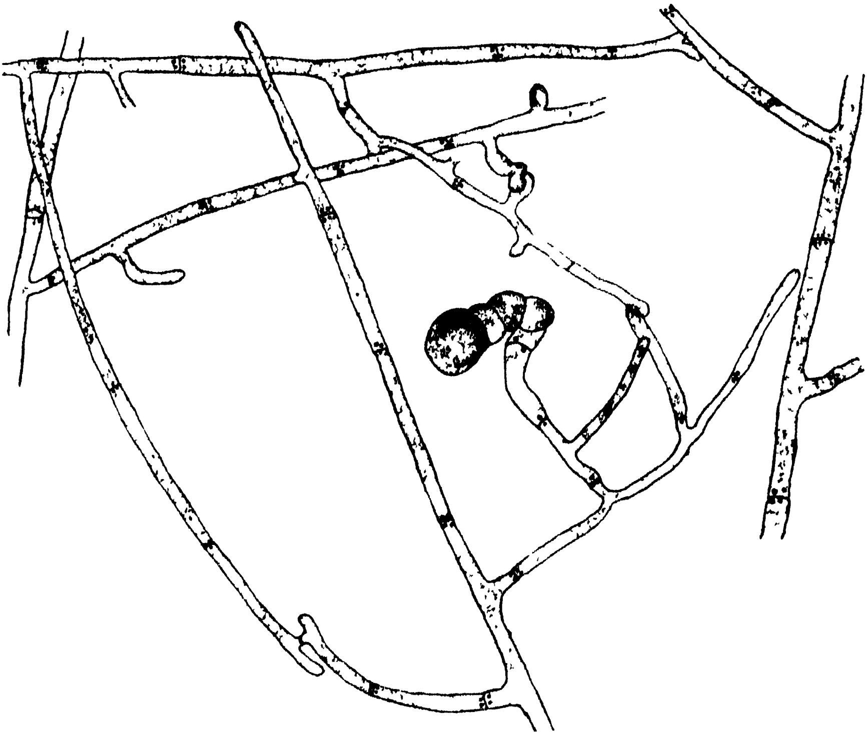


FIG. 23 —*Humaria granulata*, an oogonium developing where an A and a B mycelium have fused, a fusion is to be seen on the left and others are indicated near the top. × 300.

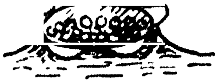
but of such characters as the colour of conidia, and the usual mendelian lines are found to be followed.

Sometimes the heterothallic condition is associated with differences of structure, as in *Glomerella cingulata*, where A has more aerial hyphæ than B, or with differences in energy of development, as in *Penicillium luteum*, a species related to the blue mould on cheese. In Glomerella both A and B mycelia may give rise alone to fruits with fertile spores, but reproduction is far more energetic when the complementary strains have met.

The next step in the loss of the sexual apparatus is the disappearance of the female organ, the ascogenous hyphæ arising direct from vegetative cells, sometimes after fusion of vegetative nuclei in pairs has taken place, sometimes when this also has been lost and, as in Neurospora, the only nuclear fusion is that in the ascus. This is the case with many of the larger members of the Ascomycetes, but most of them have proved difficult to grow in culture, so that their hetero- or homothallic condition has not yet been ascertained.

In the Basidiomycetes, on the other hand, numbers of cases at the corresponding evolutionary level have been studied and the details of development are thoroughly known. This group includes such parasites as the rusts and smuts of wheat and also a considerable series of forms on non-living, organic substrata, of which the mushrooms, toadstools, puffballs and bracket fungi are familiar examples.

The rusts, which still retain their sexual cells, are characterised by their large variety of spores and by the fact that many of them spend the sexual stage of their life history on one host plant and the stage associated with the reduction division on a different species The rust of wheat, *Puccinia Graminis*, for example, undergoes the first stage of its development on the barberry. On this host it produces chains of binucleate spores, grouped in cluster cups (Fig. 24) and arising from a layer of female or fertile cells (Fig. 25). These cells are at first uninucleate, but during their development a second nucleus enters, though fusion does not take place, being postponed till, after the infection of the wheat plant and an extended period of growth and division, meiosis is about to occur. In many rusts inoculation made on a host leaf by a single spore will result in the formation of cluster cups with spore chains from the female cells: these species may be described as homothallic. In others, such as *Puccinia Graminis*, single spore infections are fertile only to a limited extent and after several weeks. If, however, contact is made between inoculations from two complementary strains, abundant cluster cups appear in ten or eleven days. The mycelium which bears the female cells also bears groups of hyphæ (Fig. 25) giving rise to small cells, the spermatia. These have never been seen to form a mycelium, and, by reason of their large nuclei and scanty food reserves, it was long ago suggested that they were detachable male organs. Unfortunately, we do not yet know the cytological details of their behaviour, but recent work in Canada has made clear that the addition of spermatia to a single spore infection of a complementary strain results in the energetic formation of cluster cups, whereas spermatia grown on the same mycelium as the female cells do not produce this effect, so that a case of heterothallism or self-sterility similar to that of *Ascobolus magnificus* appears to be indicated.



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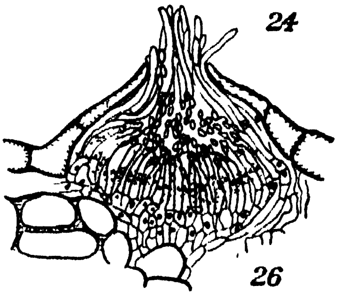

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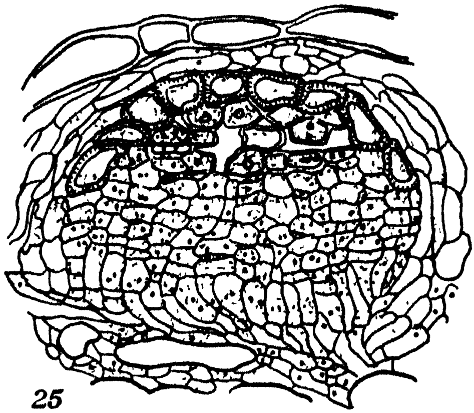

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FIG. 24.—*Puccinia Pruni-spinosæ*, general view of a cluster cup from without. × 20.

FIG. 25.—*Uromyces Poæ*, longitudinal section through a young cluster cup showing rows of binucleate cells. × 310 (*after* Blackman and Fraser).

FIG. 26.—*Gymnosporangium clavariæforme*, receptacle containing spermatia which have been cut off from the narrow hyphæ at the base. × 260 (*after* Blackman).

In the smuts and also in the section to which the mushroom belongs—the Hymenomycetes—there is no trace at all of sexual organs. In 1918 it was, however, discovered that, if the hymenomycetous species *Coprinus lagopus*, one of the inkcaps, is grown under controlled conditions, the single spore cultures may produce small spores known as conidia but otherwise remain persistently sterile. But, when two complementary strains are present or when conidia from one strain are carried to the other, the characteristic, mushroom-like fructifications develop and bear spore mother cells, the basidia (Fig. 27), in which meiosis takes place and which produce externally four basidiospores. A similarly heterothallic condition has been found in a large number of other species, and homothallic forms, for example the mushroom itself, are also known. It would appear that in nature the conidia are often carried by insects. When a basidio-spore germinates it gives rise to a mycelium of uninucleate cells with wide spreading branches. When two such mycelia meet, if the form is heterothallic and they are of different complement, the hyphæ fuse and a mycelium of binucleate cells is formed. This process, in reference to the fact that the cells now contain a double outfit of chromosomes, though still in separate nuclei, has been known as diploidisation. Such a mycelium is characterised by branches growing at an acute angle and by the presence of those curious structures termed clamp connections (Fig. 28). The clamp originates as a minute outgrowth just above the zone where a cell wall is about to be laid down and later reunites with the parent cell. The two nuclei in the cell divide simultaneously, one daughter of each going to each new cell, till at last, when the basidium is formed, the two nuclei present in it fuse in preparation for meiosis and spore formation.

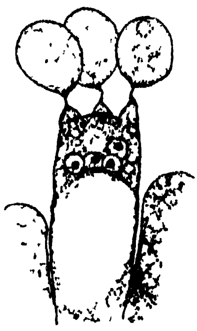

FIG. 27.—*Stropharia semiglobata*, one of the Hymenomycetes, a basidium containing four nuclei and showing three of the four spores. × 500.

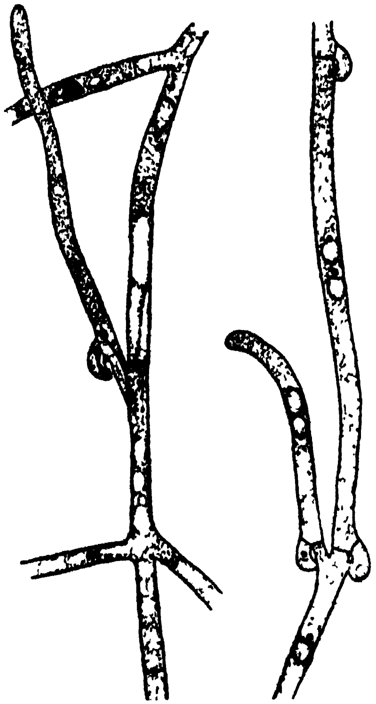

FIG. 28.—*Collybia conigena* (Hymenomycetes), binucleate hyphæ with clamp connections. × 600 (*after* Kniep).

In most heterothallic species there are two strains, A and B, and, just as the A and B factors are segregated in the meiotic phase in the ascus of heterothallic Ascomycetes, so here A and B are separated in meiosis in the basidium, so that two of the basidio-spores give rise to A and two to B mycelia. In some Basidiomycetes, however, there are four strains, these are determined by the segregation in the basidium by four factors which may be represented by A, *a*, B and *b*. Each basidiospore receives A or *a* and B or *b* so that the four strains are AB, A*b*, *a*B and *ab*. Fructifications are only formed when the union of AB and *ab*, or of A*b* and *a*B brings A, *a*, B and *b* together. More rarely three strains are found, AB, AC and BC, the association of any two of which provides the factors A, B and C required for fruit formation. The situation is further complicated by the fact that the complementary strains of one fructification may all be fertile with all the strains of another, A with A and B with B, or AB with AB, so that, in some species, a hundred or more strains with different reactions may be recognised. Nor is it impossible for a single mycelium in a heterothallic species to give rise to a fructification and produce fertile spores alone. As in the rusts, such a development is usually much slower than that induced by the meeting of complementary strains, and the basidiospores show, as might be anticipated, only the characters of the single mycelia from which they arose. The same thing has been observed in heterothallic Ascomycetes.

In the higher Basidiomycetes there is no sign of any sexual apparatus; when the union of complementary strains was first observed it was assumed to constitute a sexual process and the difference between the complements was regarded as one of sex. There are many difficulties, however, in this conception, not the least being the idea of three or four or even a larger number of sexes, a thing unknown in plants or animals possessing sexual organs. We know now that, in forms such as *Ascobolus magnificus*, heterothallism can coexist with normal sex, so that it must be regarded, rather, as a form of self sterility. It may be surmised that, whereas self sterility developed in *Ascobolus magnificus* while the sexual organs were still functional, and in *Humaria granulata* and others after the antheridium had disappeared, in Hymenomycetes it was not established till after all trace of the sexual apparatus had been lost. We may regard heterothallism, or self sterility, not as diœcism —the separation of the sexes in different individuals—but as a special adaptation, replacing and in many respects fulfilling the function of a diœcious condition which has been lost, not impossibly, in connection with the invasion of the land.

HIGH VOLTAGE CABLES

By L. G. BRAZIER, Ph.D., B.Sc., A.M.I.E.E.

The Research Staff, Callender's Cable & Construction Co., Ltd.

"Brown paper soaked in oil" is the description generally given of the dielectric used for the insulation of high voltage cables. It was the dielectric first used by Ferranti when, in 1890, he conceived and carried through by heroic personal enterprise the project of generating and distributing electricity at 10,000 volts at Deptford power station. And it is a matter which has not escaped critical comment, that to this day the cable maker has not found a more satisfactory material.

This failure to make any radical development cannot be attributed either to the technical perfection of the material or to the lack of financial inducement to discover superior materials and methods. It is not generally realised how large a part the cost of distribution contributes to the total cost of electricity. If we refer to the Electricity Commissioners' Returns, we find that at March 1932 the capital cost in Great Britain of the overhead lines and cables (£161 m.) was greater than the capital cost of the whole of the generating plant and machinery (£98 m.), by 1·6 times. If we consider working costs we find that in the previous nine years, technical development of the generating plant has resulted in the average coal consumption per unit generated being brought down from 3·42 lb. to 1·78 lb.—a reduction to 52 per cent. Over the same period the contribution made by distribution to the total working costs (excluding capital charges) has risen from 9·7 per cent. to 16·8 per cent.

We can see, therefore, that development in transmission efficiency has not kept pace with the development of generating machinery; and it is clear that if the cost of transmission could be reduced, it would result in a substantial lowering of the total cost of electricity delivered to the consumer. In particular, if the cost of the underground high voltage cable could be reduced to that of the overhead line, and certain technical difficulties in the use of long lengths of high voltage cable could be overcome, enormous advantages should result, not the least of which would be that it should render unnecessary the building of super power stations on important sites in the centre of large urban districts.

In pressing forward technical development, however, the cable maker finds himself peculiarly handicapped by the lack of any rational theory of the breakdown or behaviour of dielectrics under electric stress. Numerous workers have attacked the problem of breakdown strength with so little profit that the subject is now viewed with something of distaste, except by those who are forced to it by the necessity of meeting practical requirements. We find, for example, in the *Dictionary of Applied Physics* that, in the two articles on " Dielectrics " and " Insulated Electric Cables," practically no reference is made to the question of dielectric strength.

In other directions we find that theoretical treatment has little assistance to offer. Swiedler's laws show that the A.C. phenomenon of dielectric loss is closely associated with the D.C. phenomenon of absorption ; but carry the subject no further. Debye's dipolar theory does provide a real physical insight into dielectric loss under A.C. stress under certain restricted conditions which are unfortunately outside the working range used by the cable maker.

The absence of a reliable theoretical background would not prove so severe a handicap if, like the mechanical engineer, the cable maker could determine empirically by one or two simple tests the strength of his materials and completed structures. Actually it has not been found possible to formulate any test or system of tests which will determine with certainty how the cable will behave under working conditions.

The difficulty arises from the circumstance that it appears impossible to specify with one figure a value for the A.C. breakdown strength of any material. As the electric stress or potential gradient is raised, there is no well-defined limit marking the boundary between failure and non-failure. If different stresses are applied to a number of identical specimens it is found that in addition to a wide dispersion of the results there is a general drift, so that the higher stresses give the shortest testing times before breakdown or "life." By a sufficient number of lengthy tests this curve of electric stress against life can be traced out as far as perhaps a life of 200 hours. As generally plotted, the curve by this stage appears to be fairly flat, but the problem to be faced is whether an asymptotic value has really been reached so that any stress below this level can be safely applied without risk of failure. The practice of the cable maker is to take this level and use a factor of safety of four times for his working designs; and judged by the standards of mechanical engineering, this can hardly be criticised as unenterprising or unduly conservative.

It will be seen, however, that this level giving the stress for an arbitrarily selected life of 200 hours can only be determined by a tedious series of tests which is too expensive to be embarked upon, except in special cases. Many attempts have been made to eliminate this difficulty by devising systems of testing in which the voltage is raised by arbitrarily selected steps at arbitrarily selected intervals of time. In this way, very high stresses are soon reached, and the breakdown of every specimen in a short time is assured. An enormous amount of work on these lines has been published which, when judged by the test of its influence on cable practice, can only be regarded as somewhat unprofitable. The explanation probably lies with very recent work, from which it appears that at the highest stresses the breakdown is of a different type from that which occurs at working stresses.

The interactions between the oil and the paper, by which the combination forms a dielectric of high breakdown strength, are little understood. Oil is universally recognised as a dielectric of high value. A long electric path of oil, however, fails to develop a high electric strength because of the ease with which the liquid can be brushed aside by the mechanical forces brought to bear by the incipient avalanche of electrons constituting a flashover or breakdown. The function of the paper apparently is to provide a dense matrix of fibres which act as a mechanical grid supporting the fluid and retaining it in position.

If this is correct, we should not expect variations in the make-up of the paper to be of great importance. This is, in fact, found to be the case. Papers made of wood pulp, mixtures of wood pulp and manilla, and even cotton fibres, are used with equal success. Apart from obvious matters, such as chemical purity and uniformity of thickness, the physical property which specially interests the cable maker is the porosity of the paper. This is measured on an instrument which measures the resistance to the flow of air under pressure through a single sheet of the paper. It is reduced to a coefficient corresponding to the volume flowing in unit time, through a unit area of the paper, for a unit pressure gradient and for a fluid of unit viscosity. A number of tests have shown that when cables are made by identical processes, but of different papers with varying thickness and porosity, the physical factor which best correlates the resulting figures for breakdown strength, is the porosity. The lower the porosity, the higher the dielectric strength. It is not possible, however, for the cable maker merely to specify a paper with an extremely low value of porosity. In this case, the paper maker merely increases the beating time of the pulp, until the fibres are hydrolised or beaten out of recognition as fibres, and the result is a grease-proof type of paper, which sets more as an amorphous mass, rather than an interlacing matrix of well-defined fibres. Either this, or excessive calendering of the surface, results in a paper which cannot be impregnated. Not only can the interior of the individual sheets not be impregnated even by immersion, but if the paper is made up into a cable with a dielectric wall of say 1·0 cm., then it is found impossible for the impregnating fluid to reach the centre papers because of the low porosity of the papers through which it must pass.[1] We thus reach at an early stage the necessity of striking a balance between technical and economic requirements, such as is the end of most technical enquiries in industry. The cable strength can be increased by lowering the porosity of the paper, but this increases the time and cost of the impregnating process.

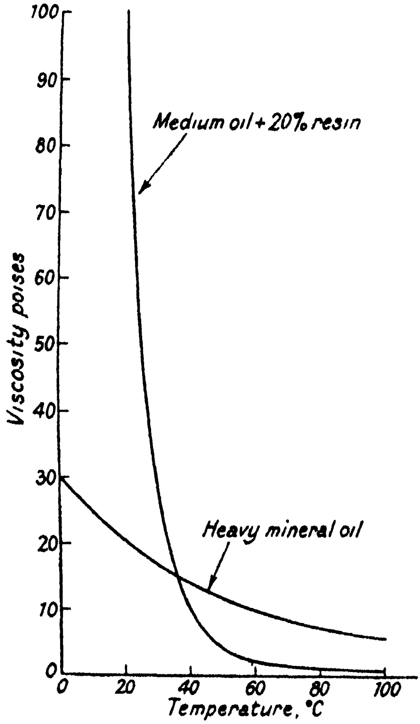

FIG. 1.—Viscosity Temperature Curves.

The oil used is generally a mineral hydro-carbon of the Pennsylvania type. At working temperature the viscosity of the oil must be sufficiently high to prevent the oil draining along the cable while it is wound on the cable drum, or when laid out of the horizontal. At the same time, it is necessary for the viscosity at a temperature of about 100° C. to be sufficiently low to allow impregnation of the dielectric wall within a reasonable and economic time.

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[1] In the normal process of manufacture the paper tape is wound unimpregnated on to the conductor. The half-finished cable is then placed in a tank and dried by a combination of vacuum and heat. When drying is complete, the vacuum in the tank is raised to the necessary level, and the impregnating fluid is admitted under vacuum. After this the cable is lead sheathed.

What is required, therefore, is an oil with a high negative temperature coefficient of viscosity. The use of resin in solution in the oil is well known for this purpose, but the point is illustrated by Fig. 1, showing viscosity temperature curves for the heaviest oil that could be utilised by itself, and for a " compound " consisting of a 20 per cent. solution of resin in a medium-grade oil. The enormous advantage obtained in this way by the use of resin is the only justification for admitting a material of which otherwise the chemical activity and physical instability form a constant source of difficulty.

The high voltage cable thus consists of a stranded copper conductor covered, to a suitable thickness, with paper applied in the form of tape wound on helically, dried, and impregnated by suitable means and the whole enclosed and protected by means of an extruded lead sheath.

We now arrive at the cable makers' main problem. As soon as the cable is used, the heat generated in the copper by the electric current causes a rise of temperature. The temperature coefficient of expansion of any liquid that can be used for impregnation will be in the neighbourhood of 0 0007/° C., which far outweighs that of any other constituent of the cable. As the compound expands, the excess is forced outward from the centre through the paper laminations. At the outside, the lead sheath must be permanently distended When the cable cools the lead sheath is sufficiently rigid to withstand atmospheric pressure without contraction or distortion, and as the compound contracts, therefore, the pressure inside the lead sheath immediately falls to a level governed by the volume of residual gas left in the cable at the moment of lead sheathing. Moreover, there is now no pressure difference to drive back the compound forced outwards through the dielectric during the expansion. As the compound cools, therefore, the liquid column must be frequently broken, giving rise to an irregular distribution of void spaces filled with gases under varying degrees of vacuum. The more perfect the original impregnation of the cable, the higher will be the general vacuum after the first heat cycle.

In high voltage cables the maximum electric stress will be in the region of 50 kV/cm. In any void space it is necessary to allow for the difference in the permittivity, *i.e.* 1·0 in the void, and 3·5 in the dielectric. In practice, the stress across the void space would be raised to something in the general neighbourhood of 100 kV/cm. The dielectric strength of the gas film at *atmospheric pressure* is about 30 kV/cm., so that it is clear that all void spaces will be immediately broken down. The details of this process must be considered in some detail. As the voltage rises on one-half of the alternating wave form, the stress across the void space increases until any electrons available are accelerated sufficiently to cause cumulative ionisation by collision with molecules in the manner originally described by Townsend. When, however, the electrons reach the boundary of the void, they are not drawn off by an electrode as in the usual discussions of the breakdown of gases, but are held up by a dielectric of high resistivity. They must, therefore, accumulate at the boundary. At the same time, probably, positive ions accumulate at the opposite boundary until the stress due to the space charges exactly neutralises that due to the applied voltage. The space charges will, therefore, continually vary in time with the alternations of the applied voltage, and the boundaries of the void will be subject to an alternating bombardment of electrons and positive ions.

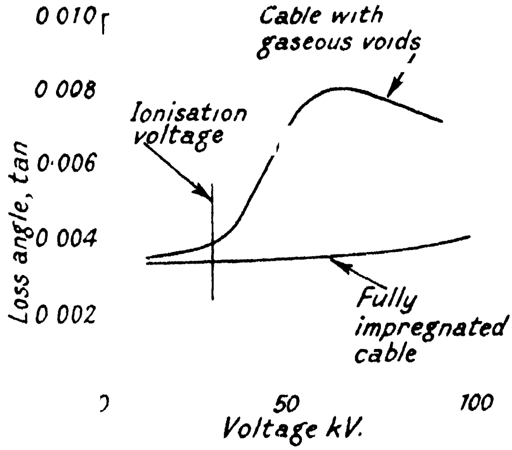


FIG. 2.—"Ionisation" curves.

The energy loss due to this action was observed and measured as soon as it became possible to carry out Dielectric Loss Angle (D.L A.) measurements at high voltage by means of a Schering Bridge. Any energy absorption in a dielectric appears as an angle of defect between the vectorial representation of the actual charging current and a vector 90° in advance of the voltage, and it is this angle which is called the dielectric loss angle.

For a normal homogeneous dielectric, the D.L.A. will remain almost constant over a wide voltage range. When a cable in which ionisation is occurring as described above is tested, it is found that at a critical voltage the D.L.A. rises, and after passing through a maximum shows a tendency to droop (Fig. 2). During recent years this phenomenon has been the subject of more intensive study probably than any other in the cable industry. This is so much the case, that it has become the accepted practice to call the curve of D.L.A. against voltage the "ionisation curve," and to call any increment of D.L.A. with voltage the "ionisation" of the cable. It is now known that other factors beside gaseous ionisation can cause an increment in D.L.A. so that these terms are peculiarly unfortunate and misleading.

Studies on impregnated paper condensers made in the laboratory show that the amount of "ionisation" is roughly proportional to the amount of residual gas left in the dielectric at the moment of impregnation. The characteristic shape of the ionisation curve can be explained on the basis of a dielectric loss proper, which is proportional to the square of the applied voltage, added to an ionisation loss proportional to the first power of the difference between the applied voltage and the "ionisation voltage."

There is little or no positive evidence that ionisation plays an important part in the behaviour of the cable under working condition. Nevertheless, during recent years strenuous efforts have been made to find manufacturing methods which will reduce the ionisation to a minimum. By a stringent application of scientific methods to the physical and chemical problems involved, it is now possible for a long series of the highest-grade cables to be made in none of which does the "ionisation" over a range of one-fourth to twice the working voltage exceed 0 0001 radians. It will be appreciated from the account given above that it does not follow that the ionisation of the cable will remain low after the cable has been passed through heat cycles. The second main effort of the manufacturer has been directed to finding manufacturing methods which will produce a cable in which the ionisation remains at a negligibly low level, while the cable is passed through a long series of heat cycles with high voltage maintained on the cable. These tests, which may be continued for periods of two years or more, are called stability tests. The problem is a complicated and difficult one. One method which has been used with some success is to make the cross-section of the cable deliberately non-circular. Generally an elliptical conductor is used. When the compound expands, the necessary volume accommodation can be obtained by a small deformation of the laminations of the dielectric into a slightly more circular cross-section: the compound is thus not forced through the laminations, and when subsequent cooling takes place, it is not impossible for the dielectric to return to its original state.

Ionisation in a cable, presumably acting in the manner described above, is known to result in condensation of the compound into a heavy hydro-carbon, producing a yellowish deposit generally known as "cheese." In the literature this is frequently termed polymerisation of the compound, but as the action is always accompanied by the evolution of hydrogen and volatile hydro-carbons, it can strictly only be described as a condensation. This condensation of the compound is necessarily accompanied by an increase of void or gas-filled space, and an increase in the amount of the ionisation losses. In exceptional cases, these additional losses might contribute to throwing the dielectric into thermal instability, as will be discussed later; but apart from this case, it would appear from very recent work that ionisation in the mass of the dielectric never

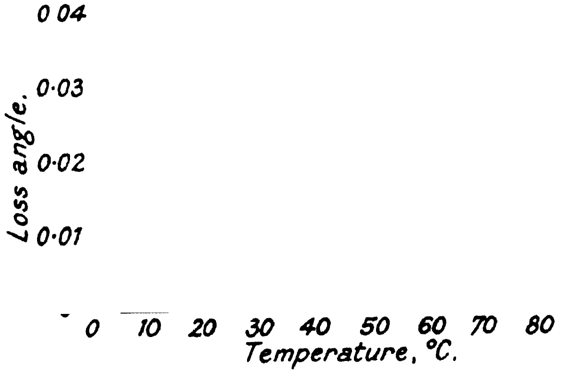


FIG. 3.—V Curve of dielectric loss angle against temperature.

results in any deleterious or particularly undesirable action. When the ionisation occurs, however, in contact with a metal electrode, a very different state of affairs exists. In this case, the ionisation results in the compound being converted into carbon. The extra stress round the pin-head of carbon produces a prolongation of the action, and by means of this "spear-head" attack, a carbon track is slowly propagated through the dielectric. These tracks are generally known from their shape as "ionisation trees." When the trees extend from the inner to the outer electrode, breakdown would occur. Normally, however, before this stage is reached, the energy losses, due to the semi-conducting tracks in an electric field, result in the dielectric being thrown into thermal instability, and the final breakdown is due to this cause. When D.L.A. is measured as a function of temperature, most cable dielectrics give the characteristic "V curve" shown in Fig. 3. Once the dielectric has reached the upward portion of the curve when an increase of temperature results in an increase of the dielectric losses, producing the temperature rise, it is easy to see that in certain circumstances a condition of thermal instability will be produced.

From this recent work it would appear, therefore, that investigation should be directed not so much to the general problem of preventing ionisation throughout the dielectric, but rather to the special problem of preventing it in the neighbourhood of the metal electrodes.

Cables made as described above are generally called of the "normal straight" type. A number of special types have been developed to a high state of perfection, in which special measures are adopted to prevent ionisation arising throughout the dielectric.

In the "oil-filled" cable a longitudinal duct is provided generally by means of a hollow conductor. The cable is impregnated and the duct filled with a thin hydro-carbon oil of low viscosity with no resin in solution. At the ends of the cable the oil ducts are connected to oil reservoirs. As the cable heats up, oil flows out of the cable along the duct into the reservoir, and when the cable cools it is driven back by the positive pressure in the reservoir into the dielectric By these means, not only is the cable very easily impregnated with the thin oil, but the drainage problems which prevent the resin content of normal straight cables being reduced, are overcome. As a result, the cable can be operated at high electric stress , but as can be easily seen, in carrying out the arrangement in practice a number of complicated hydraulic problems, both static and dynamic, must be faced. These generally require somewhat elaborate gear for their solution. At the high electric operating stress, the oil must be kept free of all gas in solution. The terminal reservoirs must not, therefore, have an oil-gas interface. The reservoirs have, therefore, to be built up of collapsible metallic containers, somewhat like gigantic aneroid cells, and the interior must be kept 100 per cent. full of oil.

As soon as the gaseous ionisation problem was clearly appreciated, there was a general move in the direction of utilising the known effect of pressure in increasing the breakdown strength of gases. Almost all gases show this increase up to pressures of about 150–200 lb. per sq. in. Fig. 4 shows the effect of pressure in eliminating or extinguishing gaseous ionisation. A number of methods have been evolved. In one cable which is in successful operation in London, a three core cable of the normal straight type complete with lead sheath is drawn into a large diameter iron pipe, and after complete installation the space between the iron pipe and the cable is filled with nitrogen at about 150 lb. per sq. in. In this case, the pressure is exerted on the dielectric through the flexibility of the lead sheath, and the result is such that the cost of the iron pipe is recouped by the higher stresses at which the dielectric can be operated.

Such a cable is called a "gas compressed" cable, because the gas is utilised merely as a medium for bringing pressure to bear on the dielectric. In the "gas impregnated" cable, an attempt is made to utilise the high dielectric properties of compressed gas itself. Compressed gas alone would form a perfect dielectric if it were not for the necessity of rigidly locating the conductor with respect to the outer electrode. Any attempt to do this by means of supporting spiders or discs introduces an entirely new and very disturbing electric phenomenon—that of surface flashover. Very little is known about this matter; the insulation engineer looks in vain for the guidance of any fundamental theory. But as a matter of empirical test it is found that when an interface between a gaseous and a solid dielectric lies in the direction of an electric field, breakdown will occur along the surface at electric stresses approximately one-tenth those that either dielectric would withstand in the absence of the interface.

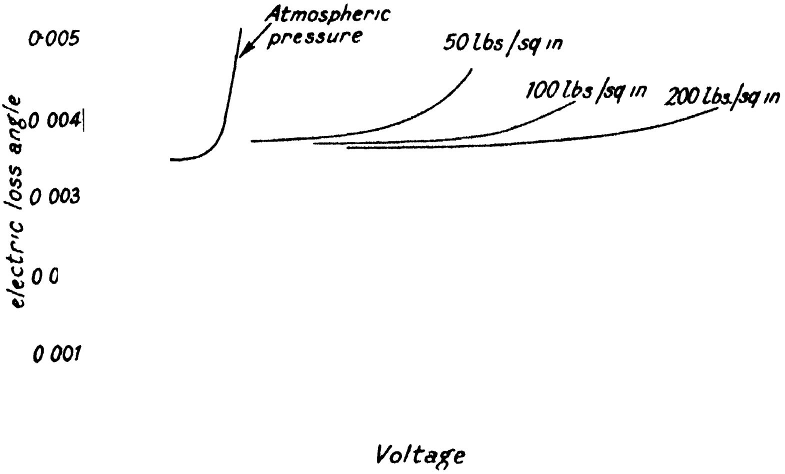


FIG. 4.—"Ionisation" curves showing the effect of pressure.

Any supporting spider or disc arrangement is thus immediately ruled out. The most satisfactory arrangement appears to be, as the name "gas impregnated" implies, a dielectric consisting of normal cable paper laid on and dried in the usual way, and then "impregnated" with dry gas at a pressure of 150–200 lb. per sq. in.

Proposals have also been made to utilise for cable purposes the high breakdown strength of a very hard vacuum. It is well known, of course, that with increasing vacuum the breakdown strength of gases falls until the mean free path becomes comparable with the distance between the testing electrodes after which it rises until, with valve hardness, enormously high dielectric strengths are reached. Here again, the surface flashover difficulty limits the possibilities of the centralising arrangement; and when the centralising material is in position it appears very improbable if the necessary high degree of vacuum could be maintained over the long dimension of the cable between terminal stations.

A cable such as the gas-impregnated cable introduces many novel considerations, apart from the question of dielectric strength. The thermal resistance is high compared with that of impregnated dielectric The first consideration of the manufacturer may be to produce a cable which will not break down, but when the cable is handed over to the distributing authority, the service that can be obtained from it is entirely determined by thermal factors. If the total thermal resistance surrounding a conductor is doubled, the current that can be passed through the cable for a given safe temperature limit is reduced by 30 per cent. (the heat generated in the conductor depending on current squared).

The thermal resistance of a gas impregnated cable designed to operate on a 132 kV. transmission system is in the region of 500 thermal ohms per cm. length of cable. (The thermal ohm is a unit of thermal resistivity corresponding to the reciprocal of the normal unit of thermal conductivity, but based on the watt-second instead of the calorie for the unit of heat.) The corresponding figure for the oil-impregnated cable is 120 thermal ohms This, at first sight, appears to be a serious disadvantage. Actually, in the majority of cases, the internal thermal resistance is swamped by the external resistance. In urban districts such as London, the congestion below the street levels is so acute that it is necessary to go to a depth of perhaps 30 ft. to find sufficient room for a high voltage feeder. This increases the thermal resistance of the earth path. In addition, the thermal interference from neighbouring cables can most conveniently be expressed as an equivalent increase to the external thermal resistance. As a result, it is not uncommon for the external resistance to rise to 500 or even 900 thermal ohms per cm.

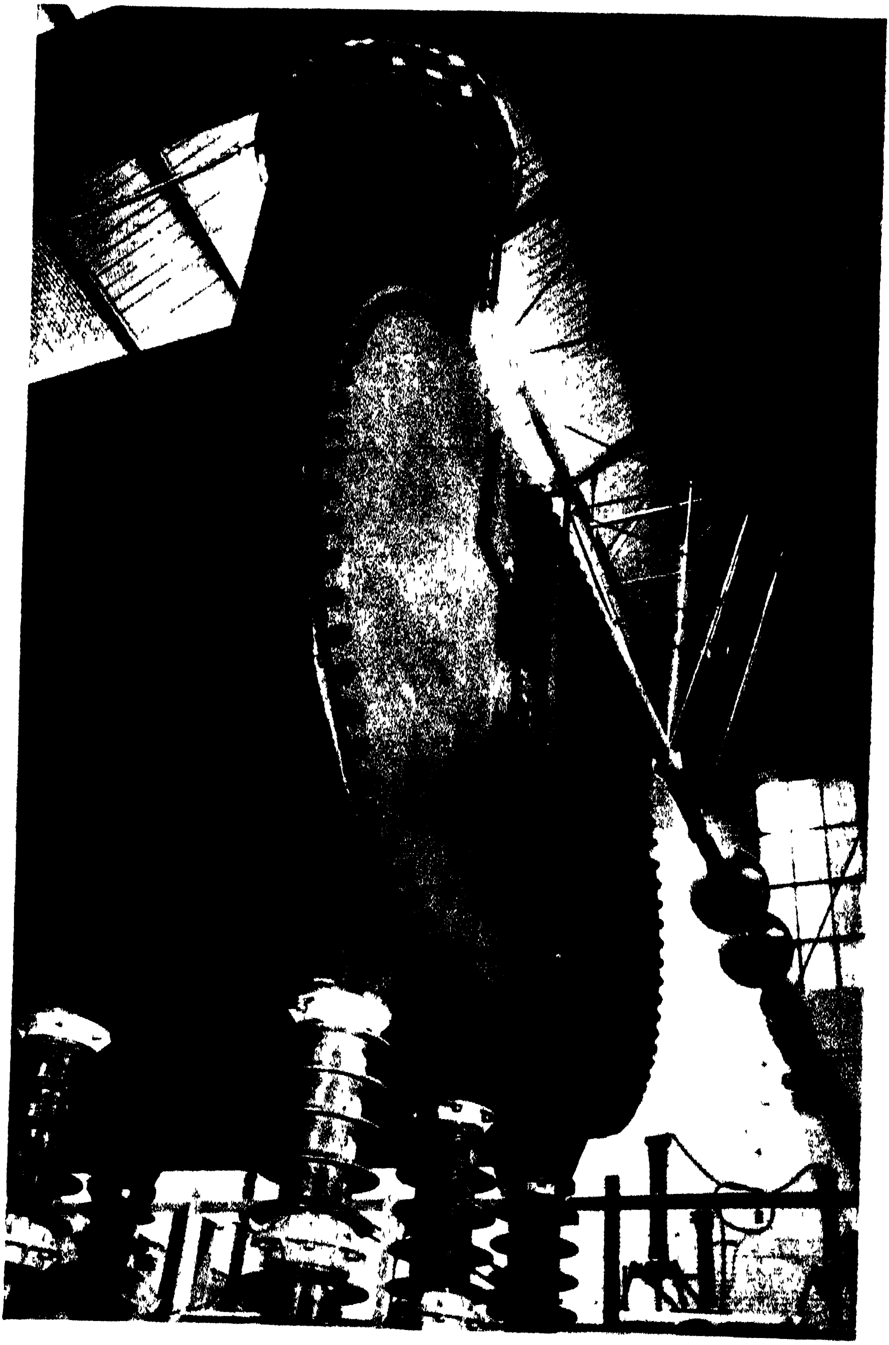
Another important difference arises from the low permittivity of the gas-impregnated cable ($\sigma = 1{\cdot}5$) compared with $\sigma = 3{\cdot}5$ for the oil-impregnated dielectric. Without experience it is difficult to realise the enormous capacitance current that is required to keep a high voltage feeder energised, apart entirely from the transmission of any useful energy. It is true that the current is in quadrature with the voltage, and does not primarily require any energy, but in practice these capacitance currents involve large no-load losses which are continuous, whether load is being transmitted or not. For example, a 40-mile length of 132 kV. feeder of oil-impregnated dielectric requires a capacitance current of 280 amperes, to supply which it is necessary to keep running at the supply station an alternator of 21,000 kVA (28,000 h.p.) output. Apart from the cost of keeping such large plant running with possibly no useful output at the far end, there is the further difficulty that the capacitance current mentioned (280 amperes) may, in some circumstances, equal the permissible full load of the cable. That is to say, with this type of cable when the feeder reaches a length of 40 miles the first sections of the feeder are fully occupied in supplying the feeder capacitance current, and there is no margin for the transmission of useful energy. This is expressed by saying that the self-loading length for this type of cable is 40 miles. With a gas-impregnated cable of the same current carrying capacity, however, the charging kVA. is, for 40 miles of feeder, reduced to 8,000 kVA. With the continual growth both of voltage and the length of transmission feeders, this is likely to become an important advantage.

From this brief and general account, it will be seen that a research laboratory in the cable industry is not likely to find itself at a loss for lack of problems to be investigated.

The great expense of, and the delay involved in, making full-scale experimental cables must always make one of the primary concerns in the laboratory the problem of determining a technique by which dielectric studies made on experimental condensers prepared in the laboratory, can, with confidence, be taken as representative of the results that can be achieved with full-scale cables. This proves to be a surprisingly difficult problem The D.L.A. of oil-impregnated dielectric is so susceptible to minute quantities of products which are obscurely formed by the interactions between the oil, resin, paper, copper, and materials of the drying and impregnating tanks and connecting pipes, that it appears to be impossible to change the scale of operations without affecting the reliability of the results. As a result, it is found, for example, with one particular type of dielectric, that using the best possible laboratory technique and starting with identical materials, it is not possible to produce a laboratory condenser with as low a value of D.L.A. as that of full-scale cables made in the factory.

We may consider one particular feature—the drying of the paper before impregnation. It is found that there is no definite

PLATE I



High-voltage transformer giving 500 kVA at 500 kV, mounted on insulators for cascading with a similar transformer to give 1000 kV end point to the drying of paper. Whether the drying is carried out by vacuum or by a combination of vacuum and heat, it is found to merge imperceptibly into the beginning of the destructive distillation of the paper. The problem of the cable maker is to select the particular stage in the process which will give the best overall results. It is, however, at once clear that in conditioning the materials to be used in making a laboratory condenser, there is no short cut permissible, and that the laboratory processing must slavishly simulate the manufacturing processes. As this involves drying and impregnating times which may total up to periods of 400 hours or more, it will be seen that without careful pre-arrangement, laboratory condenser studies do not necessarily provide a short or economical cut to the determination of the most suitable dielectric.

The research testing of full-scale cables requires specialised testing equipment in the laboratory. The development of a cable for a 132 kV. transmission system requires transformer equipment to give a testing voltage of about 500 kV. Already, however, transmission systems of 220 kV. are in operation abroad. A desirable arrangement in the laboratory, therefore, is to have two transformers of about 500 kV., arranged so that the voltage outputs can be put in series to give 1,000 kV. if necessary. Apart from the possibility of tests on 220 kV. cables in the future, this provides for flashover tests on the porcelain insulators and terminal equipment of the lower voltage systems. This arrangement of the transformers is called a cascade connection. One of the transformers is mounted on porcelain insulators suitable for withstanding 500 kV. The electric supply for the primary of this transformer then has to be obtained from a special winding located at the high voltage end of the first transformer in the manner shown in Fig. 5. In order to supply the large capacitance currents already mentioned, these transformers must be of large current output. Plate I shows such a transformer mounted on porcelain insulators, and designed for 500 kVA. output at 500 kV; or when the two transformers are cascaded, 1,000 kVA. at 1,000 kV. In order that the output of the two transformers shall be additive to give a total of 1,000 kVA. the first transformer is specially arranged so that in addition to transferring 500 kVA. of energy from its primary to secondary windings, it can transfer a further 500 kVA. from the primary to the special winding supplying the primary of the second transformer.

In Plate I the iron core and copper wire windings are located in a horizontal position in the welded steel tank immediately above the porcelain insulators. The vertical cylinder is a bakelite tube

140 cm. in diameter and 300 cm. long forming the insulation for the high voltage terminal which is the bright copper spinning at the top of the bakelite tube. This transformer weighs 40 tons; and it will be appreciated that with testing equipment of this size and weight, the technique of A.C. bridge measurements becomes a very different matter from that which is suitable for the physicist's bench.

The control or regulation of the voltage output of these transformers presents many difficulties. It is obviously desirable for research purposes that the wave form of the output voltage should be as nearly sinusoidal as possible. From a number of considerations this rules out the use of induction regulators. The normal method is, therefore, to install an alternator specially designed to give a pure sine wave output, and equal in capacity to the output of the transformer. The voltage output of the alternator, and thus of the transformer is then controlled by means of the D.C. field supply to the alternator. With this arrangement the frequency of the testing supply depends on the speed at which the alternator is driven. The Schering Bridge already mentioned is used in conjunction with a specially developed vibration galvanometer with an extremely sharply tuned response curve, so that a deviation of 0·5 per cent. in the frequency involves a complete loss of galvanometer sensitivity. It is, therefore, necessary for the speed of the alternator to be held constant to within say 0·2 per cent. When

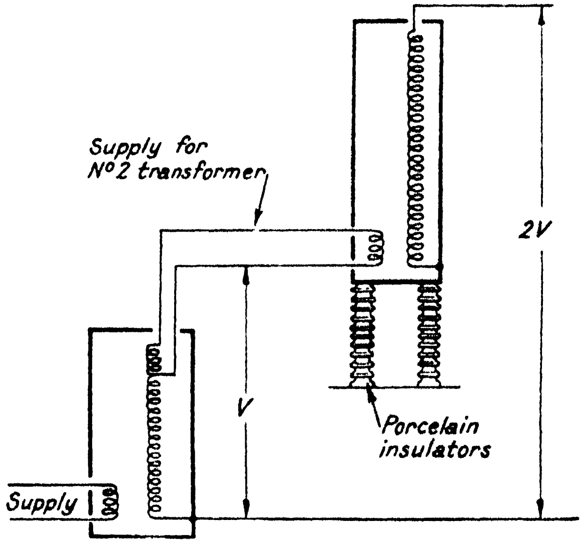


FIG. 5.—Cascade connection of transformers.

the main supply is drawn from one of the large time controlled grid systems now available this can be obtained by driving the alternator with a synchronous motor. This has the disadvantage of limiting the testing to one particular frequency. In order to obtain a frequency range, it is necessary to interpose between the synchronous motor and the alternator a speed-changing arrangement, which, for any setting, will hold the speed within the above limits. In one case, a successful arrangement has been obtained by using a fluid gear box, the gear ratio of which can be continuously varied over a range corresponding to frequencies of 25–75 cycles per second.

A completed transmission feeder involves problems over a much wider field than has been discussed here. The cable has to be enclosed by a lead sheath which must be virtually homogeneous, free from extrusion weaknesses, and proof against the strains of installation and subsequent severe vibration; suitable methods have to be found for the mechanical and chemical protection of the lead sheath; and high voltage joints and cable terminations have to be designed and manufactured on site. It has only been possible within the scope of this article to deal with the dielectric of the high voltage cable, but within this limitation an attempt has been made to give a general account of the problems involved and the methods by which these problems are being attacked.

THE STRUCTURE OF MOLECULES

By A. J. MEE, M.A., B.Sc.

One of the most outstanding advances in physical science of recent years is the investigation of the structure of molecules by physical methods. This advance has followed naturally the increase in our knowledge of the structure of the atom, for before any information concerning the molecule could be obtained, it was necessary to know fairly accurately the nature of its components.

The conception of the molecule originated with Avogadro's Hypothesis, and the discovery of the volume relationships of combining gases embodied in Gay-Lussac's Law. The atomic theory of Dalton was, at this time (1811), almost universally accepted by chemists and physicists. It postulated the existence of atoms of elements, and of "compound-atoms" of compounds, and Dalton firmly believed that neither the elementary nor the compound atom could be further divided. Avogadro, however, showed that Gay-Lussac's Law necessitated the possibility of breaking down the "compound-atoms" into their elementary constituents, and, in order to avoid the confusion which might arise by calling these compound particles atoms, thereby implying their indivisibility, the term "molecule," meaning literally "a little mass," was applied to them.

To Dalton and those of his contemporaries who believed in the atomic theory, the atom was a hard sphere, with a definite mass. Since the molecules were made up of atoms, it was natural to assume that they too were solid, being composed of atoms in contact. This view of the structure of the molecule was held until the beginning of the present century, and was indeed used very successfully in the derivation of the quantitative laws governing the behaviour of gases by the kinetic theory.

As the result of the work of Thomson, Rutherford and Bohr, to mention only three of the outstanding workers in the field of atomic structure, the conclusion was reached that the atom was not to be regarded as a solid sphere, but that it was entirely electrical in nature, consisting of a nucleus, predominantly positive in charge, and a number of electrons moving in definite orbits round the nucleus. The number of these planetary electrons is just sufficient to neutralise the charge on the nucleus, making the atom electrically neutral, and is equal to the number of the element in the series of elements arranged in order of ascending atomic weight, taking into account the undiscovered elements. Such is the present state of our knowledge concerning the atom, that the actual orbits of the electrons are known, and models of the atom can be constructed.

It is now generally regarded that electrons cannot be merely charged particles, but that they have associated with them a definite wave-motion, since it is possible to obtain interference between beams of electrons, just as if they were trains of waves. For our present purpose, however, the actual nature of the particles is not of such importance as their distribution.

This, then, is the picture of the atom. It consists of an assemblage of electrical charges, and of uncharged particles, the latter being the neutrons which are constituents of the nucleus. That there is a considerable amount of empty space in the atom is evident from the experiments of Rutherford on the scattering of α-particles at metal foils. The majority of the particles were transmitted right through the foils without deviation, showing that they could not have encountered any charged particle of any considerable size. The whole of this empty space is, however, permeated by a powerful electric field, which is effective to a certain distance outside the outermost electronic orbits. It is obvious that, far from being solid, the atom is nearly all empty space.

Yet, on the other hand, the molecule often behaves as if it were solid, especially as far as collision with other molecules is concerned. This is due to the fact that the interaction between the electric fields of the colliding molecules prevents the nuclei from coming closer together than a certain distance, and there is repulsion, just as if the two molecules were solid and elastic. The distance between the molecules when " collision " takes place is accurately known for many molecules.

The problem which has been attacked with a considerable amount of success within recent years is that of the arrangement of the atoms in the molecule, the distances between the nuclei of the constituent atoms, and the angles between the valency bonds. Consideration of the chemical properties of compounds has, in the past, enabled us to assign to them " constitutional " formulæ, which indicate the relative positions of the atoms in the molecule. These have been of great service, particularly in the development of organic chemistry. Many of them have received confirmation from the new physical methods of investigation, whilst some have been discredited. Moreover, the physical methods are able to shed considerable light on many other problems of structural chemistry, such as free rotation, and stereochemistry.

The first question to be answered is, in what ways are atoms united in the molecule ? The electronic theory of valency is able to supply a very satisfactory answer. According to this theory, there are three methods of linking atoms. The linkage may be electrovalent, covalent, or co-ordinate. In the electrovalent linkage the atoms in the molecule attain the stable electronic configurations of the inert gases by a process of transference of electrons. Consider, as a simple example, the sodium chloride " molecule." The sodium atom has the electronic structure 2, 8, 1 ; the chlorine atom, 2, 8, 7. It is clear that the sodium atom could attain the structure of the inert gas neon (2, 8) by the loss of one electron, whilst the chlorine atom could attain that of argon (2, 8, 8) by taking up one electron. The sodium and the chlorine atoms could both be satisfied if the sodium atom transferred one electron to the chlorine atom. Thus, representing the electrons in the outermost orbit (valency electrons) by dots, the sodium chloride molecule would be :

$$\mathrm{Na}:\overset{\cdot\cdot}{\underset{\cdot\cdot}{\mathrm{Cl}}}:$$

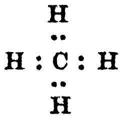
The sodium atom, having lost an electron, has become positively charged, and the chlorine, having gained one, has become negatively charged. These charged atoms are called ions.

The only force keeping the sodium and chlorine ions in proximity to each other is the electrostatic force of attraction between oppositely charged particles. Assuming that the Coulomb Law of Force holds at the small distance under consideration (in the neighbourhood of 10^{-8} cm.) this force would be

$$F = e^2/kr^2,$$

where e is the charge, r the distance of the centres of gravity of the system of charges associated with each ion from each other, and k is the dielectric constant. In the molecule itself, the dielectric constant is 1. When such an electrovalent substance is dissolved in water, of which the dielectric constant is very high (approximately 81), the force, F, becomes very small, and the energy of the temperature motion is sufficient to separate the ions, and the substance dissociates. Molecules with an electrovalent linkage are ionised even in the solid state, X-ray analysis of the crystal structure indicating that the elementary particles making up the crystal lattice are ions, and not atoms or molecules. When the crystal lattice of an ionic crystal, such as sodium chloride, is considered, it is found that the sodium ion, say, cannot be regarded as belonging to any particular chlorine ion, but is shared by a number of them, and similarly every chlorine ion is shared by a number of sodium ions, so that a molecule of sodium chloride can hardly be spoken of. It is clear that the electrovalent linkage is a very weak bond, if it may be regarded as a bond at all, since no real structural bond exists.

Nearly all inorganic salts are electrovalent compounds. A very different state of affairs is found when organic compounds are considered. These compounds are usually not dissociated in aqueous solution, and do not form ionic lattices. They possess, instead of the electrovalent linkage, a covalent linkage. In this case, the atoms attain the stability of the inert gas configurations by a process of sharing electrons. Thus, in methane, CH_4, the carbon atom itself has four valency electrons (its structure is 2, 4). It can attain the stable arrangement of electrons found in the neon atom by sharing these four electrons with four electrons of four hydrogen atoms, thus,

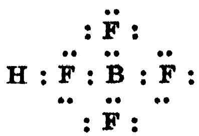


$$\begin{array}{ccccc} & & H & & \\ & & \cdot\cdot & & \\ H & : & C & : & H \\ & & \cdot\cdot & & \\ & & H & & \end{array}$$

The hydrogen atoms also have attained the electronic configuration of helium. The bonds between the carbon and the hydrogen atoms are covalent linkages, and are much more definite bonds than the electrovalent linkages. There is no actual charge on the individual atoms, and the external electric field of such a molecule is much smaller than that of an electrovalent "molecule."

The actual mechanism of the sharing of electrons has been one of the most difficult problems that the electronic theory has had to face. It seems that the simplest explanation, *viz.* that the two shared electrons may form orbits which enclose both atoms, is probably incorrect. A solution of the problem has been given by London [1] on the basis of quantum mechanics, but it cannot be entered into here.

The third type of linkage, the co-ordinate linkage, involves the sharing of two electrons, but both are supplied by the same atom. Thus, in the formation of hydrofluoboric acid, HBF_4, boron trifluoride combines with hydrogen fluoride, the boron atom completing its octet by sharing two electrons with the fluorine atom of hydrogen fluoride, as shown in the scheme below.



Both of the shared electrons have been supplied by the fluorine atom. Since the process of the formation of a co-ordinate linkage resembles both transference and sharing, it has been called by Lowry, the semi-polar double bond, and is usually written $A \overset{\rightarrow}{=} B$.

This very brief outline of the electronic theory has been given because we shall have occasion to use the terms employed above; moreover, the work on the structure of molecules applies only to covalent and co-ordinate compounds, since, as we have seen, electrovalent compounds have no molecular structure [2].

A molecule is a system of positive and negative charges, the positive charges being resident upon the nuclei of the component atoms. It is clear that a very important constant of a molecule will be the inter-nuclear distance, for this must form the basis upon which any further information, such as the valency angles, must rest. There are several methods of determining the inter-nuclear distance.

The first of these is the method of *X*-ray interference, with which may also be considered the method of electron interference, which is essentially similar to it. It was shown by Debye [3] and Ehrenfest [4] independently, that *X*-ray diffraction patterns could be obtained with gases as with solids. From the angle relationships between the maxima and minima of the interference, and the intensity of the maxima, the relative positions, and distances apart of the scattering centres could be calculated.

The experimental technique of the investigation of *X*-ray interference in gases is very similar to that used by W. H. and W. L. Bragg in dealing with crystals. A beam of *X*-rays, made as far as possible homogeneous by a series of filters, is passed through a cell containing the gas under investigation. The scattered radiation falls on a film wound round the cylindrical cell, and the photograph obtained is examined by a registering photometer. In this way, the intensities of the scattering maxima are obtained. Then probable models for the molecule are chosen, and the scattering which would be produced by these models is calculated. The form for which there is agreement between the calculated and observed scattering is taken as correct. The method of carrying out a Fourier analysis of the scattering curve (the curve between the angle of scattering and the intensity) has not yet been successful in the case of gases, although it is the usual method of deducing the structure of a crystal from the scattering.

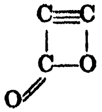
In the electron beam method, which was devised by Mark and Wierl [5], a stream of the vapour of the substance under investigation is passed into a vessel in which a high vacuum is maintained. Immediately after its entrance, the stream of vapour is irradiated with a fine bundle of rapid electrons, and the interference is photographed. The advantage of the electron beam method is that it requires only a fraction of a second exposure, whereas the *X*-ray method requires an exposure of from four to five hours.

In the case of *X*-ray interference, the scattering centres are not the nuclei themselves, but the orbital electrons also contribute to the scattering. The centre in this case is the mean centre of the electronic orbits, which does not necessarily coincide with the nucleus. With electron interference, however, the scattering centres are the nuclei.

X-ray interference at crystals may also be used to determine the shapes and sizes of molecules. All crystalline organic compounds form molecular lattices. The molecules are held together in these lattices by van der Waals forces, which are small compared with the binding forces. Hence, the molecule is not likely to undergo any considerable deformation in the crystal lattice, and the results obtained for the inter-nuclear distances from *X*-ray interference experiments may be taken as correct for the free, unrestrained molecule.

From the observations, the dimensions of the unit cell may be derived, and from this, the arrangement of the molecules making up the lattice may be obtained by making use of the relative intensities of the various maxima. If absolute intensity measurements are made, it is possible to analyse the scattering intensity curve by a Fourier analysis [6] and obtain the exact positions of the molecules. This method has been used successfully by Lonsdale for hexachlorobenzene [7], and by J. M. Robertson [8] for naphthalene, durene (1.2.4.5-tetramethyl-benzene), and dibenzyl, $C_6H_5.CH_2.CH_2.C_6H_5$.

A few of the more outstanding results of the method of *X*-ray or electron interference at crystals or vapours will be mentioned. Carbon dioxide has been proved to have a linear structure, a deduction which is confirmed by other methods to be mentioned later. Nitrous oxide is also linear, but sulphur dioxide is bent, the angle at the sulphur atom being, according to Wierl [5], greater than 160°. This angle, however, is too great, since, although the bent form is confirmed by other methods, the angle derived from them is more in the neighbourhood of 125°. Ammonia forms a pyramid, with nitrogen at the apex, and the three hydrogen atoms in a plane, forming the base. Carbon suboxide, C_3O_2, was given the linear form O=C=C=C=O by Brockway and Pauling [9], but recently Boersch has proposed the cyclic form,



[10].

From a comparison of the scattering intensities obtained with the long-chain normal hydrocarbons, such as $C_{20}H_{42}$, $C_{24}H_{50}$, etc., it has been found that the crystal lattice increases in length in one direction only, as the length of the chain is increased, the other two dimensions remaining approximately the same. The increase in length is found to be uniform as the length of the chain increases, being about 1·2 Å per carbon atom. This result indicates that in the crystal, the hydrocarbon chains are arranged parallel to each other, and from the identity period of the scattering, the length of the molecule can be obtained. The length of the chain thus derived is not consistent with a linear arrangement of carbon atoms, supposing that the distance between the carbon atoms is 1·5 Å, as obtained by other methods. It is therefore concluded that the molecule is zigzag in form, the valency angle being the tetrahedral angle of 110°, and the distance between one carbon atom and the next but one being 2·44 Å. These results are in good agreement with the work of Adam and Langmuir on thin films of the fatty acids on water.

The examination of *cis*- and *trans*-isomers by the *X*-ray and electron beam interference methods often enables a direct classification of the isomers to be made. Thus, the dichlorethylene boiling at 59° has a distance of 3·7 Å between the chlorine atoms, whilst that boiling at 48° has a distance of 4·7 Å, both determined by the *X*-ray interference method. It is obvious that the second of these is the *trans*-isomer.

One of the most interesting of molecules is benzene. Cox [11] has found, from intensity determinations, that the six carbon atoms are arranged in the form of a regular hexagon, whilst Wierl, from electron diffraction by the vapour, found the length of the side of the hexagon to be 1·39 Å.

Another, and quite different, method of investigation of the structure of molecules makes use of the absorption spectrum in the infra-red. When the infra-red absorption spectrum of a gas is examined it is found to consist of two groups of bands, one in the near infra-red (*i.e.* of wave-lengths not far removed from that of the red of the visible spectrum), and another of much longer wave-length. The latter is known to be due to the rotation of the molecule as a whole, and is referred to as the pure rotation spectrum. The former is due primarily to the vibration of the molecule, but a pure vibration spectrum is not found, the frequencies of the rotation spectrum being superimposed upon it. This spectrum is referred to as the rotation-vibration spectrum. A molecule will also give rise to a band spectrum in the visible, due to electronic changes in the atoms themselves. The frequencies, however, are not simply those due to the electronic changes, but have, superimposed upon them, the frequencies of the rotation and the vibration.

The rotation-vibration spectrum has received greater attention than the other types of molecular spectra, since the experimental technique is much simpler in the near infra-red than in the far infra-red. Prism spectrometers may be used in this range, since substances are known which will transmit radiation in this region. For the far infra-red a grating has to be used. The spectrum may be detected by converting the energy into thermal energy and detecting this by means of a bolometer, or other suitable means. In this way, not only may the position of the absorption bands be determined, but their intensity may be obtained. Infra-red photography has been used to photograph the spectra, this method having been employed by Allibone [12] in the examination of the spectrum of hydrogen.

The value of the study of infra-red absorption spectra lies in the fact that it enables the moment of inertia of the molecule to be determined, and from this the inter-nuclear distances can be obtained. It is fortunate that the frequency differences between consecutive lines in the pure rotation, and the rotation-vibration spectra, are the same. This frequency difference, $\delta\nu$, can be shown to be equal to $\frac{h}{4\pi^2 I}$, where h is Planck's constant, and I is the moment of inertia of the molecule about an axis drawn through the centre of gravity, and perpendicular to the axis of the figure. Since the differences are the same for the two spectra, it follows that the moments of inertia are the same in the two energy states, and hence I can be determined from the near infra-red spectrum just as accurately as from the far infra-red [13].

The moment of inertia of a diatomic molecule is given by

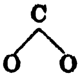
$$I = \frac{m_1 m_2}{m_1 + m_2} r^2,$$

where m_1 and m_2 are the masses of the atoms, and r is the internuclear distance. It is clear that if I is found from observations of the infra-red spectrum, r can easily be obtained, since m_1 and m_2 are known.

If the molecule is polyatomic, it will possess, in general, more than one moment of inertia, and the value of these moments may then be deduced from observations of the fine structure of the rotation-vibration spectrum.

The derivation of molecular structures from spectroscopic data has been considerably facilitated by the use of the Raman spectrum. When a beam of monochromatic light is passed through a transparent substance, some of the light is invariably scattered by the molecules of the medium. The spectrum of this scattered light lies in the visible, or ultra-violet, and can be readily investigated. It is found that the spectrum consists of lines on either side of the line due to the original radiation, and that the frequency differences between the Raman lines and the original line are equal to the infra-red absorption frequencies of the molecule concerned. In this way, the infra-red spectrum is, so to speak, brought into the visible. A considerable quantity of experimental data has been amassed concerning the Raman spectra of various molecules, and much of it remains to be dealt with from the theoretical aspect. The Raman effect is not simply the transference of the infra-red frequencies into the visible. Some of the infra-red lines are "inactive" in the Raman spectrum, *i.e.*, they are missing in the Raman spectrum, and conversely Raman lines appear which are inactive in the infra-red. Various selection rules have been derived, largely by Dennison, which state whether a particular frequency will appear or not in the Raman and infra-red spectra. By the analysis of these spectra, and bearing in mind the selection rules, the various proper frequencies associated with the molecule may be derived, but the process by which this is done cannot be gone into here [14]. The proper frequencies of molecules may also be derived from a study of the variation of the specific heat with temperature, a method which has been applied successfully by Eucken and his pupils.

A few examples of the application of spectroscopic methods to the determination of the structure of certain molecules will now be given. A molecule which has received a great deal of investigation by this method is carbon dioxide, CO_2. There are three possible forms for this molecule. It may be bent (form *a*), or linear, and, if linear, it may be symmetrical (*b*), or unsymmetrical (*c*).



(*a*)

O—C—O

(*b*)

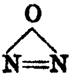
O—O—C

(*c*)

It was at first considered that the fact that three proper frequencies were obtained meant that the molecule was bent, but more recently, the analysis of the fine structure of some of the bands has been carried out by Martin and Barker [15], and the linear symmetrical form has been shown definitely to be correct, one of the three proper frequencies not being a ground frequency. This work is confirmed by the analysis of the rotational fine structure of the electron band spectrum [16], and the fact that the molecule has no electric moment (see later)

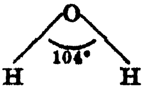
Carbon disulphide, CS_2, of which the infra-red spectrum has been investigated by Bailey and Cassie [17], is linear like carbon dioxide.

Nitrous oxide, N_2O, would be expected to be a symmetrical linear molecule, on account of its analogy to carbon dioxide, with which it is isosteric (isosteric molecules are those for which the sum of the atomic numbers of the constituent atoms is the same). If this were so, the molecule would be $N\equiv O\equiv N$. The molecule, however, possesses a small electric moment ($0{\cdot}14 \times 10^{-18}$), and cannot therefore be symmetrical, nor can it possess the possible ring form



It must therefore be linear but unsymmetrical, $N\equiv N=O$. This has been confirmed by the analysis of the infra-red spectrum by Plyler and Barker [18], who investigated twelve bands between 2μ and 17μ.

Triatomic bent molecules are much more difficult to investigate than are linear molecules. They act as unsymmetrical rotators, and the classification of the lines of the rotation spectrum to the transitions between the different rotational states is a matter of some difficulty. It has been accomplished, however, for the water molecule, which has been shown to be bent, with an angle of 104° between the two "legs," the distance between the oxygen and hydrogen atoms being 1·013 × 10^{-8} cm., and that between the two hydrogen atoms, 1·53 × 10^{-8} cm.



Analysis of the infra-red spectrum of ammonia shows that it is a symmetrical rotator. The pure rotation spectrum consists of a set of equidistant lines—a sign of the symmetrical nature of the molecule. The molecule also possesses an electric moment (1·46 × 10^{-18} e.s.u.), which shows that it cannot be planar. The nitrogen atom must therefore be situated at the apex of a pyramid, of which the base is an equilateral triangle, the corners of which are occupied by hydrogen atoms. The same result, it will be remembered, was obtained as the outcome of *X*-ray and electron interference investigations. The angle between the N—H linkages is probably a little over 90° (Badger and Mecke obtain the value 94°), whilst the angle between the N—H linkage and the vertical dropped from the apex of the pyramid to the base is about 58°. The pyramid is, therefore, comparatively stumpy.

A large number of other molecules have been investigated, but it is impossible to mention the results within the space of this article.

There is another method of investigation which has yielded most important results, and that is the determination of the dipole moment of a molecule. Since the molecule consists of an assemblage of electric charges, when the molecule is placed in an electric field, the electrons and protons (or positive nuclei, if the neutron is included) will be attracted or repelled according to the direction of the field, and hence the electronic orbits will be somewhat distorted. This effect is known as electron polarisation, in the case of the displacement of the electrons, and atomic polarisation when the nuclei are considered.

It is possible, however, that the centres of action of the positive and negative parts of a molecule may not coincide, and the molecule may then be imagined to consist of an electrical system which reduces to two point charges, equal and opposite in sign, separated by a rigid link. If e is the charge, and d the length of the link, the product ed is the moment of the system, and is called the permanent dipole moment of the molecule. As has been stated above, all molecules possess a dipole moment when placed in an electric field, owing to the relative displacement of the negative and positive parts of the molecule, but there are some molecules which possess such a moment irrespective of whether they are placed in a field or not, and it follows that these molecules must be unsymmetrical in some way.

All electrovalent molecules will possess a permanent dipole moment, but such molecules do not concern us. In a covalent compound, in which the atoms are linked by shared electrons, the molecule will possess a permanent moment if the sharing is not equal.

There are several methods of determining dipole moments. The most important is based on the determination of the dielectric constant of the compound, which must be in the form of vapour, or in dilute solution in an indifferent solvent. The reason for this stipulation is that interaction between the fields of the molecules of the substance under investigation and those of the solvent would otherwise occur, which would make it difficult, if not impossible, to calculate the permanent moment. An equation expressing the relationship between the temperature and the molecular orientation polarisation, P_0, has been derived by Debye, and takes the form

$$P_o = \frac{4\pi}{3} \cdot N \frac{\mu^2}{3kT},$$

where μ is the permanent moment in electrostatic units, N is Avogadro's number, k is Boltzmann's constant, and T the absolute temperature. The total polarisation, P, is equal to the polarisation already present owing to the permanent dipole (the molecular orientation polarisation) and that due to the distortion of the orbits by the field, which would occur even if the molecule did not possess a permanent moment. The latter polarisation is called P_d. Hence

$$P = P_d + \frac{4\pi N\mu^2}{9kT},$$

$$= A + \frac{B}{T},$$

where $$B = \frac{4\pi N\mu^2}{9k}.$$

This expresses the fact that there is a linear relationship between the polarisation, P, and the absolute temperature, T. If the line is horizontal, then B is zero, and the molecule does not possess a permanent moment μ. In any case, μ can be calculated from the slope of the line. The relationship between the polarisation and the dielectric constant is given by the Clausius-Mosotti relationship

$$P = \frac{\varepsilon - 1}{\varepsilon + 2} \cdot \frac{M}{d},$$

where ε is the dielectric constant, M is the molecular weight and d the density. It is clear that, by combining the Clausius-Mosotti relationship with the Debye equation, a method is provided of determining the permanent dipole moment μ from the dielectric constant.

With modern methods, the determination of dielectric constants is a matter of fair simplicity, and great accuracy. A heterodyne method is now invariably used, and it is now possible to obtain an instrument from which, after the necessary adjustments have been made, the dielectric constant may be read off directly from a scale. This instrument, known as the dielkometer, is marketed in Germany, and has a number of uses, not only for research work of this kind, but in technical practice. The dielectric constant is a valuable criterion of the purity of a substance, so that the instrument may find considerable use in the detection of adulterants.

Another method of determining dipole moments is the "molecular beam" method. Briefly, the method consists in passing a very narrow beam of molecules through an intense non-uniform electric field. The deviation which the beam suffers is measured by condensing the vapour on a plate cooled in liquid air, and measuring the width of the trace. The method has been largely used by Estermann [18].

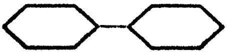
It is difficult to condense into a few paragraphs any detailed account of the results of the determination of dipole moments. Only a few can be mentioned; for the rest, one of the monographs should be consulted [19]. Diatomic molecules, with the exception of the elements, are all polar. Thus, hydrogen chloride has a moment of $1{\cdot}03 \times 10^{-18}$ e.s.u., showing that the electrons binding together the hydrogen and chlorine atoms are not equally shared.

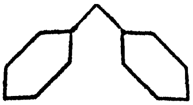
In the case of triatomic molecules, there is the possibility of symmetrical or of unsymmetrical structure. If the molecule is symmetrical, it will possess no permanent moment. If, on the other hand, it is unsymmetrical, a moment is to be expected. As has been stated, water is an unsymmetrical molecule, and possesses a moment of $1{\cdot}85 \times 10^{-18}$ e.s.u., whilst carbon dioxide, being linear, possesses no moment.

Tetratomic molecules of the type AB_3 will possess no moment if all the atoms lie in one plane, but if there is a pyramidal structure, they are polar, and this accounts for the moment of ammonia, already mentioned, and that of the trihalides of the elements of Group V.

Great interest attaches to the determination of the dipole moments of pentatomic molecules of the type CA_4. If the tetrahedral theory of the carbon atom, put forward by Le Bel and van't Hoff, is correct, these substances should possess no moment, being symmetrical. Methane, and carbon tetrachloride possess no moment, but penterythritol $C(CH_2OH)_4$ possesses a comparatively large moment, which has led to a good deal of discussion as to whether this compound possesses tetrahedral symmetry. The astounding thing is that its derivatives, $C(CH_2Cl)_4$ and $C(CH_2Br)_4$, have no moment. The examination of the crystal structure of penterythritol indicates, however, that it possesses tetrahedral symmetry.

The determination of the dipole moment of diphenyl and its derivatives has served to distinguish between the possible formulæ. The molecule may be linear, with the two benzene rings in one plane (*a*); it may be bent (*b*); or it may be folded (*c*), this being the Kaufler formula. It is clear that only the form (*a*) would possess zero moment, and this is actually found.


(*a*)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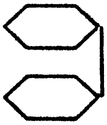
(*c*)

A very interesting application of dipole moments has been made by Hammick, New, Sidgwick, and Sutton in the determination of the structure of the isocyanide group [20], which they find to be $—\overset{+}{N}\overset{\rightarrow}{\equiv}\overset{-}{C}$, thus doing away with the difficulty of divalent carbon.

A study of dipole moments has also proved of use in discriminating between stereo-isomers. Considering the *cis*- and *trans*-dichlorethylenes, it is obvious that the *cis*-form should possess a moment, whilst the *trans*-form should not. This has been fully verified, the moment of *cis*-dichlorethylene being $1{\cdot}8 \times 10^{-18}$ e.s.u., whilst the *trans*-compound has no moment. Where there are no other means of determining whether an isomer is the *cis*- or *trans*-form, the chemist usually assumes that the form with the higher boiling point is the *cis*-form, but a study of dipole moments shows that this criterion is not always true.

The study of dipole moments has also led to a more complete understanding of the problem of benzene substitution.

There is another method of determining the geometrical symmetry of a molecule, and that is by determining the anisotropy of the polarisability of a molecule by means of the electrical Kerr effect. A study of this effect enables the polarisation ellipsoid of the molecule to be determined, and from this the structure of the molecule may be deduced. For the theory and experimental technique the reader is referred to a monograph [14]. This method of attack is of special importance in connection with the determination of the valency angle, and in settling questions of free rotation. In general, results are obtainable by this method for the more complicated molecules, such as the ethers and ketones, for which the other methods fail.

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THE INTERPRETATION OF ANIMAL BEHAVIOUR [1]

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ROUGHLY speaking, there are two schools of thought in animal psychology. Those who adhere to the first, the mechanistic school, are satisfied with the statement of animal behaviour provided by proper scientific study. According to this school, it is the task of the subject to collect accurate data about the ways of animals, to classify them according to scientific methods, and to proceed to the formulation of general concepts—or what are usually styled "mechanisms"—of behaviour. This school is essentially an experimental school, for scientific hypotheses in biology, whether they be merely an economical way of stating a series of facts in the form usually regarded as "explanation," or whether they attain to the value of "predictive hypotheses," cannot, with rare exceptions, be constructed except by the use of carefully controlled experiment. Predictive hypotheses necessarily have to assume a novel quality in relation to the content of the original premises from which they are derived, for scientific analysis involves the statement of one thing in terms of other things.

It is the good fortune of mechanistic students of animal behaviour, if they wish, both to be unlimited in the number of problems they investigate, and to be unhampered in their scientific progress by philosophical misgivings. In this way they have an advantage over the second, or vitalistic, school, whose adherents, being dissatisfied with "ordinary" scientific method, repeatedly raise doubts as to the philosophical validity of scientific investigations. This being so, the main question under discussion—*viz.* the methods of animal psychology—can best be considered by examining the opinion that the mechanistic point of view provides an unsatisfactory science of behaviour.

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[1] From a paper contributed to the discussion on "the Interpretation of Animal Behaviour," Sections D and H, British Association for the Advancement of Science, Aberdeen Meeting, September 1934.

As a rule, the primary objection raised against the mechanistic approach is that it fails to take heed of the distinction between living and non-living, and that consequently the method of biology misguidedly becomes that of the physical sciences. Our president, Dr. E. S. Russell, attributes this supposed error to the influence of Cartesian philosophy.[1]

In considering this question, it is essential to distinguish between two issues : the first that Cartesian philosophy implies that animals are simply mechanical contrivances : the second that the method of science used in investigating living things follows the same logic as the method used in the physical sciences. So far as I can see, there is no reason for regarding these two issues as necessarily interdependent. In view of what has been said to the contrary, it is necessary to stress the point that the observational and experimental methods of biological science would have been the same whether or not the Cartesian doctrine of mind and matter had ever been stated. Before this matter is considered further, it is of interest to examine the view that a definable qualitative difference between animals and inanimate matter is sufficiently marked to necessitate a different scientific method of investigation for each.

We are told that the distinctive characteristic of the behaviour of living, as opposed to non-living things, is that it exhibits purpose or directive activity; that there is always an *end* (in the teleological sense) to the individualised activity of an organism, and that because of this inherent purpose by which it is characterised, the behaviour of living things requires a type of investigation different from that employed in the investigation of the activity of a non-living system. If, however, we consider it closely, this appeal to teleology appears to argue much less *against* than *for* the investigation of the behaviour of living organisms by the methods of physical science.

An immediate objection to emphasising the teleological character of the overt behaviour of living systems is the fact that the most obviously purposive systems which we know are man-made machines. It is possible to visualise a mechanical system—for a crude example, a radio-directed model of a ship—whose overt behaviour would provide analogies for almost all the usually defined peculiar characters of the behaviour of living bodies—a system with individualised activity, with a purpose, with one " life-cycle " if the term may be allowed—with " needs," responsive to its environment, and with protective devices. Bertrand Russell,[2] in considering where the difference lies between the perception of a living system and the sensitivity which characterises many non-living systems (*e.g.* photographic plates), has suggested that it is in the fact that "living bodies are subject to the law of association or of the 'conditioned reflex.'" Even this, however, is no longer true. Electrical apparatus has been constructed,[1] whose behaviour provides an amazing parallel to the various phenomena of the conditioned reflex. On the whole, therefore, the appeal for a new methodology in the study of the behaviour of living organisms on the score of the teleological nature of this behaviour seems rather out of place. As Henderson has wisely written,[2] "though we can readily separate the mechanical from the teleological in nature, we can on no account separate the teleological from the mechanical, if we are to think about it scientifically."

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[1] *The Advancement of Science*, 1934, Brit. Assoc., London.

[2] *An Outline of Philosophy*, 1927, Allen & Unwin, London.

It is significant that students who can face their prejudices towards teleological explanation in a relatively detached manner hold the view that scientific method and science itself have no connection with teleology There could be no better exposition than that of Henderson [3] of the part teleology can play in a description of the non-living world of physics and chemistry. Wherever we seek, there is purpose, wherever we look, there is adaptation. Yet Henderson gives as his opinion that "scientific conclusions are independent of the philosophical problem of teleology." And, as we all know, teleology is never resorted to in the scientific study of the physical world. "Science knows nothing of first causes," [4] and science has little, if any, business with such teleological concepts as purpose.

Such being the case, it may be asked how the scientific investigation of animal behaviour is to proceed. This brings me back to my point that the deterministic method, which is the true scientific method, bears no necessary relation to Cartesian materialism. So far as the mechanistic investigator of behaviour is concerned, the organism is a going concern Its phenomena are investigated and classified, and laws are then devised to describe, as simply as possible, their arrangements and relationships. These laws do not rule the animal's activities; the animal still goes on, as it would have done had no laws been made, pursuing the various pastimes that make up its life-cycle.

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[1] Hull, C. L., and Baernstein, H. D., 1929, "A Mechanical Parallel to the Conditioned Reflex" *Science*, **70**, 14.

[2] *The Order of Nature*, 1917, Harvard University Press.

[3] Op. cit.

[4] Pearson, K., *The Grammar of Science*, 1900, Black, London.

The mechanistic method proceeds by the creation of scientific laws, and mechanism need not be taken to consist in more than these laws. In saying this, I feel no guilt in the fact that by completely identifying it with determinism I am perhaps limiting the meaning which can be read into mechanism. The old notion of cause, in the sense of anthropomorphic pushes and pulls, as has been argued by Bertrand Russell,[1] has no real place in science. It may still aid in the formulation of preliminary scientific laws, but philosophical validity belongs only to the view that causality subsists in " the fact that events at different times are connected by laws." Pearson has defined cause in science as " a stage in a routine of experience," and in exemplification of this view his reference to Kirchoff's definition of mechanics may well be recalled: " Mechanics is the science of motion; we define as its object the complete *description* in the *simplest* possible manner of such motions as occur in nature." In this definition, so Pearson holds, lies " the only consistent view of mechanism and the true conception of scientific law."[2] His opinion seems manifestly sound. Only in the sense of cause just defined need we believe that every phenomenon is the result of mechanical causation.

It is difficult to escape the conviction that those who assail mechanism to-day do so in misconception of its true scientific significance. There need be nothing in mechanism which is fatal to the study of living things. And before this point is dismissed, it must be remembered that mechanism is not synonymous with materialism. Provided he does not tangle his science with his philosophy, the scientist, if he wants, can be a gross materialist, or alternatively believe that all phenomena are products of his own mind. Consequently, we may be permitted to wonder if the question asked by Dr. E. S. Russell in his presidential address—namely, what chance there is for a " real science " of animal behaviour to develop if Cartesian dualism is accepted—is relevant. The Cartesian dualism of mind and matter may be an entirely fallacious philosophy, mechanism nevertheless remains the method of scientific thought—because it is the method that works.

In his address, Dr. Russell put the view that " modern physics has completely transformed the old conception of matter, and has little use for the notion of material determinism." This, if true, strikes of course at the heart of what I have just called " the method of scientific thought." I doubt, however, whether Dr. Russell meant his words to be taken literally. It is far from certain that modern physics has abandoned determinism. An appeal to authority is always useful in debate, and I may therefore point out that Einstein, Planck, and Rutherford, to mention a few theoretical and experimental physicists, have certainly not denied the philosophical implications of determinism, any more than they have turned their backs on it in the laboratory.[1] It is also relevant to quote *Nature* on this question. In a recent editorial appears the statement: "Newtonian mechanics, so far from being in ruins, is more firmly established than ever as the form taken in ordinary circumstances by the mechanics of relativity." [2]

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[1] Bertrand Russell, *op. cit.*

[2] Pearson, *op. cit.*

I have indicated, I hope successfully, the reasons for my belief that there is no definable basis for the view that the peculiarities of living systems require a type of scientific investigation different from that demanded by non-living matter, and I have also tried to show that the mechanistic method of investigation may be regarded as something valid *per se.* This brings me to another matter, namely the question of the method's reputed shortcomings.

Dr. Bierens de Haan has just affirmed that the results obtained by strictly objective analysis of behaviour are inadequate because, to quote his own words, such analysis "gives us only a number of parts, while the bond that links them is lacking." This criticism is made, not only of the analysis of living behaviour, but also of the analysis of non-living systems, and it is closely related to the basic idea of the philosophies of holism and emergent evolution, the essential thesis of which, as I understand them, is the emergence of novel and unpredictable properties at each new and higher level of the organisation of the constituent parts of things.

It seems to me, as it has seemed to others before, that the objection suffers from two striking weaknesses. In the first place, the criticism presupposes that experimental science has already uncovered all the reflexes, tropisms, and other basic mechanisms of behaviour that it can ever possibly unfold, and that the books when balanced show a sad discrepancy between the sum total of the separate items, such as reflexes, that have come out of the investigation, and the primary behaviour material that was put into the investigation. This is a sad slight on an experimental science that has hardly scratched the surface in its task of exposing mechanisms of behaviour, and the fruits of whose investigations become richer and more numerous every day. If present advances are any indication of what is to be, what we now know in the field of the conditioned reflex, or in that of perception, or in that of cortical mechanisms of behaviour, is but a very small part of the knowledge that we shall presently have. We may well doubt whether those who make this criticism would regard explanations in terms of reflex theory or in terms of any other mechanistic concept as really inadequate to interpret the actual situations to which the concepts primarily relate. The mechanist never claims more for his views, as Hogben[1] has written, than "that as far as we can see at present his way of dealing with things leads to the most complete unanimity which it is possible to attain."

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[1] See Sir Herbert Samuel, "Cause, Effect and Professor Eddington," *Nineteenth Century*, 1933, CXIII, 469.

[2] *Nature*, 134, 340, Sept. 8, 1934.

This is but one weakness of the attack which uses as its weapon the argument that parts of behaviour do not make up the whole of behaviour. There is another and perhaps more significant one; the argument is based upon a misconception of the usual direction and intention of scientific investigation. The nature of this misconception has been eloquently expounded by Professor Levy,[2] but the argument cannot be harmed by repetition, especially in view of what has been said both by Dr. Russell and by Dr. Bierens de Haan.

In our investigations of behaviour we have to begin with the whole animal as it is given to us by common sense, and from its many activities we isolate, mentally, if you like, at first, parts which we wish to examine more closely. We are careful, of course, that the isolated parts, from the point of view of the investigation of their nature, are neutral to the wider environment to which they belong. If they were not neutral they could not be investigated as unique and separable systems. So far as the original, unique, whole behaviour is concerned, this method, in Professor Levy's words, simply discloses the unique combination of isolated systems that it possesses. Every isolated system, every isolate we define, is "unique in its mode of functioning, otherwise it would not be isolated"; every isolate is also a "unique *combination* of its sub-isolates"—of which one, in the field of behaviour and as the analysis proceeds, may turn out to be—let us say—a reflex. As we become concerned with the wider or narrower issues in behaviour, we widen or restrict the sphere of isolation so as to include just enough of the environment to make our system neutral for the purpose of our particular observations. The process of isolation may be continued until the limits of scientific measurement are reached.

Reflexes and tropisms are some of the theoretical isolates defined in the study of animal behaviour. They are not, however, isolated entities. There are no disembodied reflexes, any more than there are atoms and molecules other than those which are "the intellectual conceptions by aid of which physicists classify phenomena."[1] The larger isolate, the animal's total behaviour, is primary to the smaller isolate, the reflex, and the smaller isolate has meaning only in its unique relation to its parent isolate. It is the latter which is the prime reality, and thus from the scientific point of view it is by the whole that the parts are primarily described —not the parts which have to describe the whole. It is therefore small wonder that the gross phenomena of experience cannot be reconstructed by the recombination of hypostasised scientific isolates.

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[1] *The Nature of Living Matter*, 1930, Kegan Paul, London.

[2] *The Universe of Science*, 1932, Watts, London.

A satisfactory "explanation" of a form of behaviour would be a full statement of the pattern of its usefully definable sub-isolates in their combination of interactions within it. If a statement in terms of reflexes were to prove an unsatisfactory description for any particular behaviour pattern, it would not mean that description in terms of other behavioural isolates would also necessarily provide an unsatisfactory explanation. It would not spell the doom of the analytical method in the interpretation of behaviour. It would only mean that reflexes happened to be inadequate isolates of the particular larger system concerned. Having said this it is necessary for me to add, lest I have conveyed a false impression, that no one can doubt that the reflex is an adequate concept for the description of some types of behaviour. To say that a description of behaviour in terms of such a concept leaves out of account the bond which makes reflexes part of the totality of behaviour is to leave unconsidered the fact that the concept received definition by the investigation of a part of behaviour which was capable of isolation, and had meaning, only because it was part of the organised whole of behaviour.

So far my argument has been chiefly aimed at defending the deterministic method of science against the attacks that have been directed upon it at this meeting. It is now time to examine the measures that have been suggested to replace this method. Essentially they fall into two groups: the first relating to the proposal to use the concept of organisation; the second to amplify objective description with terms borrowed from introspective psychology.

We have been advised to follow the "organismal" approach. We have been told that the wholeness of the animal is primary and fundamental to the differentiation of its behaviour mechanisms. It has been pointed out "that early behaviour does *not* arise through the addition and combination of originally separate reflexes,"[1] and we have heard that wholeness runs not only through the animal's developmental life, but also through its perceptual life and through its processes of learning.

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[1] Pearson, *op. cit.*

No one could possibly take exception to these statements. At the same time, we may be permitted to wonder whether the doctrine of "wholeness" matters much from the scientific point of view, at any rate in so far as a change of method is concerned. The co-ordination of the activities of animals is fundamentally given in the fact of their very existence. If animals were not co-ordinated wholes they could not be studied for what they are. If their parts and activities were differently co-ordinated, they would be different animals. In short, the wholeness of animals is the point at which we begin their scientific investigation.

If I correctly understand the terms as they have been used here, organisation is closely linked with adaptation, and purpose emerges as a characteristic of an animal's adapted activities within its field of experience. On their face value, I cannot see how such trite ideas add an iota to the technique of scientific enquiry, nor can I see how they could possibly change it. Indeed that is the one thing that has not been told us. We have been advised to abandon our analytical methods, and to begin as organismic students of behaviour—but, strange though it be, from the few indications we have been given on how to proceed, it would seem that having publicly declared ourselves to be organismic students we must once again start analysing. It is difficult to believe that the invocation of teleological ideas brings us a single step nearer to our specific scientific problem of the interpretation of organised patterns of behaviour. Assuming of course that they do not propose to content themselves with stating general teleological principles and leaving it at that, those who, in supposed opposition to the mechanistic school, declare themselves to be organismic students are hiding from themselves the fact that any moves they might make to investigate animal behaviour will have to be along paths beaten by the mechanists.

I think I am right in believing that the opponents of mechanistic analysis have not used the concept of organisation at this meeting to mean more than a mystical something given in the initial premise of organised behaviour. I can find no indication of any other meaning in what I have heard. At the same time, it would be unfair to leave the matter at this point, for a scientific problem definitely lies in the question of the organising factors themselves. Like other scientific questions, it cannot, however, be profitably examined except by the deterministic method of science. The search for "organising principles" by means of this method has yielded such fruits as the theory of the integrative action of the nervous system and the theory of endocrine regulation. If organising factors exist in overt behaviour, they will be uncovered in the same way—and so far as we now know, in no other way.

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[1] E. S. Russell, *The Behaviour of Animals*, 1934, Arnold, London.

It is by no means uncommon for those opposed to mechanistic abstractions such as the conditioned reflex, to regard the concept of organisation within the perceptual field—one of the central issues of "Gestalt psychology"—as something which undermines a deterministic science of behaviour. This, for example, is the opinion of our President, who has summarised his view of the matter by telling us that "the principle of the whole is as valid for the perceptual field as it is for executive behaviour." The same debating use has been made of Lashley's experiments on the relation of the cortex to learning.[1] The argument is one which I fail to understand.

It is a fact, amply established by experiment, that the responses of animals in some situations are responses to complexes or patterns in the perceptual field, and not to unrelated single specific things within the field. It is also established that animals sometimes react to the relations between things and not to the things themselves. These findings, like those of Lashley, seem to me to fit perfectly well into a mechanistic view of behaviour, even though they may argue against the possibility of an exhaustive explanation in terms of point-to-point relations between single stimuli and single receptors. Some "Gestalt" and organismic psychologists attempt to persuade us, however, that the facts mean something more than appears at first sight. They see in them, as it were, a wider pattern formed by an intimate linkage of the perceiving object and the pattern it perceives and to which it reacts, and they are startled by the vision as if by a new discovery. But is it not in fact the very situation which they originally set out to interpret? Is it not the problem as it occurs in nature? Köhler[2] would have us believe that physical science takes a disorderly nature, "a world of confused experience," and out of it fashions "a world of clear and hard reality." I find it difficult to imagine this disorderly naïve universe of perception. Moreover, as a mechanist, I find it equally difficult to be impressed with the obvious fact that order and pattern do exist in the world we perceive. If they did not so exist, there could be no repetition in thought, and we should be unable to construct any organised system of scientific data. In our work we do not synthesise *by chance*, out of a completely heterogeneous perceptual world, particular patterns for examination. If in any conceivable way we did so, it is remarkable that so many of us synthesise the same elements into the same patterns in such a way that we can get together and discuss them as we are doing this morning. Because we are all fundamentally made in the same way, there is nothing more inevitable than the fact that a common type of organisation characterises the perceptual world each of us as it were creates.

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[1] *Brain Mechanisms and Intelligence*, 1929, University of Chicago Press.

[2] *Gestalt Psychology*, 1930, Bell, London.

Every living organism presumably forms a whole with those parts of the external world which are significant to it. In so far as the small worlds of different organisms do not overlap, they are all presumably smaller parts of a wider and more embracing world. Each organism exists only within its own organised perceptual world, and all its reactions in it are obviously organised reactions to organised parts of it. To emphasise this fact as though it were revolutionary seems to me absurd. To do so merely points to the sad fact that once again we have been blinded by general principles, and that once again we have come back to the starting point in our interpretation of behaviour, to the point where deterministic analysis begins.

Of the suggestions that have been made for the modification of the present mechanistic methods in animal psychology, only one is of any concrete significance. Both Dr. Bierens de Haan and Dr. Russell wish us, if necessary, to explain an animal's behaviour in terms of its presumed subjective experiences.

I suppose no one could seriously object to the thesis that animals have subjective lives, for the question is one that is impervious to scientific statement and scientific proof. If there are students of animal behaviour who feel that a science relating to subjective experience can be established in non-ambiguous terms, it is up to them to prove their point. Introspective human psychology has had considerable difficulty from this point of view, and animal psychology can scarcely hope to have less. It cannot even have the direct aid of the animals of whose subjective experiences it wishes to learn, for unlike human beings, animals cannot tell us what they feel, and why they think they do the things they do. I should think that the best we could expect from describing animal behaviour in the terminology of introspective psychology is the enhancement of our sympathetic attitude toward animals, with a consequent increase in our intuitive understanding of their ways.

But these things do not necessarily have anything to do with science, even though they might help to discover the kind of question regarding animal behaviour which can profitably be asked from the scientific point of view. In spite of this possible advantage, my opinion is that the re-introduction of the terminology of introspective psychology would be a disastrous step, calculated only to return the subject to its discredited anthropomorphic and anecdotal phase.

The study of animal behaviour should be allowed to proceed, and animal behaviour should continue to be interpreted, by the one method that gives certain knowledge—namely, by the application of well-established scientific method to objectively definable data, data which can be stated without the fear of ambiguity. What real advances animal psychology has made in the past have only been made by this method, which is in no way restricted in its mode of operation. The method can operate in the usual inductive way to elucidate a limitless field of phenomena of animal behaviour. And it can also be used deductively to great effect—as it has been, for example, by Professor C. L. Hull[1]—not in mystical fashion, but by employing the general findings of experimental psychology as a basis for postulates which may serve for the purpose of further investigation.

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[1] For example, "The Goal Gradient Hypothesis and Maze Learning," *Psychol. rev.*, 1932, 39, p. 25.

DANIEL RUTHERFORD AND THE DISCOVERY OF NITROGEN

With Translations from his "Dissertatio Inauguralis de Ære Fixo Dicto, aut Mephitico" (1772)

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> "Again, all things must have air in their body, since they are of porous structure and air surrounds and adjoins all things."
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> Lucretius, *De Rerum Natura*, trans. Rouse.

Daniel Rutherford (1749–1819) was the son of John Rutherford, Professor of Medicine in the University of Edinburgh from 1726 to 1765, by his second wife, Anne Mackay, a member of the Reay family. He studied at Edinburgh under Cullen and Black, receiving the degree of M.D. in 1772 for his *Dissertatio Inauguralis de Ære Fixo Dicto, aut Mephitico*, in which he announced among other matters his discovery of "noxious air," the gas subsequently known as nitrogen.

Rutherford did not include in his Dissertation any details of his experiments, but gave only their general results. More than a half of his text was devoted to an account of "Mephitic Air," which he defined thus: "By Mephitic Air, which others call Fixed Air, I understand with the celebrated Professor Black that particular kind of air that causes the death of animals, extinguishes fire and flame, and is strongly attracted by quicklime and alkalis." This is evidently carbon dioxide, Black's "Fixed Air" (see *Alembic Club Reprints*, No. 1); and there is no evidence that Black ever called it by any other name. Having discussed the "mephitic air" that was evolved from calcareous bodies, Rutherford proceeded to study the other kinds of "air" that extinguished fire and life. First among these was the "air" that had contracted its noxiousness by animal respiration. Thus, when an animal was confined in a limited amount of air, it presently expired; and the air was reduced in volume and rendered incapable of supporting fire or life. But removal of the "mephitic air," *i.e.* carbon dioxode, from this polluted air by means of caustic ley did not restore it to its former wholesomeness; for, after this separation, it still proved as noxious as it was before. Exactly the same result occurred in the case of air that had supported the combustion of coals; caustic ley removed the "mephitic air" from it, but it was still noxious. "Indeed," argued Rutherford, "it appears by experiments that this change in the air, [*i.e.* its becoming noxious] is the only one that can be ascribed to fire." This conclusion he based on the observation that no "mephitic air" was produced in the combustion of substances consisting of phlogiston and a fixed and simple base, *e.g.* in the combustion of sulphur and phosphorus, the residual air did not give a precipitate with lime-water, except that in the case of phosphorus lime-water sometimes yielded a turbidity, "but a very slight one, not to be attributed to mephitic air but rather to that acid which is contained in phosphorus and which, as experiments have taught us, markedly possesses this particular property."

It was therefore clear that "mephitic air" was not produced by a change of common air brought about by the action of fire; it was rather produced or ejected from the substance that was resolved by the fire. The other kind of air that was fatal to life and fire, air that Rutherford now began to speak of as "noxious air," appeared therefore to be "compounded of atmospheric air united and, as it were, saturated with phlogiston." This theory, Rutherford argued, was confirmed by the observation that this same "noxious air" was obtained when metals were calcined in common air. "Mephitic air" itself appeared to be "composed of phlogistic matter and atmospheric air; for it is never produced except from bodies that abound in material apt for combustion. . . . *From phlogistic matter* I say, because, as noted above, pure phlogiston combined with common air seems to constitute another kind of air," namely, the kind he had called "noxious air."

As for the production of "mephitic air" in respiration, Rutherford pointed to Black's suggestion that this air was possibly not produced in the lungs, *i.e.* not produced by a change of common air, but perhaps produced in the body by foods and ejected from the blood through the lungs.

Rutherford's Dissertation bears the date, September 1772. Six months earlier, Priestley, in papers read before the Royal Society of London on March 5, 12, 19 and 26, 1772, had described experiments resembling those of Rutherford (*Phil. Trans.*, 1772, **62,** 162–5, 181–3, 207–9, 225–33). Indeed, Priestley had shown that a mixture of brimstone and moist iron-filings diminished common air between $\frac{1}{5}$ and $\frac{1}{4}$, and that the residual air did not yield a turbidity with lime-water, was "rather lighter than common air" and "exceedingly noxious to animals"; that common air was similarly diminished by $\frac{1}{5}$ "by the fumes of burning charcoal," the residual air, after the removal of "fixed air" by means of lime-water, extinguishing flame, being noxious to animals, showing no action with "nitrous air" (nitric oxide) and being incapable of further diminution "by any other cause of the diminution of air that I am acquainted with"; and that the same result was obtained when lead and tin were calcined in air, in which case however there was never any turbidity produced by lime-water, and also when paint made from white-lead and oil was exposed to air. The diminution of the air in the experiments with charcoal was ascribed by Priestley to "its having more than its usual quantity of phlogiston"; and this led him to the experiments with metals since these were "generally supposed to consist of nothing but a metallic earth united to phlogiston," whereas, "in other cases there was not only a cause of diminution, but causes of addition also, either of fixed or inflammable air, or some other permanently elastic matter . . . the effect of the calcination of metals being simply the escape of phlogiston, the cause of diminution was alone and uncontrolled." "In the experiments with metals," he wrote, "the diminution of air seems to be the consequence of nothing but a saturation with phlogiston; and in all the other cases of the diminution of air, I do not see but that it may be effected by the same means. . . . Again, the iron, by its fermentation with brimstone and water, is evidently reduced to a calx, so that phlogiston must have escaped from it. Phlogiston also must evidently be set loose by the ignition of charcoal, and is not improbably the matter which flies off from paint, composed of white lead and oil. Lastly, since spirit of nitre is known to have a very remarkable affinity with phlogiston, it is far from being improbable that nitrous air [nitric oxide] may also produce the same effect by the same means."

Priestley did not, however, come to any more precise conclusion than this in 1772. Later, in 1774, he explained that "in the first publication of my papers, I confined myself chiefly to the narration of the new *facts* which I had discovered, barely mentioning any *hypotheses* that occurred to me, and never seeming to lay much stress upon them. The reason why I was so much upon my guard in this respect was, lest, in consequence of attaching myself to any hypothesis too soon, the success of my future inquiries might be obstructed. But subsequent experiments having thrown great light upon the preceding ones, and having confirmed the few conjectures I then advanced, I may now venture to speak of my hypothe with a little less diffidence. . . . In a great variety of cases I have observed that there is a remarkable *diminution* of common, or respirable air, in proportion to which it is always rendered unfit for respiration, indisposed to effervesce with nitrous air [nitric oxide], and incapable of farther diminution from any other cause. The circumstances which produce this effect I had then observed to be the burning of candles, the respiration of animals, the putrefaction of vegetable or animal substances, the effervescence of iron filings and brimstone, the calcination of metals, the fumes of charcoal, the effluvia of paint made of white-lead and oil, and a mixture of nitrous air. All these processes, I observed, agree in this one circumstance, and I believe in no other, that the principle which the chymists call *phlogiston* is set loose , and therefore I concluded that the diminution of the air was, in some way or other, the consequence of the air becoming overcharged with phlogiston " (*Experiments and Observations on Different Kinds of Air*, London, 1774, I, 177–8). In a footnote he added · " On this account, if it was thought convenient to introduce a new term (or rather make a new application of a term already in use among chymists) it might not be amiss to call air that has been diminished, and made noxious by any of the processes above mentioned, or others similar to them, by the common appellation of *phlogisticated air* " (*ibid.*, p. 178). This was the first appearance of the term " phlogisticated air " . it was coined by Priestley in 1774. Meanwhile, Daniel Rutherford had named the new substance " noxious air " and had advanced in September, 1772, an explanation similar to that which Priestley put tentatively before the Royal Society in March 1772, and more definitely in his *Experiments and Observations, etc.*, in 1774. Priestley's experiments were more numerous than those of Rutherford, but his explanation was originally less precise.

It is possible that Rutherford was unacquainted with Priestley's papers of 1772, although he makes one (mistaken) reference to Priestley's work, namely, to his study of the action of growing plants on " mephitic air " (*Phil. Trans.*, 1772, **62**, 193 *et seq.*). Now Thomas Thomson (*History of the Royal Society*, London, 1812, p. 11) states that this volume, No. 62, was published in two parts. When such was the case, it appears that papers read before the Society in March would be included in the second part, which was published after the Society's summer recess ; but it also appears that, contrary to Thomson's statement, this was not the procedure until after Vol. 62 (see copy of resolution by Council, January 28, 1773, printed in *Phil. Trans.*, 1772, **62**, Preface, p. iv). Weld (*History of the Royal Society*, London, 1848, II, 565) makes a statement similar to that of Thomson. Comparison of various copies of Vol. 62 shows that it was published in one part, not in two parts. It thus seems unlikely that Rutherford was aware of Priestley's work before he published his Dissertation. We are, however, informed by Mr. H. W. Robinson, Assistant-Librarian to the Royal Society, that there is evidence that authors obtained and circulated copies of their papers shortly after reading them before the Society and before the publication of the completed volume of the *Philosophical Transactions*. But there are sufficient differences between the two researches to suggest that any knowledge that Rutherford may have had of Priestley's work was slight.

It is however possible, even probable, that Rutherford may have heard only of Priestley's discovery of the restorative action of growing plants on what he (Rutherford) supposed to be "mephitic air" and not of any of the other advances described by Priestley to the Royal Society in March 1772; for on March 30, 1772, Benjamin Bell wrote from London to Cullen, Rutherford's teacher, stating that he had been present at the "last meeting"[1] of the Society and had heard Priestley announce this discovery, and making no reference to any other part of Priestley's paper (J. Thomson, *Life of William Cullen*, Edin. and London, 1832, I, 648–50). The fact that Rutherford's sole reference to Priestley's researches (in the final paragraph[2] of the Dissertation) opened with the phrase "Lately, as I have heard" and that it mentioned only this one of Priestley's numerous observations strongly suggests that Bell's letter was the source of Rutherford's information and that this was all that he knew at that time of his great contemporary's recent labours. Further evidence of this appears below.

Rutherford's work is either ignored or misrepresented by historians of chemistry and the authors of chemical text-books. The only accurate account we have seen is contained in Sir Philip Hartog's article on Rutherford in the *Dictionary of National Biog-*

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[1] The "last meeting" would be that held on March 26, but the *Journal Book* of the Society for 1772 shows that Bell was present as a "stranger" at the meeting held on March 19 and not at that of March 26. No details are given either in the *Journal Book* or in the *Phil. Trans.* of what sections of Priestley's paper were read on the various dates, but it is not unreasonable to suppose that the particular section to which Bell was referring was read on March 19, when he was present, not merely from the contents of his letter but also from its place in Priestley's very long paper. It is interesting to find that an unidentifiable "Mr. Rutherford" was present as a "stranger" at the meeting held on April 2, 1772, the meeting following that at which Priestley completed the reading of his paper.

[2] Translated below.

raphy.[1] Most writers state that Rutherford called the new air that he had discovered in 1772 " mephitic air." Presumably they have misunderstood the title of his Dissertation, although this does not account for the curious statement made by one historian that Rutherford " adhered to Mayow's name, *nitrous air.*" Some clear assertion is necessary on this point. Rutherford's *aer mephiticus* or " mephitic air " was " fixed air," our carbon dioxide ; and he called the new air *aer malignus*, which we have here translated as " noxious air." In 1774, as we have already noted, Priestley named it " phlogisticated air " ; and in 1777 Scheele called it " vitiated air " (*Collected Papers of Carl Wilhelm Scheele*, trans. L. Dobbin, London, 1931, p. 106). The French chemists in revising the nomenclature of chemistry in 1787 named it " azote " (*Méthode de Nomenclature Chimique*, by de Morveau, Lavoisier, Berthollet and de Fourcroy, Paris, 1787, p. 36 ; English trans. by J. St. John, where it appears as " azot," *Method of Chymical Nomenclature*, London, 1788, p. 26). Chaptal gave it its present name, nitrogen, in 1790, deriving it " from the characteristic and exclusive property of this gas, which forms the radical of the nitric acid " (*Elements of Chemistry*, trans. W. Nicholson, London, 1791, I, xxxiv–xxxvi). An earlier suggestion that it be called " alkaligen " on the grounds that it entered into the composition of the volatile alkali or ammonia was not well received.

Rutherford did not extend his chemical researches. From 1772 to 1775 he travelled abroad , and from 1775 to 1786 he practised medicine in Edinburgh. In 1786 he succeeded John Hope in the chair of Botany at Edinburgh. He died in 1819. Copies of Rutherford's Dissertation are exceedingly rare. The author's thanks are due to the Royal Society of Medicine, Wimpole Street, London, for the loan of their copy for the preparation of this paper. The title-page bears a quotation from the *De Rerum Natura* of Lucretius ; a translation is given below the title of the present publication.

Finally, it is to be noted that a French version of Rutherford's Dissertation appeared in Rozier's Journal, *Observations sur la Physique*, in the number for June 1773 (*Observations, etc.*, 1773, **1**, 450), and that extensive corrections to this were made in the next number, that of July 1773, the Editor explaining that the first translation had been made from a manuscript sent from London, that the author's text was now available and that the translator was not familiar with Latin or else unpractised in the French idiom. But even this corrected version contains errors, omissions and additions and is not a fair rendering of the original, which is not to be wondered at, having regard to the translator's curious qualifications. However, it is more important to note that a translation of Priestley's paper from the *Philosophical Transactions* of 1772, which we have quoted above, appeared in the numbers of this periodical for April and May 1773 (*Observations, etc.*, 1773, **1**, 292 and 394), and that the papers both of Priestley and of Rutherford were read, with what result we do not suggest here, by one who, first among his countrymen, was soon to begin to take an intense interest in the study of gases with a view to effecting a revolution in chemical thought,[1] namely, by Lavoisier,[2] himself a contributor to this Journal.

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[1] Professor J. R. Partington, M.B.E., has kindly pointed out to us that relevant paragraphs of Rutherford's Dissertation were quoted by Thomas Thomson in the chapter on " azot " in his *System of Chemistry* (Edin., 1802, I, 65–6 and 69–70).

There is other interesting evidence that Lavoisier was familiar with Rutherford's work ; for in his famous *Mémoire sur la nature du Principe qui se combine avec les Métaux pendant leur calcination, & qui en augmente le poids,* as originally read to the Academy on April 26, 1775, but not as published in 1778 (*Mém. Acad. R. Sci.*, 1775, p. 520) [3], he discussed the view that "fixed air is merely a combination of common air and phlogiston" and said "This is Dr. Priestley's opinion." This was certainly not Priestley's opinion : it was Rutherford's. Priestley's view was that fixed air was an elementary substance, and common air a compound. Lavoisier had evidently become acquainted with this theory through the medium of the French version of Rutherford's Dissertation that appeared in the *Observations* and to which we have already referred. Now, it is well known that Priestley suffered much misrepresentation at the hands of translators, against the versions of several of whom he took trouble to protest, inserting detailed corrections of their errors in Vol II of his *Experiments and Observations on Different Kinds of Air* (London, 1775, pp 304–23). Turning therefore to this volume (p. 313), we find that the statement by Lavoisier to which we have just alluded appeared to Priestley to be " the most unaccountable mistake concerning any of my opinions relating to air," Priestley adding that he had been made aware of it through the publication of Lavoisier's memoir in the *Observations* for May 1775.[1] In protesting, Priestley pointed out that this was Rutherford's theory, referring his readers to the latter's *Dissertatio de aere fixo* (p. 25), and added : " Every thing that is English is not mine. I have mistakes enow of my own to answer for, and I cannot conceive how any thing that I have ever advanced on the subject should have been construed to bear that meaning " (*op. cit.*, p. 314).

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[1] Cf. A. N. Meldrum, *The Eighteenth Century Revolution in Science—The First Phase*, Calcutta, 1929, pp. 7–13.

[2] Lavoisier (*Opuscules Physiques et Chymiques*, Paris, 1774, p. 183, footnote ; English trans. by T. Henry, *Essays Physical and Chemical*, London, 1776, p. 217, footnote) mentioned Rutherford's "very well written thesis," but it is clear that at this date (Jan. 1774) he regarded it as a mere summary of the work of Black, Cavendish and Lane and that Rutherford's recognition of "noxious air" had escaped his notice (cf. Meldrum, *op. cit.*, p. 20).

[3] The memoir as originally read to the Academy on April 26, 1775, was published in the *Observations, etc.* (1775, **5**, 429), but when the volume of the *Mémoires* of the Academy for 1775 was published in 1778 the reference to Priestley was omitted for reasons that appear above.

Moreover, as Priestley (*ibid.*, p 316) stated incidentally that " Dr. Rutherford, when he published his *Dissertation on fixed air*, had only heard of my experiments " and " supposes . . . I had restored *fixed air* to a fitness for respiration by vegetation, whereas it was *air injured by respiration or putrefaction*," this provides further evidence that Rutherford's acquaintance with Priestley's work was limited to what was conveyed in Bell's letter to Cullen mentioned above ; for Bell wrote that " mephitic air being confined with growing plants, is . . . thereby entirely changed, and again rendered pure " (*op. cit.*, p. 650), and to Rutherford " mephitic air " was " fixed air," whereas Bell was applying the term to " air . . . spoiled, or rendered unfit for the purposes of life, by one or more animals frequently breathing it in a confined place " (*ibid.*, p. 649). This provides strong confirmatory evidence of our suggestion as to the source of Rutherford's very limited knowledge of Priestley's work and confirms the view that the researches that led to his discovery of the gas now known as nitrogen were carried out in entire independence of Priestley's experiments.

Translations of those sections of the Dissertation that refer to the discovery of nitrogen read as follows, but brief parenthetical reference to the omitted sections is necessary for proper appreciation of Rutherford's approach to the problem with which he was dealing.

The Dissertation opened with two short paragraphs on atmospheric air, the necessity of such air for respiration and combustion and the knowledge of its weight, density and elasticity gained in the previous century (p. 1). Then followed a long account of " Mephitic Air," a well-written summary of Black's classic investigations with some mention of the work of Cavendish (*Phil. Trans.*, 1766, **56**, 141 and 1767, **57**, 92) and Lane (*ibid.*, 1769, **59**, 216) (pp. 2–15). Rutherford proceeded : " Having now discussed the mephitic air that is regenerated from calcareous bodies, I shall add a few words about such air of this kind as arises from other sources; and firstly of that which contracts its noxiousness through animal respiration. It seems indeed remarkable that, although no animals can live without the benefit of atmospheric air, yet by their vital action the air becomes so deadly that it takes life away very quickly, almost before all other poisons. For the most vigorous animals, when plunged into such air, perish almost instantly. Indeed, if an animal is confined in a glass vessel away from all communication with the external air, it begins to be affected by an uneasiness and unrest and, sooner or later, according to the capacity of the vessel and the volume of the air in which it breathes, it collapses as if stricken by apoplexy. Meanwhile the air in which the animal is confined, although it appears at first to be expanded and rarefied a little by the heat of the animal, yet in a short time begins to lose something of its elasticity until, when the animal has died, it gradually returns to its former dimensions, and is then reduced to a smaller volume and found to be in some degree mephitic. Thus air, in which a mouse had expired, lost about $\frac{1}{16}$ of its volume; of this about $\frac{1}{11}$ was absorbed by alkali; the flame of a wax-candle immersed in it was immediately extinguished, but the wick continued to glow a little longer.

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[1] *Observations, etc.*, 1775, **5**, 433.

"It certainly seems probable that different animals, breathing in a definite quantity of air, render different proportions of it noxious; indeed that the same animal can at different times corrupt the air with that lethal quality in shorter or longer intervals; since experiment shows particularly that the volume of air, in which different animals are confined, is diminished at one time more and at another time less, at one time indeed by $\frac{1}{11}$ part of the whole and at another by not more than $\frac{1}{30}$. Further, it is clear from experiments that, among animals of the same kind, some can endure noxious air longer than others can; for if two mice were confined in the same air, one generally lived longer than the other; and it is indeed probable that the same thing can happen to the same animal in a different time. It is therefore difficult to determine the proportion of mephitic air that makes common air unfit for respiration; and perhaps the same thing would happen if it contained $\frac{1}{6}$ or $\frac{1}{8}$ part of mephitic air.

"But pure and wholesome air not only becomes partly mephitic by animal respiration, but also suffers thereby another remarkable change in its nature. For, after all the mephitic air has been separated and removed from it by means of caustic ley, nevertheless what remains does not thereby become in any way more wholesome; for, although it gives no precipitate with lime-water, it extinguishes both flame and life no less than before. Indeed, it is doubtful whether mephitic air is really produced in the lungs or whether, as appears probable, it is ejected from the blood through the lungs as something noxious, perhaps already produced in the body by foods.

"It is observed too that animals, the more warm-blooded they are, the more they need a perfect and constant respiration and the more rapidly they pollute the air with a noxious quality; may we not therefore be allowed to suspect that the heat of animals and this change in the air arise from the same cause?[1]

"Just as the life of animals depends on the free enjoyment of air, so also is air wholly necessary for the maintenance of flame and fire. But no less by fire than by respiration is it changed into a nature unsuitable for and opposed to both uses. And, since the effects of these are entirely similar, what I have previously said about respiration can be repeated about fire. . . .

"Air, which has supported fire, suffers scarcely less loss of its elasticity than if it had served for animal respiration; its volume is indeed diminished by about a twentieth part. . . .

"The quantity of mephitic air produced from a candle enclosed in a glass vessel bears almost the same ratio to the whole mass of included air as if an animal had expired in it.

"Indeed almost the same proportion of mephitic air, mixed with common air, appears to be sufficient to extinguish both fire and life.

"Air, which has been blown through kindled coals by bellows and then purified from all mephitic air, still appears noxious and is entirely similar to that which has been polluted by respiration. Indeed, it appears by experiments that this change in the air is the only one that can be ascribed to fire. For, if any material, which consists of phlogiston and a fixed and simple base, be kindled, the air that is produced from it appears to contain not the least quantity of mephitic air. Thus air, in which sulphur or phosphorus of urine has been burned, although in the highest degree noxious, nevertheless does not at all precipitate lime from water. Sometimes indeed this air, if it has been produced from phosphorus, induces a little cloud in lime-water, but a very slight one, not to be attributed to mephitic air but rather to that acid which is contained in phosphorus and which, as experiments have taught us, markedly possesses this particular property. Whence therefore it is evident that pure air is not changed into mephitic air by the action of fire; but that mephitic air is rather produced by or ejected from the body that is resolved by the fire. In like manner it may even be concluded that noxious air is compounded of atmospheric air united and, as it were, saturated with phlogiston. And this is confirmed by the observation that air, which has served for the calcination of metals and has taken away the phlogiston from them, is plainly of the same nature" (pp. 15–20).

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[1] Cf. Professor Black. (Black considered that animal heat depended on respiration, and that heat was generated in the lungs by the action of air on the principle of inflammability "in a manner little dissimilar to what occurs in actual inflammation." See P. D. Leslie, *A Philosophical Inquiry into the Cause of Animal Heat*, London, 1778, p. 75.)

Rutherford, after referring briefly to the production of mephitic air in decomposition, went on to discuss the opinion of Stephen Hales that air was the bond uniting the ultimate particles of bodies (*Vegetable Staticks*, London, 1727, I, 314), and to refute the views of those (cf. D. Macbride, *Experimental Essays*, London, 1764) who, after Black's discoveries, had argued that mephitic air was "the universal bond of the elements," by showing that this substance was not at all so common a product of decomposition as they supposed and that in many cases the products of chemical decomposition were of "a nature entirely opposite to that of mephitic air" and, indeed, not infrequently inflammable (pp. 21–4). He went on:

"I intended to subjoin some observations concerning the composition of mephitic air and to seek thence a means for the removal of its noxious powers; but, in this matter, I have so far found nothing certain. Some observations suggest that it is composed of phlogistic matter and atmospheric air; for it is never produced except from bodies that abound in material apt for combustion; yet phlogiston seems to be imparted to other bodies and hence avails for the reduction of metallic calces. *From phlogistic matter* I say, because, as noted above, pure phlogiston combined with common air seems to constitute another kind of air. Lately, as I have heard, that very ingenious man, Dr. Joseph Priestley, the celebrated author of the history of electricity, has shown that vegetables growing in mephitic air drive away or, as it were, draw out its noxious powers and restore it to its former wholesomeness; and indeed that mephitic air by the addition of air from putrid flesh partly loses its noxiousness. But experiments which could confirm these things cannot yet be devised with sufficient accuracy" (pp. 24–5).

THE SOLID STATE

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It is perhaps natural that studies of the properties of matter should have been divided into three groups, corresponding to the obvious division of matter into its three states, gaseous, liquid and solid, between which the lines of demarcation have, until recently, been so clearly discernible. This sequence is suitable also, in that it corresponds to the complexity of the various problems involved and forms to-day the order of the relative states of general development of these three branches.

Because of the more tractable nature of the problems which it offered, the gaseous state was the first selected for study. Compared with their own size, the atoms or molecules in a gas are relatively far apart and, except at the instants of collisions arising from Brownian motion, the forces of interaction are negligible and the solutions of problems correspondingly simple.

Next in order of complexity is the liquid state, in which the atoms or molecules are much closer, close enough in fact for the forces of interaction to be of importance, yet sufficiently distant for these forces to be averaged and the field to possess central symmetry.

Pursuing this point of view, the solid state is of course the most complex, as the fields of force of interaction have not only attained greater values but have lost their centro-symmetry ; this is reflected in the acquisition of rigidity by the material. The interatomic forces are now very definitely localised or directed. Fortunately, however, an accompanying simplification was discovered in the regularity which was found to underlie the natural architecture of the solid state.

Only just over twenty years have elapsed since the first experimental demonstration of the diffraction of X-rays by matter, yet our knowledge of the solid state has reached an advanced stage of development, for it is essentially a study of regularity, of repetition. For every substance under any stated conditions, just two main discoveries are necessary; firstly, the geometry of the smallest unit or brick, for there is one only which has to be repeated to build the whole edifice and secondly, the particular mode of repetition used in completing the edifice. The second part of the problem, *viz.* how to arrange exactly similar bricks in regular fashion to fill all space, was a problem of which, fortunately, the earlier crystallographers had disposed, in so far as exhibiting the strictly limited number of possible arrangements; it remained only to select the requisite one for each substance. The mathematical theory of the crystal lattice was ready to hand, therefore, and succeeding years have been crowded with rapid advances. Starting from Bragg's elucidation of inorganic substances like rocksalt and diamond, crystalline structures have been found for hundreds of substances of progressively increasing complexity, until such unpromising materials as, for example, hair, rubber and fibres, are now to be found in the lists of known structures.

The structures deduced from X-ray observations afford a good explanation of many of the properties of the respective substances and therefore must represent their true structures fairly closely. Although these structures are expressed in terms of the infinite mathematical lattice, it is obvious that the correspondence cannot be exact. It is certainly true that no specimen is infinite in size, but is it equally true that even within the limited boundaries of the specimen, the actual lattice is ideal? In other words, there remains for consideration the deviations of real crystals from the ideal lattice, their imperfections. If such imperfections are to be disclosed by their effect upon the diffraction of radiation in producing or cancelling spectral lines, then there are two essential conditions, *viz.* that the imperfections should be periodic and secondly, that the repeat distance should be comparable with the wavelength of the radiation. As there is no reason why either of these conditions should be fulfilled, X-rays are limited in their usefulness in this problem. Fortunately, however, should such imperfections exist, they must determine the size of the coherent regions of the ordinary lattice, and in this way become amenable to detection by X-rays by the small modifications of intensity distribution to which they give rise in existing spectral lines.

An early step in this direction was made by W. L. Bragg [1], Evald and others, by studying the intensity of reflection of X-rays by crystals in both magnitude and direction, *i.e.* in absolute value and angular spread. If the network of diffracting centres corresponded exactly with the mathematical conception, the width of an X-ray reflection under the usual experimental conditions should be about

5″ of arc, whereas in practice, much larger values, often 300″ of arc, were generally obtained. The integrated intensity of reflection also was much larger for most substances than the ideal lattice would warrant. Both these discrepancies pointed to lattice imperfections and vindicated the ideas advanced originally in 1914 by C. G. Darwin [2]. As the basis of his mathematical work, he considered that crystals may be in any stage of imperfection between the two extreme cases of (i) the ideally perfect crystal corresponding closely to the mathematical lattice; and of (ii) the ideally imperfect or mosaic crystal consisting of minute blocks of a few thousand atoms for linear dimensions, each block being perfect in itself but set only nearly parallel to its neighbours. The lack of parallelism spreads the reflection and increases the integrated intensity by allowing blocks situated well inside the crystal to add their full quota to the reflection. With a very few exceptions, nearly all crystals correspond, more or less closely, to the mosaic type.

This one type of experiment alone is sufficient to show that perfection is extremely rare even in the world of crystals, imperfection being the normal state for all methods of growth. Naturally many questions present themselves, *e.g.* why should ideally perfect crystals be so rare, what other evidence can be advanced as to the nature or even the existence of these imperfections and are they a fundamental property of matter? The present position is therefore one of great interest, as witness the time devoted by the recent International Conference on Physics to these and many other problems associated with the solid state. In fact such a wealth of theory and experiments of so diverse a nature have been brought to bear upon this subject, that it is impossible even to mention many important researches in a short account of the position.

The ideal lattice structure is adequate for the explanation of many physical properties and calculations, as an example of which may be quoted the calculation of the refractive indices of calcite and aragonite by W. L. Bragg; but there are other properties, classed as structure-sensitive, for the explanation of which it soon becomes obvious that something more is needed to complete the picture. From amongst the structure-sensitive properties, it may not be out of place to direct attention first to the particular one which will emphasise the technical importance of the problem in hand, *viz.* to cohesion. The ultimate breaking stress is always measured and regarded as a definite physical constant for a material, yet it does not agree, even in order of magnitude, with values calculated on the basis of ideal lattices. The calculation certainly involves a knowledge of the laws of force between the atoms in the lattice under consideration, but although it is not necessarily exact, it cannot be in error to any great degree. The measured values are always too small by a factor of some hundreds or even some thousands. What is the cause of this discrepancy ?

Attention was attracted to the problem by A A. Griffith [3], who, in 1920, showed that values very much nearer the theoretical do occur in special cases. Experimenting with freshly drawn thin glass or silica fibres, he obtained breaking stresses up to one million lb.-wt. per sq. inch, whereas about twenty thousand is the normal value. Unfortunately these fibres proved to be unstable, the breaking stress being large only if the measurement was made soon after the preparation of the specimens. After a short interval the fibres would revert spontaneously to their normal weak state ; but if fractured whilst in the abnormally strong state, the report was much louder and deeper than that accompanying normal fracture, whilst the fibre was always shattered into numerous pieces and often reduced in parts to a powder. A fracture at any position markedly reduced the strength throughout the entire length of the fibre. Griffith found that the breaking stress S of the normal weak fibre could be correlated with its diameter D by a formula of the type $S = a + b/D$, where a and b are constants, a result which he interpreted as indicating a weak interior and a very strong surface layer Numerical values indicate for this skin a maximum breaking stress of about $1{\cdot}6 \times 10^6$ lb. wt./in.^{2} and a thickness of about 10^{-5} in.

Griffith attributed the weakness of the interior to the existence of faults, coupled with a process of fault propagation, arising from the stress concentration which would naturally occur at the sharp edges, and in explanation he assumed a molecular model in which attraction is dependent not only upon the separation of the molecules but also upon their relative orientation. For a solid consisting of molecules of this type, orientated initially at random, he visualised the approach towards stable equilibrium as rotations, aligning the molecules with their attraction maxima, collinear or coplanar. Each sheet or unit, containing a large number of ordered molecules, would be arranged in a haphazard manner relatively to its neighbours and, possessing directions of maxima and minima of attraction, could itself constitute a potential flaw. In a free surface, the sheets were to be orientated with their maxima of attraction lying in, *i.e.* parallel to the surface, a tendency diminishing with increasing distance from the surface. Such an alignment would cause the flaws near the surface to run parallel with it, endowing this surface layer therefore with an enhanced strength. Griffith advanced evidence in favour of the existence of these units of about 10,000 atomic diameters in width in both solids and liquids, and, although his ideas are not considered entirely correct to-day, they served a valuable purpose as an incentive for further work on this problem.

Before proceeding to consider later developments, however, it will be advisable to recall the existence of two rather different types of mechanical failure, *viz.* plastic and brittle. Illustrating failure by reference to a uniform rod under tension, the plastic type is associated with a *local* thinning of the stressed material prior to fracture at this position, whilst the brittle type occurs without previous deformation. The type of fracture likely to occur in any particular case depends upon the relative values of the elastic limit and of the breaking stress, and will be plastic or brittle respectively as the elastic limit is lower or higher than the breaking stress. Now whilst the latter is almost independent of temperature, the elastic limit decreases with rise of temperature, so that materials may be subjected to either type of fracture according to the temperature: *e.g.* cadmium, which is plastic at room temperature, is brittle at that of liquid air; and magnesium, which is normally brittle, is plastic at temperatures corresponding to a "red heat." The fibre experiments of Griffith were, of course, examples of brittle fracture.

Many of Griffith's suggestions have been challenged by Joffé [4] who, with his co-workers in Russia, has carried out many interesting investigations on solids. He etched in hydrofluoric acid a section of a glass rod of about one mm. diameter down to about 60 μ and showed that, if put under tension whilst still in the process of etching, the unetched segment of original thickness fractured before the thin portion; and secondly, that the breaking stress of the etched part was the same as that of a fibre of equal thickness drawn in a flame. Thus the rod had been strengthened by the removal of its surface. Elaborations of this type of experiment led Joffé to agree with Griffith in visualising the bulk of material as riddled with a random distribution of an immense number of flaws, but to disagree with him as to the cause of weakness. Joffé considered that these flaws only became dangerous on reaching and forming a part of the surface, the most dangerous flaw being responsible finally for mechanical failure. It is rather curious that ideas should have to be so completely reversed, for whereas Griffith considered the surface as a source of strength, Joffé maintained it to be the real source of weakness, arising from the initiation of macroscopic cracks at the sharp surface irregularities. The immense number and statistical distribution of the flaws accounted for both the constancy of the breaking stress as measured on large specimens and for its marked variations from one specimen to the next for very thin fibres. The law of variation of breaking stress with fibre size follows from the assumption that the probability of a fault of given sharpness, upon which is dependent the degree of danger, increases with decrease in size of the fault, *i.e.* there are only a few sharp faults large enough to be dangerous. The large surface of a big rod is therefore much more likely to contain some of these really dangerous faults than is the surface of a small fibre, *i.e.* the probability of a dangerous surface fault decreases with decrease of surface, and finally therefore thin fibres should possess greater strength.

We turn now to other materials and methods which have been used in demonstrating the existence of a high cohesion. All the methods are designed to avoid surface weakness, either by continuously creating a new surface or by placing the body of the material under stress whilst leaving the surface free. The former method which includes the etching of glass fibres already described, was used also by Joffé for observations on rocksalt. The normal breaking stress of the crystal is about 0 4 kg.wt./mm.^{2}, but, by maintaining the crystal under stress whilst the surface was being continuously dissolved away, he obtained values up to 160 kg.wt./mm.^{2}, which is not much below the theoretical maximum of 200 kg.wt./mm.^{2} It is noteworthy that the values obtained under such conditions are in excess of the elastic limit and, as would be expected in such a case, rocksalt, which is normally brittle, becomes plastic. This has raised the difficult question, at present unsolved, as to what has happened to the underlying lattice when, say, a right-angled bend has been impressed on a slip of rocksalt.

The first of the experiments involving the second method of demonstrating large cohesive forces is due again to Joffé. He subjected spheres of rocksalt to a very sudden change of temperature by first cooling them in liquid air and then inserting them in molten lead. Although the spherical surface was free, very large stresses must have been developed internally, and from the fact that the specimens remained intact, it was calculated that the cohesive forces must amount to at least 60 kg.wt./mm.^{2}. The second experiment was concerned with the tensile strength of mica lamellæ, which Orowan [5] measured by applying the tension through clamps much narrower than the width of the mica strip and much closer together than its length. This simple artifice, by which the stress was applied between positions far removed from any edge, was effective in leaving the edges of the strip practically free from stress, so that the tendency for crack propagation inwards from the edges was very much reduced and correspondingly large values of the breaking stress were obtained.

Having considered at some length the great discrepancy between the theoretical and practical breaking stresses, it is perhaps relieving and certainly suggestive to consider a case of agreement. Such agreement between theory and experiment exists if, instead of the cohesive *force*, the surface *energy* is considered. To create new surfaces, work must be done, and Obreimoff [6] measured this quantity for the cleavage plane of mica. He measured the energy which had to be expended to split off a given area of lamella by driving in a carefully shaped glass wedge by successive taps with an iron hammer actuated by a magnetic field; performing the experiment in a vacuum, he obtained the large value of 20,000 ergs/cm.^{2}. In a vacuum it takes a considerable time for sheets to separate, whilst in the air a sheet could be levered off immediately, the air and moisture quickly contaminating the freshly formed surfaces and destroying any residual attraction between the ions in the opposite surfaces. Even a monomolecular contamination layer is sufficient completely to alter the properties of a surface, so that due allowance for this must be made in the interpretation of any results. Obreimoff's experiment, as Sir William Bragg has pointed out, really provides the clue to the mystery. Surface energy and cohesive force can both be calculated from the lattice theory, the first result agreeing closely with experiment, the second disagreeing entirely. This means effectively that, whilst there is agreement as to the value of the product of force and distance, the force itself is in practice too small, *i.e.* the process of breaking a body involves too small a force acting over too large an integrated distance or, expressed alternately, the interatomic attractions are overcome successively; in fact it is just one more example of the old adage that unity is strength. Sir William Bragg has given an instructive illustration in recalling the small sharp cut in the edge of a piece of material, which everyone would make before attempting to tear the material into two pieces. In view of the satisfactory nature of this explanation, there is little doubt that weakness must arise from concentration of stress at the ends of faults of some description, most probably propagated inwards from the surface; much remains, however, to be said about the faults themselves.

Passing now to the other type of deformation and rupture, *viz.* plastic, we encounter a whole array of new phenomena, phenomena for many of which no really satisfactory explanation exists.

The phenomena are capable of easier analysis if observations are made on single crystals. Metals, some of which were obtained by Andrade in 1914 in the form of single crystals are very suitable, particularly as their preparation in this form is now comparatively easy and the orientation of the internal structure relative to the outer contour can be controlled. If single crystal wires or rods are stretched, the surfaces become marked by a large number of parallel lines. These are really the elliptical traces on the cylindrical surface of the wire of a parallel set of slip planes in the metal. They will therefore appear straight or curved, depending upon the viewpoint. The slip planes are, of course, referable to the crystal lattice and are generally certain members of the set of parallel planes containing the greatest number of atoms per unit area in the structure, *i.e.* the most widely spaced planes of the lattice. Frequently two or more sets of slip planes become operative and cover the surface with their respective traces, but for the moment it may be assumed that a single set alone is involved.

The various parts of a wire slip relatively on this set of planes corresponding to the rather crude analogy of a portion of a stack of corrugated iron sheets sliding over the remainder. In the case of the crystal, the direction of slip can generally be specified as parallel to the direction of greatest linear atomic density.

The fracture of plastics is of interest, as it formed one of the difficulties of the Griffith theory, which would necessitate for such materials the existence of very long cracks. Recently, however, Orowan has shown that these are not essential, quite small cracks of the order 1 μ, coupled with plastic flow, sufficing to initiate the process of crack propagation.

The feature of particular interest is that, although all the members of a parallel set of planes in a lattice are crystallographically similar, yet the slipping is restricted, as far as can be ascertained, to a comparatively small number. The operative planes are separated from one another by several hundreds, possibly thousands, of inoperative neighbours. The discovery and explanation of some process leading to the selection of these comparatively few isolated planes is the aim of many current researches. The total extension of a stretched wire results then, not as the integral of an infinitesimal slip on all the equivalent planes, but as the sum of a measurable slip on widely separated planes. Joffé and others have shown that the extension progresses discontinuously, the individual slips being audible like ticks of a clock, following one another regularly at gradually increasing intervals, the magnitude of the jumps being approximately constant. Naturally the spatial separation of these planes is of fundamental importance, particularly as, although differing widely in materials, for any one specimen it is sufficiently constant to lead some investigators to demand an exact periodicity. A statistical periodicity, however, such as has been shown by Andrade and Hutchings [7] to exist for solid mercury, would seem to meet the case more closely. It is easy to consider that the particular planes on which slip occurs, were really inherent slip planes already differentiated from the remainder before any deformation takes place. The differentiation might arise, for example, from traces of impurities segregated in these planes, although for mercury this explanation has been shown by Andrade not to be acceptable. Another suggestion was due to Zwicky [8], who postulated the instability of an ideal lattice and supposed that the special nature of these planes arose from some form of secondary structure of an ultramicroscopic size of several thousand atomic diameters as the periodic unit, a structure which was to be inherent in all crystalline materials. Zwicky's hypothesis of regular secondary structures has met with considerable opposition, but it is worth notice since it has led to many interesting experiments.

It is obvious that atoms or ions in or near the surface of a crystal, say rocksalt, must reside in a less uniform field of force than those well within the body of the material, resulting in a residual attraction normal to the surface and directed inwards. There would be a tendency, therefore, for the surfaces of conic structures to contract as though they were in tension; in fact, the effect is measured as if it were a surface tension. In the case of rocksalt, Lennard Jones [9] has calculated that a two-dimensional lattice of Na and Cl ions would have a spacing smaller by 5 per cent. than that of rocksalt itself. Naturally in an actual crystal, the surface cannot be contracted to this extent without severely deforming the lattice. Zwicky considered the surface contracted in patches of substantially uniform size, separated by crevices, the linear spacing of which he calculated to be about 100 Å. both in and perpendicular to the surface. Upon this structure he superimposed a still coarser superstructure to correspond in size to the mosaic blocks discovered by X-rays, and arrived finally therefore at a regular unit of a size indicated by many diverse experiments. It is now admitted that the conception of a regular secondary structure was based upon a rather unsafe foundation.

If the exact periodicity is replaced by a partial random distribution, but with a tendency towards some selected spacing, other hypotheses are forthcoming. Joffé, for example, suggests that slipping may tend to destroy a plane and that the irregularity introduced may extend to some distance on either side, thereby causing these regions to remain free from subsequent slipping and introducing an average spacing between slip planes.

Smekal [10] and his school have been to the fore in attacking the regular secondary structure, but still favour the internal surface, which they consider reconcilable with the ideal lattice. These internal surfaces or gaps, of which there must be an immense number, are considered to arise in the course of the growth of the crystal, which, as Volmer and Brandes, and Kossel and Stranski have shown, is essentially a tangential growth along definite lattice planes. The molecular or atomic character of the growth expresses itself as deviations, perhaps in the form of gaps or variations of orientation for which Smekal postulates a distribution which in general locates the flaws in the principal planes of growth. Such a distribution coupled with simultaneous growth from many centres would, Smekal considers, produce a type of block structure.

Once again these primary flaws are submicroscopic, but from them a secondary system of much larger flaws may arise as a result of distortion due to temperature changes or mechanical working. The primary group is made responsible for many properties associated with flaws, whereas plastic deformation discloses the secondary group alone. A fuller development of these ideas enables Smekal to give some explanation of the structure sensitive properties such as diffusion, photochemical phenomena, cohesion and plastic deformation. It may be noticed in passing that the original Griffith cracks are gradually becoming less severe, in that the irregularities may be just discontinuities caused by the inclusion of stranger atoms or be only variations of density, as Smekal suggests, a kind of "frozen Brownian motion."

Before considering any other theories, we may pause for a moment on one particular question, *viz.* is a block structure, which has figured rather frequently as a fundamental state of matter, really constitutional? In partial answer to this we must renew our consideration of rocksalt, unexpectedly as it may be, not in confirmation but as a caution. Recently, by careful crystal growth, Evald and Renninger [11] have been able to show that rocksalt, the standard example of mosaicness, need not be mosaic at all. They obtained X-ray reflections agreeing exactly with those expected from a perfect lattice. If in contrast to a freshly cleaved surface, a polished surface of the same crystal was examined, the results for the mosaic structure were at once obtained. The polishing had produced mosaicness to a depth at least greater than $\frac{1}{10}$ mm. Here then is definite evidence that, at least in a range below 1 μ, the crystal can be perfect, and mosaicness can no longer be considered constitutional.

As a second illustration of the same wide variation in crystals of one material, reference may be made to the work on the diamond of Robertson and Fox, who have shown that even amongst these crystals, which normally form the standard of perfection, there are just a very few specimens possessing a laminar or mosaic structure and exhibiting the properties associated with this type. It has been suggested that these specimens may have been subjected to deformation under special conditions at an early stage in their history. Andrade and Martindale [12] succeeded in making visible certain structural properties of the surface of these diamonds by spluttering on them thin uniform films of silver or gold about fifty atoms thick. Subsequent heating *in vacuo* up to just over 200° C. caused these films, except the part immediately in contact with the supporting surface, to become so mobile that small agglomerations of silver, say, were formed at the expense of the general thickness of the film. Two points of interest arise: firstly, evidence is advanced for considering these specks to be spherulitic; and secondly, their distribution on the surface. On a normal diamond the distribution is haphazard, but on a mosaic diamond the spherulites are formed along very sharp lines. Sometimes these lines are curved, but in many cases a group of parallel lines of spherulites is formed. They were obtained also on silica but not on mica, the cleavage face of which is known to be more nearly perfect. The importance of this linear grouping lies in the fact that these spherulites line up where no line or marking was previously visible; they expose therefore some surface irregularity which would otherwise remain undetected. Andrade points out that these lines correspond to traces of important crystalline planes and suggests that they correspond also to the Griffith flaws.

There remains only one more phenomenon of major importance which has so far not found a place in this account, mainly because so much study has been devoted to the problem that its introduction at an earlier stage would have been attended by the risk of exclusion of other matters; it is the work-hardening of metals. Although in practice experiments frequently deal with materials of polycrystalline texture, the phenomenon is so complex that it is essential to restrict our attention to a single crystal. Even quite a thick rod of copper, for example, can be bent quite easily if it is a single crystal, but to maintain or repeat the process is impossible without a very large increase of stress; some profound change, termed work-hardening, has been induced. Visual and X-ray observations by W. G. Burgers [13] and others show that a shear deformation can be produced which, on the macroscopic scale, appears homogeneous. Such an ideal shear deformation could occur without any distortion of the lattice, but the asterism shown by Laue photographs reveal the inhomogeneity of the process and a curvature of the crystal planes. The apparently conflicting results can be consistent, however, if the curvatures are strictly local in character. These would cause a sort of puckering, and probably arise from the limited extent of the regions of glide. The curvatures may be the result of an attempt to overcome the local non-correspondence of neighbouring atom rows, in fact to accommodate the lattice dislocations which are of primary importance in G. I. Taylor's [14] theory, to which reference will be made almost immediately. Amongst the many results which Burgers has obtained is the dependence of shear hardening on the total shear alone, irrespective of its occurrence on one or more sets of planes, an observation which he interprets as arising from the equality of hardening effect of a large number of weak and a small number of strong curvatures. Recrystallisation power also he finds to be closely associated with these local curvatures.

Taylor's theory joins company with those which endow the perfect crystal with ability to withstand large stresses and which attribute the observed weakness to a concentration of stress due to internal faults. Dealing only with cubic structures, Taylor visualises each fault as arising from the deficiency of one atom. Each deficiency creates a dislocation which is healed progressively in successive rows, and according to the direction in which the healing is effected, the dislocations are termed positive or negative. They move in opposite directions along lines of atoms under the action of a shear, and both produce exactly the same result, leaving in their wake a slipped but undistorted crystal. He finds that unlike dislocations attract, but like repel; so that although an isolated dislocation is mobile under the smallest shear stress, the presence of both types at once introduces a minimum stress below which plastic distortion or slipping cannot proceed. Furthermore, although the dislocations are distributed at random initially, mutual interaction would tend to produce a pseudo-regular lattice distribution and lead to a finite crystalline shear strength. The mathematical theory involves a distance which can be interpreted as the average path which a dislocation can travel freely under a very low stress before being stopped at one of the internal surface barriers which form an essential part of the theory. The average separation of these barriers, which form a mosaic pattern, is about one to five microns, depending upon the material; that is just slightly larger than the mosaic unit which so many experiments almost demand.

Straumaris, for example, has shown how crystals of cadmium can grow as short hexagonal pyramids in which a unit of layer thickness of 0 8 μ appears to exist; whilst for lead, Andrade and Roscoe found the most probable separation of slip planes to be 4·2 μ. Pearlite lamellæ in steel, such as are shown in the fine microphotograph by Wrighton [15], display a spacing of 0·16 μ. Many etch figures offer strong support for units of about the same size, *e.g.* Goetz [16] has shown how the cleavage plane of bismuth possesses a reticular triangular marking of which all the lines can be fitted to a triangular network of size 1·4 μ. Careful observation of the progress of etching bismuth shows that the small pits grow more or less circularly, until the sides of a unit triangle are reached; these seem to offer resistance, for the curved figure grows only in those parts away from the triangular edges until the whole triangle is completely etched. Even the process of grinding rocksalt crystals offers strong support to a texture of small cubes of about one micron linear dimension. The material dislodged is always in the form of small cubes even when ground from an octahedral face, whilst the relative amounts of work necessary to grind from crystal faces is also consistent with this texture. The process of solution has been shown by Traube, Klein and Behren to occur, in some cases, by the breaking of a crystal into an immense number of particles of ultramicroscopic size, whilst lastly, many experiments have been devised to prove that small ordered crystal units exist in the melt well above the ordinary melting point. The continued existence of such units is invoked to explain the pronounced dependence of the possibilities of super cooling and of the texture and orientation of subsequent crystal growth upon the maximum temperature attained.

Taylor's theory requires the strain to occur within these mosaic blocks and is not concerned with the actual nature of the stress barriers, which may be pores, superstructures or interlineage dendritic boundaries as suggested by M. J. Buerger [17]. By supposing that the stress boundaries are able to transmit dislocations to a limited extent dependent upon temperature, Taylor is able to give a reasonable explanation of the observed dependence of plasticity upon temperature. He has applied the theory not only to metals but also with equal success to rocksalt; however, even he himself would not expect this to be the last word, rather the reverse; it is the first theory which has enabled calculations to be made in this extremely difficult and complex field. The theory possesses the great advantage of being capable of expression in exact terms. This enables the problem to be treated mathematically and yields results agreeing closely with experimental observations.

In attempting to test the various theories mentioned, a great amount of work and ingenuity has been brought to bear upon the problems; they are so interconnected as to defy orderly presentation. It is quite obvious that current ideas of the solid state are not fully crystallised, at least with any ordered certainty, and in summarising too much importance may be given to one or other idea. It does seem that "filling space" is not quite as easy as might appear, for if the atoms are built into a perfect array, the edifice may begin to collapse under almost a glance, although to carry on the destruction requires greater and greater effort. It is fascinating to speculate upon what would be the strength of a *really perfect* crystal, as improved purity and greater precautions generally have reduced the limit of shear stress at which visible deformation begins, to a curiously consistent value of about 10 gm. wt./mm.^{2} for several metals. Is the absolutely perfect lattice really very strong or is it so weak that its rarity arises from its inability to support its own weight? Sometimes the view is expressed that the frequency of occurrence of the mosaic structure results from the distortions created by temperature gradients and changes associated with the process of solidification.

It is obviously impossible definitely to specify yet the cause of structural mosaicness, and it is not at all certain that any gain of mechanical strength would result from its removal, were that possible. The immediate cause of the weakness of materials is reasonably explicable, but it remains for someone to find a means of preventing neighbouring planes of atoms parting company except as a concerted action.

In conclusion, this short account will have served its purpose if it gives to the reader some idea of the problems still ahead and of the fascination of trying to solve them.

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RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

The Size of the Galaxy —Estimates of the size of the galaxy are necessarily dependent on the amount of light absorption caused by the obscuring clouds of matter which undoubtedly exist near its central plane. In the *Publications of the Washburn Observatory*, XV, Part 5, Joel Stebbins and C. M. Huffer deduce the amount of space reddening in the galaxy from observations of the colours of 733 B-type stars, measured with a photo-electric cell fitted with blue and yellow filters, giving effective sensitivities at about 4260 A. and 4770 A. respectively. The normal colours of B stars were taken from the means of such stars brighter than visual magnitude 6 0 and more than 15° from the galactic equator, and from 110 standard stars of classes A to M, all giants, and with galactic latitudes greater than 10°. The observed colour excesses of the B stars, when charted in galactic co-ordinates, demonstrate the existence of a thin layer of dark scattering material near the median plane of the galaxy. This layer seems to be irregularly distributed, for there are a number of regions where the reddening effect is exceptionally strong, but, taken on the whole, there is a conspicuous variation of space reddening with galactic longitude, the coefficient increasing with angular distance from the anti-centre. The galaxy appears to resemble a spiral nebula with a clear space or lane inclined about 40° to a radius from the centre. In the view of Stebbins and Huffer the general region near the galactic centre is opaque to the light of objects which lie beyond the nucleus. In Shapley's general system of globular clusters the positions in space of a number of members are nearly in line with, but far beyond the centre of the galaxy, but the authors of this paper consider that in this direction the light of all such objects is blotted out altogether, and that the fainter globular clusters actually observed there are really between us and the centre. Certainly the reddest B stars are in the zone of avoidance of the clusters, and it is probable that still greater reddening would be found if the investigation were extended to still fainter B stars close to the galactic centre. It might naturally have been expected that the material which causes the reddening would be connected with the calcium clouds which, by measurement of the spectra of early B stars, have been shown to partake in the rotation of the galaxy. However, Stebbins and Huffer find no correlation between the colour excesses and the intensities of the interstellar calcium lines in the spectra of the B stars, and there is evidently no such thin layer of the calcium cloud as there is of the reddening material. Whether this latter is composed of dust particles or of gas molecules cannot yet be decided. It seems certain, however, that hitherto accepted distances of objects near the galactic plane, especially in the direction of the galactic centre, must be greatly reduced. According to the maximum value found for the absorption the distances must be divided by 2·5, but the necessary correction varies considerably in different galactic longitudes.

Further information concerning this stratum of absorbing material has been obtained by E. Hubble from an investigation into the distribution of extra-galactic nebulæ (*Contributions of Mount Wilson Observatory,* No. **485**). Counts were made of about 44,000 nebulæ on 1,283 plates taken with the 100-inch and 60-inch reflectors, distributed over the three-quarters of the sky north of Dec. –30°. He finds a zone of avoidance along the galactic plane, irregular in its boundaries, and bordered by a region of partial obscuration, which gradually fades away into the areas of normal distribution. Obscuration is conspicuously greater towards the galactic centre than in the opposite direction. It thus appears that the evidence from B stars, from clusters, and from the extra galactic nebulæ all agree in establishing the presence of this absorbing material near the galactic plane. It is probable that it is similar in form to the dark lanes so often observed in other galaxies which we view in an edge-on position. Current estimates of the size of our galaxy have made it much larger than the similar systems by which it is surrounded, but, when the effect of the absorption due to the dark material is properly allowed for, this discrepancy will largely disappear, and our galaxy will approximate in size as well as in structure to the vast numbers of others which constitute the visible universe.

In an address entitled " Between the Stars (*Publ. Ast. Soc. of the Pacific,* **271**), J. A. Anderson considers that meteoric matter in a rather fine state of division is responsible for the obscuration. Assuming that the particles are spherical, opaque, and sensibly non-reflecting, he calculates that the total quantity of matter of this kind in our galaxy necessary to produce an absorption of one magnitude for light travelling normal to the galactic plane is about equal to 10^9 solar masses. The number of stars in our galaxy is estimated as between 10^{10} and 10^{11}, the unoccupied space being at least 10^{19} times as great as the combined volume of all the stars. This space contains, in addition to the obscuring material, a very tenuous gaseous medium consisting of atoms of neutral sodium and of ionised calcium, which show themselves in the spectra of certain stars by lines which cannot originate in the atmosphere of the star itself, but must be produced in the intervening space. According to Eddington, conditions in space are such that for every neutral sodium atom there must be 100,000 ionised atoms, and Anderson considers it probable that atoms of other elements are also present, more or less in the same relative abundance as they are found in stellar atmospheres. This interstellar gaseous medium is strongly concentrated towards the galactic plane, and Anderson estimates its total mass as approximately equal to that of the whole of the stars in the galaxy. It cannot, however, be responsible for the obscuration and reddening, for the quantity of material of this kind which would be required would be so great as to be ruled out by dynamical considerations.

The distribution of interstellar scattering matter as shown by colours of early type stars is the subject of a paper by Samuel L. Thorndike (*Lick Observatory Bulletin*, **461**). On the assumption that stars of the same spectral type emit light initially of the same colour, any abnormal redness would indicate the presence of scattering matter in space. The photographs were taken with the Crossley reflector, fitted with a slitless spectroscope with two quartz prisms, and each plate contained spectra of comparison stars for purposes of calibration. The plates were measured with a micro-photometer, and the colour of each star was computed from the relative intensities of its light at certain points in its spectrum. The colours were plotted against apparent visual magnitude, against galactic latitude, against distance from the plane of the local system, and against the richness of the stellar field. The results indicate that the differences observed in the colours are due to intrinsic dissimilarities in the stars as well as to the effects of interstellar scattering matter. The latter appears to be by no means in the form of a continuous stratum along the galactic plane, the obscuring masses being distributed in a haphazard manner but confined to the neighbourhood of that plane. The redness of the stars shows no correlation with distance from the plane of the local cluster, and the inference is drawn that the irregular clouds of scattering material are connected with the galaxy as a whole and not with our local system. As only stars of apparent magnitude brighter than 10 were included in the investigation the conclusions are restricted to a region within about 2,000 parsecs of the sun, which represents only a small fraction of the space occupied by the galaxy as a whole.

There is fortunately a further way of determining the dimensions of the galactic system which is entirely independent of estimates of Cepheid luminosity or of space absorption. This is provided by the theory of galactic rotation developed by Lindblad and Oort, the distance to the gravitational centre being obtained dynamically from the constants of the rotation. According to Oort the stars have a systematic radial motion of the form $rA \sin 2(l - l_0)$ and a transverse motion of the form $rA \cos 2(l - l_0) + r B$, where l_0 is the longitude of the galactic centre, V is the orbital velocity, and $B = A - V/r$. To determine our distance from the galactic centre the constants A and B together with the value of V in the neighbourhood of the sun are required. In *Monthly Notices*, *R.A.S.*, **94**, No. 8, J. S. Plaskett and J. A. Pearce give results obtained from a study of stars of spectral classes O to B7, which include the most luminous stars in the sky and therefore the most distant that can be adequately spectroscopically observed. The radial velocities, mostly obtained at Victoria, B.C., in the last ten years, number 849, and 881 proper motions of stars of the same classes have been utilised in the investigation. The radial velocities were corrected for a large peculiar motion in the Scorpio Centaurus group, and values were then obtained for the solar motion, the K term, and the galactic rotation. The mean value of l_0, the longitude of the galactic centre, has been adopted as 324°. To obtain the constant A it is necessary to know the mean distances of the various groups of stars. These were derived from the proper motions after all known systematic movements such as Raymond's correction, the solar motion and stream motion had been removed. The constant B was then obtained from the proper motion components in galactic longitude, while Stromberg's value of 275 km. per sec. was adopted for the orbital velocity in the neighbourhood of the sun. The distance to the galactic centre was found to be 10,000 parsecs and the diameter of the galaxy to be 30,000 parsecs, with a period at the distance of the sun of $2 \cdot 24 \times 10^8$ years. The total mass of the system is estimated as $16 \cdot 5 \times 10^{10}$ times that of the sun. On the assumption that the galaxy contains 10^{11} stars with average mass equal to that of the sun, interstellar material must be responsible for the very considerable balance.

Thus the dynamical and luminosity methods agree in fixing the distance to the galactic centre as 10,000 parsecs, this being the value to which Shapley's latest estimate of 16,000 parsecs is reduced by Stebbins' allowance for the elimination of the effect of space absorption. The value of 30,000 parsecs for the diameter of the galaxy can therefore be accepted with more confidence, especially as the disparity in size between it and the diameter of the Andromeda Nebula, the largest known external system, has now almost disappeared. In Hubble's opinion certain nebulous objects photographed in the neighbourhood of the Andromeda Nebula are globular clusters, similar to the clusters near our own galaxy, forming part of the system and increasing its total diameter to 30,000 parsecs. A similar result has been reached by Stebbins from measures of sky luminosity near the Andromeda Nebula with his photo-electric photometer (*Proc. Nat. Acad. Sci.*, **20**, 93). Our galaxy appears to be a spiral similar in general structure and dimensions to the Andromeda Nebula. In Plaskett's words, "There is no necessity for considering the galaxy as a complex aggregation of small nebulæ, and our inherent belief in the homogeneity of the Cosmos is now well supported observationally."

It has been found at Harvard and elsewhere that a modern microdensitometer can make a photographic register of the outermost extensions of nebulæ well beyond the point at which they cease to be detected by the eye, and therefore the measures of practically all galaxies made hitherto by visual examination of photographic plates are too small. In the *Bulletin of Harvard College Observatory*, **895**, Harlow Shapley has four notes on the diameters of galaxies as determined by microphotometer analysis. The notes are interim reports of investigations whose full results will be published later in more detail. The angular diameter of the Andromeda Nebula has been measured on long exposure plates taken with the Harvard patrol cameras, and the lengths of the major and minor axes were found to be 270′ and 230′ respectively. These dimensions are considerably larger than have been detected on the reflector plates, the extension being due in Shapley's opinion to the small focal ratio, the length of the exposures, the flatness of the field, and the small scale which help in satisfactory microphotometric analysis. Using Hubble's revised estimate of the distance of the nebula, 247 kiloparsecs, the linear dimensions of the axes become 19·4 and 16·4 kiloparsecs. Certain outlying clusters must now be regarded as definitely within the bounds of the major system, whose shape tends much more towards sphericity. By analogy, Shapley considers that the Magellanic Clouds, the nearest to us of the external galaxies, really lie within the thinly populated margins of our own system and belong to it.

While the Andromeda Nebula appears to be a spiral with satellites rather than a multiple of equally ranked galaxies, an example of a double galaxy is found in Messier 60 (N.G.C. 4649), in the Virgo group. Here we have an 11th magnitude spheroidal system with a 12th magnitude spiral companion, the nucleus of the latter being well within the radius of the former, and vice versa. On ordinary photographs they appear quite separated, their edges being divided by a space nearly equal to their radii, but from a microphotometer tracing it becomes plain that the two are really nuclei of a multiple system whose diameter exceeds 9,000 parsecs.

In *Monthly Notices, R.A.S.*, **94**, No. 3, J. H. Reynolds, in a paper on "The Spatial Distribution of the Extra-galactic Nebulæ within a Radius of 4,000 kiloparsecs," concludes that the dimensions of spiral nebulæ are on an average more than twice those of spheroidal systems, the latter being more comparable in size to the nuclei of the spirals than to the spirals themselves. Shapley has found that with the application of the microphotometer to long exposure plates this difference in size tends to disappear. In an examination of 85 objects, all members of the Virgo cluster and therefore presumably with their linear diameters correctly indicated by their angular ones, the mean diameter of 55 spiral nebulæ was found to be only 1·23 times the mean diameter of 30 spheroidal ones. He considers it probable that if cameras of the most appropriate focal length and aperture ratio are employed the difference in size between the two types of system will disappear altogether.

Among other interesting results reached by Plaskett and Pearce from their study of B-type stars referred to above is a new determination of the solar motion. The velocity is found to be 19·09 km. per sec. towards a point L = 21°·4, B = + 20° (galactic co-ordinates), and both velocity and apex agree closely with the values obtained from the stars in general. The K term, an excess of positive radial velocities always found in B-type stars, has hitherto been a puzzling phenomenon, for it seemed as if this group of stars were expanding with respect to the sun. Plaskett and Pearce have found that for northern stars this term is much reduced by allowing for galactic rotation, while for southern stars a reduction is obtained by removing a systematic outward trend identified with the radial component of the southern stream first investigated by Kapteyn. When these corrections are applied the K term is reduced from its earlier value of + 5 km. per sec. to only + 1·1 km. per sec. This smaller value can be satisfactorily accounted for by the Einstein gravitational displacement of the spectral lines towards the red, as computed for average values of the masses and densities of the

B-type stars, while for O-type the red shift reaches the value of + 2·62 km. per sec. Thus Campbell's famous K term has at last received a satisfactory explanation.

PHYSICS. By L. F. BATES, D.Sc., Ph.D., F.Inst.P., University College, London.

NUCLEAR SPIN.—It is well known that certain hyperfine structures of spectral lines arise from a spin of the nucleus of the atom by which they are emitted, but the measurement and interpretation of such structures is by no means simple and often lead to no unique value for the magnitude of the nuclear spin. For example, Kopfermann (*Zeit. für Phys.*, **73**, 437, 1932) measured the intervals of the hyperfine structure of some spectral lines of CsII, and deduced that a nuclear spin of $7/2.h/2\pi$ units was the most probable value for cæsium, but he was unable to rule out the possibility that 8/2 or $9/2.h/2\pi$ units might be correct. Spectra intensity measurements are often equally indecisive. Consequently, apart from the interest normally attached to a direct determination of any physical quantity, the direct measurement of the nuclear spins of sodium and cæsium by Rabi and his collaborators warrants description here.

The measurement in the case of sodium (*Phys. Rev.*, **46**, 707, 1934) depends upon the effect of the nuclear spin on the spatial quantisation of the extranuclear electron configuration in a magnetic field. It is well established by experiments of the Gerlach and Stern type that neutral atoms set with their magnetic axes at definite angles with respect to the lines of force of a magnetic field. Thus, for an alkali atom with a nuclear spin i, $2(2i + 1)$ positions are possible, or a beam of such atoms could be split into two groups, each with $2i + 1$ components If sodium possessed no nuclear spin, there would, of course, be just two simple groups of atoms under these conditions.

Now, if a narrow beam of sodium atoms is passed through a pair of fine slits, S_1 and S_2, it may first be spread out into a velocity spectrum by a suitable non-homogeneous magnetic field, A, such as that used by Gerlach and Stern, provided that the field is sufficiently intense to cause decoupling of the nuclear and electron spins, *i.e.* to cause them to orientate independently of each other, thus ensuring that the force upon each atom of the same velocity is the same. Of course, the force may cause the atom to move either parallel or antiparallel to the direction of the lines of force, so that the atoms are divided into two groups, one of which may be allowed to pass through another slit, S_3. These may now be exposed to the action of a relatively weak non-uniform field in which decoupling will not now occur, and each atom will experience a force whose magnitude will be different for each of the $2i + 1$ magnetic levels. Consequently, the beam falling upon a receiver will now consist of a pattern containing $2i + 1$ peaks.

In making such an experiment it was essential that all the apparatus should be thoroughly evacuated. The sodium was evaporated inside a nickel furnace, S_1 was attached to the furnace and S_2 was mounted 10 cm. from S_1 and accurately parallel to it, so that the beam was parallel to the edges of the "wedge and trough" pole pieces used to produce the necessary non-uniform field of about 200 gauss or œrsteds. Between S_2 and S_3 the atoms successively passed through a weak non-uniform magnetic field, B, formed between pole-pieces 18 cm. long, a field free gap of 1·5 cm., and a stronger non-uniform, focusing, field, C, between pole-pieces 16 cm. long. The troughs of the pole-pieces for the B and C fields were on the opposite side of the beam to the trough of A, in order to produce a narrowing of the several components of the final beam, and the stronger field C served to remove any atoms so scattered in the furnace that they had not pursued a normal route through the slits.

Detection was effected by the method of Taylor (*Zeit. für Phys.*, **57**, 242, 1929). A stretched tungsten filament, partially oxidised, received the sodium atoms, and, accordingly, became a source of sodium ions when heated. The positive ion emission was measured by a suitable amplifier, a sudden heating of the filament causing a rapid emission of the sodium ions and a ballistic deflection of the galvanometer in the amplifier circuit. In this way four well-defined peaks were registered with sodium as the filament was moved across the final beam; *i.e.* $i = 3/2$, or the nuclear spin is $\frac{3}{2}.h/2\pi$ units.

In the case of cæsium the above apparatus was modified and a new method of examination of the final beam adopted by Cohen (*Phys. Rev.*, **46**, 713, 1934). The complete expression for the force acting on a cæsium atom in a non-uniform magnetic field in which there is strong interaction between the nuclear and electron spins and between each of these and the field,—*i.e.* the state of affairs we postulate when considering the anomalous Zeeman effect,—shows us that the force vanishes for certain values of the field, because the magnetic moment of the atom, as a whole, vanishes. Consequently, if a beam of atoms passes through a long, relatively weak, non-uniform magnetic field, the central beam will show marked variations in intensity for certain values of the field. In fact, if the intensity of this beam is plotted as a function of the field strength, variations should be found, the high values or peaks corresponding to the vanishing of the force on a particular set of atoms. Now, the number and relative position of these peaks is uniquely fixed by the value of the nuclear spin. Actually, in the case of cæsium, three equally spaced peaks should be observed if i is equal to $7/2.h/2\pi$ units. The value of the field at any one of the peaks should also be sufficient to fix the value of i uniquely.

The apparatus used by Cohen was that described above, except that S_1 was rather wider, S_2 rather narrower, and S_3 was now a collimator slit, 0·019 mm. wide, and the deflecting field was produced by the field B alone. Detection and measurement of the beam was made by the tungsten wire method described above. Exact knowledge of the value of the field was considered unnecessary, for only the proportional rate of change of the field with the magnetising current was required, and this was found by search coil measurements. In this way the existence of three equally spaced peaks was proved, corresponding to $i = 7/2.h/2\pi$ units, *i.e.* to a hyperfine structure separation of 0 295 ± 0 01 cm^{-1}.

Another important direct measurement, that of the magnetic moment of the proton, by Rabi, Kellogg and Zacharias (*Phys. Rev.*, **46**, 137, 1934), can be only briefly discussed here. They passed a beam of neutral H atoms from a Wood discharge tube through a weak non-uniform magnetic field. The latter was produced in rather an original manner, by passing an electric current through two long, horizontal and parallel straight wires joined in series; the beam of atoms travelled above and parallel to these wires. The coupling was weak and the composite beam received upon a plate coated with yellow oxide of molybdenum soot produced a blue coloured pattern. This consisted of two close components displaced some distance from the position of the direct beam. The separation of the inner edges of these traces and their widths were found, and from these measurements the value of the magnetic moment of the proton was calculated. The theoretical basis of this calculation cannot be dealt with here. The value found for the magnetic moment of the proton was 3·25 ± 0·3 nuclear units, one nuclear unit being the Bohr magneton divided by the mass of a proton—a unit chosen for convenience in view of the smallness of nuclear magnetic moments. The value found by Estermann and Stern (*Zeit. für Phys.*, **85**, 17, 1933) was 2·5 ± ·3 units.

In like manner the American authors measured the magnetic moment of the deuton, and found a value of 0·77 ± 0·2 units. They point out that they cannot, of course, determine the sign of a nuclear moment; consequently, the proton and deuton moments may both be positive or negative with respect to the angular momentum of the nucleus. Hence, if a deuton consists of a proton plus a neutron, then the magnetic moment of the latter could be either ± 2·5 or ± 4·0 units from these results.

The theoretical aspects of nuclear magnetic moments and their origin are discussed in an informative paper by Landé (*Phys. Rev.*, **46**, 477, 1934), who takes as the main basic idea, the view that one particle only, *i.e.* one proton or one neutron, is responsible for the total spin and for the magnetic properties of the whole nucleus. The remaining particles in the nucleus are assumed to form closed shells. Hence the observed vector i is made up of the orbital l and spin s of one particle alone, and since s is known to be $1/2$ for the proton and for the neutron, l can only be $i \pm 1/2$. A subsidiary assumption that two neutrons may be so aligned in a nucleus that their spins become additive, $s = 1$ instead of 0, is also made. On these lines Landé examines the results of hyperfine structure measurements and deduces that the proton and neutron should possess magnetic moments of approximately 2 and − 0·6 units, respectively. The American direct measurements do not appear to support these deductions, as far as can be seen at present.

Recent Thermal Measurements.—Two sets of recent high temperature measurements are worthy of special comment. The first, by Ezer Griffiths and G. G. Sherratt (*Proc. Roy. Soc.* A., **147**, 292, 1934) is a determination of the specific heat of a gas by the sound velocity method, which is shown to be adapted for measurements with carbon monoxide up to temperatures of the order of 2000° C. The main requirements are the production of sound waves of two accurately known frequencies and the precise measurement of temperature.

The source of sound was a rectangular block of quartz crystal which could be set into longitudinal or transverse piezo-electric oscillations; *i.e.* two fixed frequencies were obtainable from the same crystal, so that certain sound dispersion phenomena could be studied. Stationary waves were accordingly produced in the gas inside an electrically heated carbon tube, about 3 ft. long, by placing the crystal near one end of the tube and adjusting the effective length of the tube by a graphite rod and piston, whose position was recorded on a steel scale. Resonance positions of the piston were detected by changes in the plate current of the crystal oscillator circuit. Corrections were, of course, applied for the expansion of the graphite rod, whose coefficient was found by a special experiment, of which details are given in the paper.

The temperature of the gas was determined by sighting a "disappearing filament" pyrometer, by means of a reflecting prism, upon the inner surface of the graphite piston. The carbon tube was surrounded by radiation shields, and enclosed within a large iron cylinder whose sides and insulating ends were water cooled. The purity of the gas was checked by measurements in a subsidiary tube at room temperature. The velocity of sound in free space was calculated from the observations, using a formula appropriate to the case of a tube with rough sides.

The apparent ratio of the two specific heats of carbon monoxide, as found in this work, appeared to be different for the two frequencies used. This difference, however, may be explained on a theory due to Kneser (*Ann. der Phys.*, **11**, 761, 1931), in which it is suggested that energy may not be transferred so readily from vibrational states of the molecule to other degrees of freedom as is postulated on classical mechanics; *i.e.* the gas is not in an equilibrium condition during the experiment, because part of the vibrational energy is unable to follow the acoustic changes, and therefore vanishes from the expression for the adiabatic elasticity.

Kneser's theory leads to a formula

$$C_v' = \frac{C_v^2 + 4\pi n^2\beta^2 C_a^2}{C_v + 4\pi n^2\beta^2 C_a}$$

where C_v' is the apparent specific heat for the frequency n, $C_a = 5/2.R$ is the specific heat of translation and rotation, C_v is the true specific heat at constant volume, and β is the time of relaxation of the vibrational energy. Hence, at any given temperature the measurements of Griffiths and Sherratt gave two equations from which C_v and β may be found. The values thus obtained for C_v were in good agreement with those calculated from data on band spectra, and thus the long standing discrepancy between determinations by the spectroscopic and sound velocity methods disappears.

The other set of high temperature measurements were of the freezing point of platinum on the International Temperature Scale, by F. H. Schofield (*Proc. Roy. Soc.* A., **146**, 792, 1934). This was done by measuring the ratio of brightness of the radiation of a certain wave-length from black body radiators maintained at the freezing points of platinum and gold respectively. The freezing point of gold is defined as 1063·0° C., and is the basic point of the International Scale for all high temperature measurements. For the measurement of the brightness ratio, an optical pyrometer of the disappearing filament type was used in conjunction with a

Corning glass filter, and with a two stage rotating sector arrangement to deal with the 300 to 1 ratio of intensities. In the original paper is given an interesting description of the black body radiators, consisting of hollow enclosures of refractory materials immersed in ingots of platinum or gold, heated by electromagnetic induction. Observations on five ingots of platinum and two of gold gave a mean value of 1773·3 ± 1° C. for the freezing point of platinum. This is in complete agreement with the determination by Roeser, Caldwell and Wensel (*Bur. of Stand. Journ. of Res.*, **6**, 1121, 1931).

We now turn to some interesting work on the thermal and electrical properties of metals. It is well known that the electrical resistivity (ρ) and the thermal resistivity (ω) of a pure metal are both increased by the addition of a foreign metal. Recent experiments by Grüneisen and Goens (*Zeit. für Phys.*, **44**, 615, 1927) showed that in many instances the change in electrical resistivity, $\Delta\rho$, was proportional to corresponding change in thermal resistivity, $\Delta\omega$. In other words, there existed an extension of the well-known Wiedemann-Franz-Lorentz law, which might be written

$$\Delta\omega = \frac{1}{\mathrm{Z.T}}.\Delta\rho,$$

where T is the absolute temperature and 1/Z is measured by the slope of what is termed the isothermal line, obtained by plotting ω against ρ/T, or the values of ω against ρ for constant T. The above statements mean that Norbury's rule, *viz.* that the change in electrical resistance produced by addition of a foreign metal is the greater the wider the horizontal separation of the original and the added metals in the periodic system, holds also for change in thermal resistivity.

Some interesting points were raised by Reddemann (*Ann. der Phys.*, **20**, 502, 1934), in a discussion of the published data on the alloys of copper, aluminium, gold, silver, platinum and palladium with other metals. For example, when the values of thermal resistivity of a series of alloys of different metals with copper are plotted against the corresponding values of the electrical resistivity for a chosen temperature, the points for nearly all the alloys lie on a straight line, the isothermal line, in spite of the variety of the alloys. Reddemann thus found that the slope 1/Z was practically independent of the added metal up to some limiting value of the electrical resistivity.

Grüneisen and Reddemann (*Ann. der Phys.*, **20**, 843, 1934) subsequently measured the thermal conductivities of a number of pure metals and their binary alloys at the fixed temperatures − 183, − 195 and − 251° C. They found that the isothermal line was obtained only when a certain degree of lattice distortion was not exceeded. The lower the temperature, the lower was the degree of such distortion above which departures from linearity were observed. In the case of a binary alloy consisting of mixed crystals the isothermal line was sometimes replaced by a curve in the form of a loop whose ends overlapped.

Grüneisen and Reddemann attempted to explain these results on the assumption that conduction is maintained both by conduction electrons and by lattice vibrations, the isothermal line being due to electron conduction. The lattice conduction was held to obey certain rules deduced by Eucken and Kuhn from a study of the conductivity of insulators. It was found that for high temperatures the experimental results could thus be satisfactorily explained. It appeared that lattice conduction was less affected by distortion than electron conduction. Consequently, if in a pure metal the lattice conduction were small compared with the electron conduction, then a normal isothermal line with constant Z was obtained, until with increase in lattice distortion the lattice conduction became more and more effective, and the isothermal line bent towards the ρ/T axis. The region over which the isothermal line extends therefore depends upon the extent to which the ratio of lattice conduction to electron conduction is influenced by distortion. If this ratio increases rapidly with distortion, as, for example, at low temperatures, then the isothermal line is obtained only in the case of a small degree of distortion.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

One of the most striking of meteorological phenomena is the thunderstorm. It has in consequence been a subject for study and speculation at least as far back as the great Greek philosophers. Aristotle, for example, devoted considerable parts of the second and third books of his *Meteorologica* to the subject of thunder and lightning. It is a remarkable fact that even to-day the subject shows no signs of becoming exhausted in spite of important recent advances. These advances have been rendered practicable, as has been the case with so many physical investigations, by rapid improvements in the manufacture of apparatus for experimental purposes, the technical developments of one section of industry having benefited other branches of industry not intimately connected with them, pure experimental research benefiting in one way and another from all.

The main characteristics of thunderstorms are so clearly defined that one might have expected that one or two simple discoveries concerning the manner of accumulation of the electricity, the discharge of which results in the lightning flash and in the subsequent thunderclap, would have explained practically everything that there is to explain. That, however, is far from being the case. In an interesting paper entitled "The Branching of Lightning and the Polarity of Thunderclouds," J. C. Jensen of Nebraska Wesleyan University [1] draws attention to the surprising fact that there are at least two schools of opinion as to the manner of accumulation of the electric charge, one school following G. C. Simpson [2] and the other school Elster and Geitel [3]. As Jensen's paper includes a survey of a good deal of recent work on this subject, in particular most of the advances made in it in the last twenty-five years, a brief summary of it will be given.

Simpson's main tenets, which were advanced in 1909, are firstly that the separation of the two electricities is brought about in a thunderstorm by a process that has been demonstrated in laboratory experiments, *viz.* that when drops of water are broken up in the air the drops acquire a positive charge and the air a negative one; that very large drops of rain are formed in the vigorous convexion that is an invariable accompaniment of thunderstorms, and that such drops break up automatically into smaller drops when their rate of fall attains to 8 metres per second; and lastly that vertical wind currents of that magnitude are developed in the most active part of a thundercloud, with the result that a great accumulation of positively charged drops is to be found in that region. Simpson sketched the stream lines for a thunderstorm of the "heat" type, with which he was mainly concerned in his observational studies and experiments in Simla, and indicated roughly the distribution of the electric charges, and the types of discharge to be expected—discharges from the region of accumulation of positive electricity near the bottom of the cloud and not far from its forward edge both upwards into the negatively charged parts of the cloud and downwards to the ground, and also from the ground to negatively charged parts of the cloud when the latter give a sufficiently intense field. Simpson's theory has been widely accepted by meteorologists. His diagrammatic representations of a thundercloud have been reproduced in many meteorological works, with the consequence that undue importance has sometimes been attached to the exact way in which the distribution of the charges is depicted, as though his theory must be judged by the extent to which this ideal distribution is found in actual thunderstorms. It is desirable to emphasise therefore that it is the separation of the two electricities by the process described in a powerful up-current that is the fundamental element in the theory.

The process suggested by Elster and Geitel whereby separation of electricities is brought about, to which reference has been made, is described as influence-machine action; it requires that the larger drops have induced charges of opposite sign on their upper and lower surfaces and that the smaller droplets carry aloft charges obtained by contact with the lower hemispheres of the falling drops. Toepler is said to prefer this theory to Simpson's "breaking drop theory," while another well-known worker on this subject—Watson-Watt—regards the influence-machine action as merely an auxiliary to Simpson's process.

In regard to the identification of a discharge of positive electricity to earth, Simpson at one time as a result of laboratory experiments held that branching of lightning was evidence of such a discharge. Jensen has, however, collected strong evidence in this paper [1] that Simpson's criterion based on the branching of the lightning is no longer strictly tenable. For example, in the high-tension laboratories of the General Electric Company branching streamers have been obtained issuing from both positive and negative electrodes. On the other hand, there is evidence to show that a flash from a seat of positive charge results in many more finely branching streamers than one from a negatively charged cloud. Jensen's photographs of lightning flashes illustrate this point, and show also examples of the more limited branching in negative discharges.

Detailed investigations of electrical phenomena accompanying individual thunderstorms in Simla that were made by Simpson, which included measurements of the charge carried by the rain in different parts of the storm, have stimulated others to make more or less similar investigations in other parts of the world. In India S. K. Banerji in two recent papers, [4] and [5], has given more complete meteorological and electrical data for thunderstorms than any previously published. Self-recording meteorological instruments were used, and records of electrical potential gradient and of the sudden field changes associated with lightning flashes were obtained when studying 41 storms near Bombay. These storms occurred under circumstances differing in some important respects from those accompanying Himalayan storms such as those studied by Simpson at Simla; for example the Bombay thunder clouds had a greater altitude above the ground and lightning was seldom seen to pass from cloud to ground. The distribution of electricity in thunderstorms of the "heat" type, as well as those of the line-squall type, differed somewhat from that suggested by Simpson, in that a powerful concentration of negative electricity is in each case assumed by Banerji to be present in the extreme front of the thunder cloud. Single and double storms were observed, the double storms having electrical phenomena generally similar to those that would be produced by two single storms following one another in quick succession. In the typical single storm the potential gradient from the ground towards the cloud was first strongly negative, but became strongly positive as the active part of the cloud came overhead; subsequently it became negative again.

C. T. R. Wilson, [6] and [7], is another worker in the same field who has made many measurements of potential gradient under thunder clouds, and has concluded that they are bipolar; but he finds that the lower pole is negative, and obtains theoretical support for his results in a consideration of the ionisation of the atmosphere.

Schonland and Craib [8] worked in South Africa on similar lines to Wilson and also found in nearly every case that the thunder cloud was bipolar, with the negative pole nearest to the ground, and Schonland, [9] and [10], extended the work with similar results. In [10], moreover, he measured the current escaping from an insulated bush to an overhead thunder cloud and found on an average a current of slightly more than two amperes, with a direction such that negative electricity passed from the cloud to earth. Schonland and Craib found that the change in potential gradient accompanying a flash of lightning changes its sign at a certain height depending on the heights of the poles of the cloud—generally at a height between 6 and 8 km. at Cape Town.

It may be noted that, with polarity of the kind implied by Simpson's diagrams, a discharge from the most active part of the cloud to the ground would necessarily produce a negative change of field, while Schonland and Craib's polarity would give a positive field change, as stated by Schonland in [9]. A similar conclusion from experimental data was reached by Simon [11] and Noto [12], supporting the field change implied by polarity with the negative pole nearest to the ground. Jensen quotes the results of a number of electrical engineers who have studied cases where power transmission lines have been struck by lightning, and states that Peek [13] found that most line surges showing very low voltages (induced) were positive, which would be the result of a negative pole in the cloud, while many of the very high voltages that must be attributed to direct lightning strokes, were negative, and consequently also indicated negative polarity in the cloud base. Lewis and Foust [14] give a summary of the results obtained by other engineers as well as by themselves, and find that in a hundred cases where transmission line towers have been struck, and records obtained, the tower-top was always negative with respect to the base, which again implies a negative discharge from the cloud to the earth. These results are confirmed by other studies of strokes to transmission lines, *e.g.* by Sporn and Lloyd [15], Smeloff and Price [16] and Fortescue [17], which seems to place them almost beyond dispute. It can be seen therefore that, just as in the question of the origin of the separation of the electrical charges in a thunder cloud, so in that of the distribution of the electricity within the cloud, there are two opposed schools of thought, both able to adduce much observational and theoretical backing for their views, and the many recent advances in the subject have not been in the direction of a simplification of our ideas. In the paper [1] on which this general survey is based the author goes on to give an account of some of his own studies of the electrical phenomena associated with thunder clouds, made at Lincoln, Nebraska, a region subject to violent thunderstorms of the line-squall type with many flashes that reach the ground. A novel feature was the use of a battery of microbarographs, which furnished much information about the dimensions and direction of travel of these storms. Confirmation was obtained of the frequent occurrence of the double type of storm noted by Banerji at Bombay. Among the many photographs of lightning were 77 showing downward branching from a negative cloud to the ground. Among more than seven thousand field changes due to lightning discharges the ratio of positive to negative field changes was found to be 1·71 to 1, which may be compared with the ratio of 1·56 to 1 found by Wilson. The result is not, however, held to support Wilson's scheme of polarity, but to be the consequence of there being a larger area of negative cloud than positive cloud in a horizontal cross section of the cloud. On the whole the results of this work support those of Banerji, especially in regard to the concentration of negative charge in the front of a thunder cloud.

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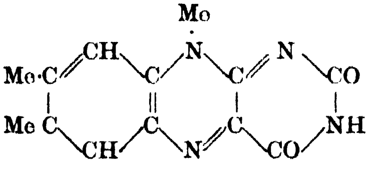
BIOCHEMISTRY.

By W. O. Kermack, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

Synthesis of Vitamin. B_2.—Possibly the most spectacular advance in biochemistry during the last six months has been the elucidation by Kuhn and Weygand (*Ber.*, 1934, **64**, 2084) of the structure of vitamin B_2 and the synthesis of a compound apparently identical in all respects with the natural product and possessing a vitamin B_2 activity when tested out biologically identical with that of the latter. As explained recently in this series of articles (Science Progress, 1934, **29**, 291) it was discovered by Kuhn that vitamin B_2 was in all probability identical with the powerfully fluorescing yellow pigment present in minute amount in the whey of milk and therefore called lactoflavin. It is only two or three years since representatives of these flavin pigments were first isolated. Though widely distributed in nature they exist for the most part only in extremely minute concentration. For example Karrer, Schopf, Benz and Pfachler (*Ber.*, 1935, **65**, 216), who have also been working intensively on this subject and who have reached conclusions substantially identical with those of Kuhn, state that they obtained 12 gm. of pigment from 110 tons of whey—that is, 1 gm. from about 10 tons or 10,000 litres. Whey is, in fact, the most readily available natural source. These pigments, too, are very unstable and cannot easily be obtained in a crystalline condition. They are destroyed by exposure to light. It is all the more remarkable therefore that within two or three years of the first isolation it should have been found possible not only to ascertain the constitution but to work out a method for their complete synthesis in the laboratory. With this achievement coming so rapidly after the synthesis by Reichstein and by Haworth and Hirst of vitamin C (ascorbic acid), it becomes clear that the present time is witnessing a revolution in the field of biochemistry and especially in that domain which is concerned with those organic compounds which occur in nature in very small concentration but which at the same time are of the utmost importance for the processes upon which vital activity depends These substances include the vitamins, hormones and enzymes.

It is not appropriate in this article on biochemistry to give a detailed account of the method worked out by Kuhn and his collaborators (cf. *Ber.*, 1934, **64,** 1932 and 1939) for the synthesis of the flavin pigments. It will be sufficient to state that the synthesis proves conclusively that vitamin B_2 has the constitution represented in formula I and that the synthesis itself of this fairly large molecule is effected in a remarkably small number of stages and in a series of relatively simple reactions. There seems no reason why vitamin B_2 should not soon be on the market in practically unlimited amounts as is the case already with vitamins D and C. The synthetic product was tested for its vitamin B_2 activity by administering it to young rats placed on a diet deficient in this vitamin. It had previously been shown by Kuhn that with a suitably chosen vitamin B_2 deficient diet the growth of young rats is retarded, but that the



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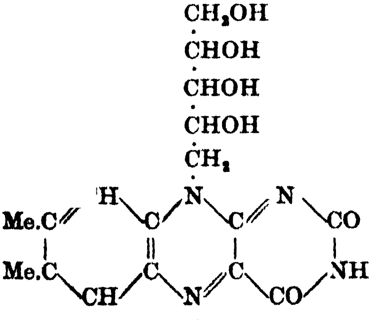

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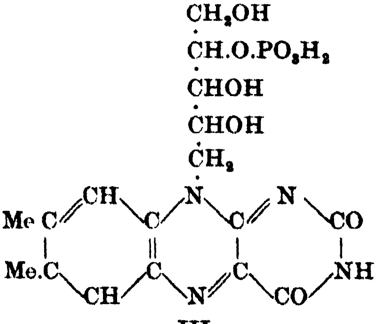


III

addition of very small quantities of the pure natural lactoflavin is sufficient to produce a marked increase of growth. The first experiments he carried out on those lines using the natural product were in fact negative but it was discovered that the addition to the diet of an unknown factor vitamin B_4, though not in itself capable of producing growth, was sufficient to make the animals respond quite unambiguously to the presence of lactoflavin. It was clear that two factors were concerned, namely vitamin B_4, still unknown, and vitamin B_2, that is, lactoflavin. The synthetic compound when administered in doses of 15γ per rat per day produced a response undistinguishable from that produced by the natural lactoflavin, when given in equal amounts. The synthetic compound, it may be mentioned, was given in the form of its crystalline tetraacetate which is presumably hydrolysed in the organism to the unacetylated compound.

The synthetic method devised by Kuhn is a very flexible one, and so the way is opened up for the preparation of compounds differing only in minor respects from the active compound mentioned above. It will be seen that the compound consists of two main parts, the *iso*-alloxazine nucleus and the sugar-like side chain. The nucleus has also two methyl groups at positions 6 and 7. It is of considerable interest to find that these two apparently rather inactive methyl groups are essential for the biological activity of the compound. Kuhn and his collaborators have already synthesised and tested out a compound identical in all respects with the active one except that the two methyl groups were missing. It was found that this compound was apparently completely inactive. The effect of variations on the long side chain attached to the nitrogen atom will be awaited with interest. The exact constitution and, in particular, the stereochemical configuration of the side chain, depends on the sugar from which it is derived. In the synthetically prepared active compound the side chain is derived from *l*-arabinose. It appears that the nature of the compound is such that by purely chemical methods it is difficult to reach a final conclusion on the question of absolute identity with natural lactoflavin. It is quite possible that only this one out of the eight possible stereoisomers will prove active, but a definite experimental decision on this point should not be long delayed. It should be made clear that until this has been done the possibility always remains open that the naturally occurring lactoflavin is not a derivative of *l*-arabinose but of some other pentose.

The action of light on lactoflavin is of considerable interest as it is this which to a large extent has contributed to the success of the work which has just been described. In alkaline solution lactoflavin when irradiated by ultra-violet light is converted into lumilactoflavin (II). It will be seen that practically all the long side chain has been split off leaving only a methyl group in its place. Lumilactoflavin in contrast to lactoflavin itself is relatively stable and can be crystallised and readily isolated. Further, by a lucky chance, it happens that in the conversion of lactoflavin into lumilactoflavin the former need not be purified or isolated, and that an almost quantitative yield of crystalline, but not completely homogenous (cf. Kuhn, Rudy and Wagner-Jauregg. *Ber.*, 1933, **66,** 1950), material is obtained even with very crude extracts. The result is that this conversion into lumilactoflavin can be applied for analytical purposes. Further, the constitution of lumilactoflavin presented a problem in no way as formidable as that of lactoflavin itself. Once however the structure of lumilactoflavin was elucidated and its synthesis was successfully accomplished the way lay open for the application of the same synthetical methods for the preparation of synthetic lactoflavin itself

Very recently a second synthesis of flavin compounds has been successfully accomplished by Karrer, Schopf, Benz and Pfachler (loc. cit.) with results substantially in agreement with those of Kuhn. Though the final stage of the synthesis by Karrer and his collaborators is practically the same as that by Kuhn, the methods employed for the preparation of the necessary intermediates are somewhat different and are said to give better yields. Karrer, like Kuhn, finds that the synthetic flavin derived from *l*-arabinose is extremely similar in its chemical properties to the naturally occurring flavin ; but both authors refrain from committing themselves to any final decision until they have prepared the other stereoisomers for comparison purposes.

Meanwhile some extremely interesting results have been obtained by Theorell (*Biochem. Zeit*, 1934, **272,** 155 : **275,** 37, 344) bearing upon the relationship of the flavin pigments to the yellow respiratory enzyme, "atmungsferment," discovered some years ago by Warburg and Christian (*Naturwiss.*, 1932, **20,** 688 ; *Biochem. Zeit.*, 1932, **254,** 438) in yeast and now known to be a common constituent of living cells. For some time it has been known that this yellow respiratory enzyme contained as an essential part of its structure a relatively simple, strongly fluorescing yellow pigment which could be separated from a protein constituent. The latter was probably the carrier of the active group, and as the yellow pigment showed very close similarities to lactoflavin it was strongly suspected that it was identical with the latter. Theorell has been able to purify the enzyme by a process of electrocataphoresis and then, by dialysis against HCl, succeeded in breaking up this purified enzyme into two component parts, protein and fluorescing pigment, in such a way that, though each fraction separately showed little or no enzymatic activity, the two could be recombined to reconstitute the enzyme. The protein portion was apparently specific as it could not be replaced by other proteins such as native globin. Furthermore it was found that if the fluorescing pigment, separated from the enzyme, was replaced by purified lactoflavin obtained from whey, recombination with the protein fraction of the enzyme could not be effected. This proved definitely that the pigment moiety of the yellow respiratory enzyme was not identical with lactoflavin. Theorell also made the important observation that the pigment obtained from the enzyme contained phosphorus which could be split off without difficulty as phosphoric acid and that there was one molecule of phosphorus to each molecule of the pigment Subsequently methods were described for the isolation of the new phosphoric acid-containing pigment from the enzyme. It may be isolated either as the calcium salt or as the free acid. The pigment thus isolated is probably a phosphoric ester of lactoflavin, and it differs from the latter in that it can combine with the specific protein of the respiratory enzyme to produce the active enzyme. The point of attachment of the phosphoric acid is at present uncertain but it is probably combined with one of the hydroxyl groups of the side chain. In this case it would have the constitution III where the hydroxyl group to which the phosphoric acid is attached has been chosen somewhat arbitrarily. Theorell raises the question as to whether the new flavin phosphoric acid should not be regarded as a co-enzyme of oxydation rather than as the enzyme itself. He points out that it has all the properties which distinguish a co-enzyme from an enzyme. It is of small molecular weight, dialysable and stable to boiling, and there is a certain similarity to the typical co-enzyme adenosine phosphoric acid which is now well known as the co-enzyme of yeast fermentation. In this case the specific protein would presumably correspond to the enzyme itself. The Willstatter school has tended to regard an enzyme as consisting of an active or " prosthetic " group accompanied by a protein carrier. It seemed natural to assume that in the yellow respiratory enzyme of Warburg the flavin pigment corresponded to the prosthetic group. It now looks as if it corresponded rather to the co-enzyme. It may be that the distinction between prosthetic group and co-enzyme is no hard and fast one. Some prosthetic or active groups may dissociate themselves more easily from the protein than others and those which happen to be easily and reversibly dissociable and to have been isolated may be the compounds which we call coenzymes. This would at least seem to be a working hypothesis consistent with the known facts and forcibly suggested by this new and important discovery by Theorell of the presence of a flavin phosphoric acid in the yellow respiratory enzyme of Warburg and Christian.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., The University, Glasgow.

Igneous Activity and Igneous Rocks.—In addition to the recent British and Swedish collections of analyses (Science Progress, Oct. 1932, p. 212) we have now a collection from Finland. This has been compiled by L. Lokka (*Bull. Comm. Géol. Finlande*, No. 105, 1934, 65 pp.) and contains 312 analyses of Finnish rocks which have been made since 1905, 66 of which, made by Lokka himself, have not yet been published elsewhere.

Professor A. Lacroix has seized the occasion of the publication of his work on the igneous rocks of Indo-China to set forth his long-awaited scheme of classification of igneous rocks (*Bull. Serv. Géol. de l'Indochine*, XX, Fasc. 3, Hanoi, 1933, 15–36 ; 183–206) which has been distributed as a separate pamphlet. While it is impossible here even to indicate the main lines of the classification we may say that it is chemico-mineralogical and based on a norm calculation. In that point it differs from Niggli's (Science Progress, Oct. 1932, p. 211), but as they work out the correspondences of the two methods of classification seem to be considerable.

In a penetrating paper entitled, " The Broader Story of Magmatic Differentiation, Briefly Told," Dr. N. L. Bowen (*Ore Deposits of the Western States*, *Amer. Inst. Min. Metall. Eng.*, 1933, 106–28) shows that the trend of fractional crystallisation in igneous magma is toward alkaline aqueous residual solutions, and that under moderately deep-seated conditions this residual liquid boils, setting up a fractional-distillation column in the interstices and fractures of the intrusion and its surroundings. By this action an acid aqueous solution is formed, and it is this which is regarded as the principal ore-bringer.

In his Presidential Address to the Geological Society of Washington, Dr. C. N. Fenner (*Journ. Wash. Acad. Sci.*, **24**, 1934, 113-24) discusses " Some Magmatic Problems," and in particular, the large-scale assimilation of basaltic rock by rhyolitic magma in Alaska and the Yellowstone Park. Unless there are immense unrecognised sources of heat supply in rhyolite magma these facts are completely at variance with the accepted theory of crystallisation-differentiation, under which rhyolite should be the coolest member of the series of partial magmas from basaltic to rhyolitic.

With the aid of a study of the external factors which influence the crystallisation of a hydrous melt in the system K_2SiO_3 — SiO_2 — H_2O, Dr. T. C. Phemister (*Min. u. Petr. Mitt*, **45**, 1934, 19–77; 99–132) has attempted to determine the probable influence of water on the course of crystallisation of basaltic magma. The concentration of dissolved water in basaltic magmas at plutonic depths is from 4 to 5 per cent., and for less deep-seated magma the concentration is diminished. The effect of dissolved water on the mineralogy of basalts, and its control over assimilation and differentiation, are fully discussed in this important memoir.

In a paper entitled " Pneumatolytic Processes in the Formation of Minerals and Ores," Dr. C. N. Fenner (*Ore Deposits of the Western States*, *Amer. Inst. Min. Metall. Eng.*, 1933, 58–106) discusses " the manner in which gases may be evolved and residual solutions formed from magmas, their different behaviour and composition, and the results they may be expected to accomplish especially in the collection, transportation and deposition of ores." He believes that " pneumatolytic processes are capable of effecting important metamorphic and metasomatic results . . . by adding or subtracting ingredients and by bringing about recrystallisation, and that some of the effects commonly ascribed to orthomagmatic crystallisation or to hydrothermal solutions have actually been brought about by pneumatolysis."

A thorough investigation of the alkali-trachyte of Solfatara (Naples) and its alteration by fumarole gases has been carried out by A. Rittmann (*Schweiz. Min. u. Petr. Mitt.* XIV, 1934, 1–21).

In a discussion of " Correlation of Kinds of Igneous Rocks with Kinds of Mineralization," A. F. Buddington (*Ore Deposits of the Western States. Amer. Inst. Min. Metall. Eng.*, 1933, 350–85) deals with the criteria for determining the igneous mass genetically associated with mineralisation, metallogenetic provinces and the variations of primary magma, assimilation of wall rocks, and variations in mineralisation due to the tectonic and cooling history of magma. These points are illustrated by a summary of the mineral deposits of the United States which are found in association with various kinds of igneous rocks.

Dr. C. S. Hitchen has studied the granite, greisen and mineral veins of the Skiddaw Granite from the point of view that each of the mineral associations are integral phases of one igneous episode (*Quart. Journ. Geol. Soc.*, XC, 1934, 158–200). The orthomagmatic, pegmatitic and hydrothermal phases of residual igneous activity, but not the pneumatolytic phase of Niggli's scheme, are shown to have occurred. Several new chemical analyses are published.

Discussing the "Genesis of the North Pennine Ore Deposits," K. C. Dunham (*Quart. Journ. Geol. Soc.*, XC, 1934, 689–720) comes to the conclusion that their zonal distribution and richness in fluorite provide definite evidence of derivation from a magmatic source, possibly granitic, situated beneath the Pennines.

The conclusion of F. Huttenlocher's comprehensive study of "Die Erzlagerstättenzonen der Westalpen" (*Schweiz, Min. u. Petr. Mitt.* XIV, 1934, 22–149) is that, apart from isolated examples of Hercynian ore deposition in the northern autochthonous massifs, the whole of the metallisation of the Western Alps is associated with the main Alpine orogeny, and offers a typical example of tectonic-orogenetic ore deposition.

A valuable summary of the characters of "Die magmatische Mineral- und Gesteins-provinz im Mitteldevon des Lahn-Dillgebietes" has been published by E. Lehmann (*Zeitschr. d. Deutsch. Geol. Ges.*, **86**, 1934, 348–59). The province consists of keratophyres and spilites with two kinds of schalstein, magmatic and tuffogenous. The associated iron-ore deposits are very closely connected with magmatic processes.

By plotting spilite and metabasalt analyses on two kinds of triangular diagrams H. W. Fairbairn concludes that spilite is chemically transitional to low-soda metabasalt, and that the conception of an independent spilitic magma is not justified (*Amer. Journ. Sci.*, XXVII, 1934, 92–7).

An interesting series of spilite and keratophyre lavas with associated tuffs, probably of Pre-Cambrian age, is described by G. F. Flaherty (*Journ. Geol.*, XLII, 1934, 785–808) from south-eastern New Brunswick. It is considered that the albite of the spilites is secondary, and that albitisation was due to soda-rich solutions generated within the flows themselves during their consolidation. Hence the spilitic magma is thought to have been inherently rich in soda.

A valuable discussion of the process of prehnitisation is given by P. Eskola in his description of a "Prehnite Amygdaloid from the Bottom of the Baltic" (*C.-R. Soc. Géol. de Finlande*, No. 8, 1934, 1–13). The alteration is regarded as "autohydrothermal metasomatism," the reacting solutions being derived from the lavas themselves.

Sir J. S. Flett describes an interesting thomsomitised inclusion from the Blackness teschenite sill, East Lothian (*Summ. Prog. Geol. Surv. for* **1933**, Pt. II, 1934, 85–90). The inclusion is of the nature of a calc-silicate-hornfels composed of thomsonite, garnet and pyroxene. Rounded areas within the rock are believed to represent original steam cavities. The inclusion may be a contact-metamorphosed fragment of the Houston Marl, and the thomsonite may have arisen by pneumatolysis of basic plagioclase.

S. I. Tomkeieff describes an excellent example of gravitative differentiation in the basaltic lavas of Island Magee, co. Antrim (*Geol. Mag.*, LXXI, 1934, 501–12). Olivine-rich basalt occurs at the bases, grading to normal olivine-basalt in the central parts of the thicker flows. The differentiation is ascribed to the sinking of olivine crystals without re-melting. The accompanying rise of volatiles accounts for the abundance of zeolites at the tops of the flows.

Field and laboratory investigation of the rocks at the eastern end of the Newry Igneous Complex has led Miss D. L. Reynolds (*Quart. Journ. Geol. Soc.*, XC, 1934, 585–636) to some remarkable conclusions. The different rock types are regarded as having been derived from ultrabasic magmas which differentially fused the adjacent Silurian greywackes and shales and, by " soaking " into the fused sediments, have given rise to augite-monzonite. Hybridisation of the ultrabasic magmas by a somewhat hypothetical " plagioclase magma " gave rise to augite-biotite-diorite. The granodiorite which forms the bulk of the intrusion rose by stoping through the earlier members of the complex, and is itself regarded as possibly due to deep-seated mixing of biotite-pyroxenite, " plagioclase magma," and sediments.

These observations have led to the formulation of a comprehensive theory of the origin of igneous rocks in general, in which extensive assimilation with biotite-pyroxenite as parent magma plays the main part. So far this theory has been presented only in abstract (*Geol. Mag.*, LXXI, 1934, 479–80), and will be elaborated, with illustrative examples, in a further paper.

The " Marginal and Contact Phenomena of the Dorback Granite," east of Tomintoul, have been described by F. Walker and C. F. Davidson (*Geol. Mag.*, LXXII, 1935, 49–63). To some extent the phenomena appear to be similar to those described by Miss Reynolds from Newry (see above) The mass includes marginal monzonites and diorites the mineralogical features of which point to the absorption of lime from limestone contacts and xenoliths. An association of relatively rare sharp contacts and more common merging junctions with marginal hybridisation, is noted (cf. Anderson's Ben Nevis work, below).

C. B. Bisset describes some granite masses to the east of Aberdeen with the aid of three new analyses (*Trans. Edin. Geol. Soc.*, XIII, Pt. 1, 1934, 72–88). These comprise an earlier group forming the Skene Complex, consisting of diorite, porphyritic adamellite, grey granites and minor intrusions; and a later group consisting of the coarse red granites of Bennachie and Hill of Fare. Both groups belong to the Newer or Post-Silurian Granites.

Mr. Bisset has also dealt with a series of minor intrusions which occur within the Skene Complex (*Trans. Edin. Geol. Soc.*, XIII, Pt. 1, 1934, 133–47) with the aid of four new analyses. The rocks comprise felsites and lamprophyres, and are believed to have been derived from a residual magma of the Skene granite.

J. G. C. Anderson has published some important work on "The Marginal Intrusions of Ben Nevis; the Coille Lianachain Complex; and the Ben Nevis Dyke Swarm" (*Trans. Geol. Soc. Glasgow*, XIX, Pt. 2, 1935, 225–69), in which he shows that the non-porphyritic margin of the Outer Granite of Ben Nevis consists of three interrupted arcuate intrusions of fine, medium and coarse textures respectively, which are everywhere arranged inward from the margin in the above order. This order is also the order of age, but the successive intrusions followed one another so closely that merging junctions often occur. Five new chemical analyses accompany the paper. A summary of results is given in a paper on "The Outer Granite of Ben Nevis" (*Quarry Managers' Journ.*, Mar. 1934, 4 pp., reprint).

Mr. Anderson has also written a useful summary account of "Caledonian Igneous Activity in the South-West Highlands" (*Quarry Managers' Journ.*, Jan. 1935, 6 pp., reprint) describing the plutonic masses of that region, and giving a preliminary notice of new work on the Arrochar (Loch Long) series of intrusions.

A small area of tonalite on the margin of the Cruachan Granite is extremely rich in xenoliths of the argillaceous and calcareous schists with which it comes into contact. Dr. S. R. Nockolds has made a close study of the resulting contamination of the igneous rock, and has dealt with some general considerations bearing on these reactions (*Quart. Journ. Geol. Soc.*, XC, 1934, 302–21).

The petrography and petrogenesis of "The Mountsorrel Granodiorite and Associated Igneous Rocks" have been described by J. H. Taylor (*Geol. Mag.*, LXXI, 1934, 1–16). The rocks, of which three new analyses are given, range from gabbro to soda-aplogranite. An intermediate diorite member is thought to represent a hybrid formed by the action of acid magma on basic rocks.

Dr. P. K. Ghosh has dealt with "The Carnmenellis Granite: Its Petrology, Metamorphism and Tectonics" (*Quart. Journ. Geol.*

Soc., XC, 1934, 240–76) using for the tectonic investigation the now well-known methods of H. Cloos. Numerous new analyses of the granites, elvans and hornfels, are given.

D. Balsillie has described the petrology of the intrusive igneous rocks of South Angus, mostly within a 10-miles radius of Dundee (*Trans. Perthshire Soc. Nat. Sci.*, IX, Pt. 4, 1934, 133–45). Quartz-dolerites, basalts, augite-porphyrites, andesites and felsites are described. Two of the most interesting points are the great display of coarse and fine acid veins in the Hilltown of Knapp quartz-dolerite, and the widespread albitisation of the rocks.

R. Campbell and A. G. Stenhouse record nepheline and fayalite in the trachyte of the Bass Rock, and thus confirm the phonolitic character of this intrusion which has been indicated by other observers (*Trans. Edin. Geol. Soc.*, XIII, Pt. 1, 1934, 126–32).

The second part of the memoir on the Braefoot Outer Sill, Fife, by R. Campbell, T. C. Day, and A. G. Stenhouse (see SCIENCE PROGRESS, April 1934, 692) deals with the continuation of the sill in districts other than the Braefoot promontory (*Trans. Geol. Soc. Edin.*, XIII, Pt. 1, 1934, 148–72). A summary of the characters of the whole intrusion is now given, leading to the conclusion that, with the exception of an acidified dolerite at the top of the sill, the various facies have resulted from differentiation *in situ* of an olivine-basalt magma.

Dr. F. Walker has written an interesting account of the little-known quartz-dolerite dikes of Perthshire (*Trans. and Proc. Perthshire Soc. Nat. Sci.*, IX, Pt. 4, 1934, 109–17). These occur in the southern half of the county only. Most of the dikes are of tholeiitic character, but the pink acid veins of similar intrusions in the Scottish Midlands are scarce in the Perthshire area.

According to O. Burre (*Zeitschr. d. Deutsch. Geol. Ges.*, **86**, 1934, 100–10) Tertiary volcanicity in the Siebengebirge and its surroundings began in the Lower Miocene with trachyte-tuffs and trachytic lavas. The following andesites (? trachyandesites) and basalts (? trachybasalts) of Upper Miocene age covered a much wider area than the trachytes. The vents are disposed along a S.E. to N.W. line which coincides with the direction of the Tertiary fault system in the Rhenish Schiefergebirge.

The volcanicity of the Lower Carboniferous in the Variscian Mountain system of Central Germany, according to K. Gundlach (*Abh. d. Preuss. Geol. Landes. N. F.*, **157**, 1933, 59 pp.), gave rise to pillow-lavas (diabases) of submarine eruption, followed by quartz-keratophyre lavas and tuffs. Compared with the somewhat similar lavas of the Middle Devonian these rocks are less amygdaloidal and less tuffaceous, indicating that they were relatively gas-poor. Tectonically they seem to be connected with transition-forms between basins and uplifts (Becken und Schwellen).

Feldspathic yamaskite and jacupirangite, which occur as plutonic inclusions in a gauteite dike at Aussig in the Bohemian Mittelgebirge, are described by E. Tröger as of cognate origin (*Min. u. Petr. Mitt.*, **45**, 1934,, 209–24).

The paper by K. Preclik, " Zur Kenntnis der chemischen Zusammensetzung der moravischen Erstarrungsgesteine " (*Min. u. Petr. Mitt.*, **45**, 1934, 269–332) is a valuable collection and comparative study of analyses of Pre-Devonian, Devonian and Carboniferous igneous massifs in Moravia, which are mainly of calc-alkalic characters. A comparison with Moldanubian plutonic rocks is instituted. The Cistà-Jechnitz granite massif in Bohemia, according to A. Orlov (*Min. u. Petr. Mitt.*, **44**, 1933, 205–10), is composed of acid granite and oligoclase-granodiorite forming successive differentiates from the same magma. This plutonic mass shows certain affinities with the Erzgebirge granites, but is strikingly different from the main facies of the Mid-Bohemian granite massif.

Gabbroidal dike-rocks from the Brissago district on L. Maggiore are described by C. Burri and F. de Quervain (*Schweiz. Min. u. Petr. Mitt.*, XIV, 1934, 507–17). These rocks are associated with the basic intrusions of the Ivrea Zone. An analysis of an anorthite-bearing type is given.

A compilative account of " Les formations volcaniques de la Sardaigne," supplemented by some new work, and by a good collection of analyses and a bibliography, has been given by J. A. Burford (*Schweiz. Min. u. Petr. Mitt.*, XIII, 1933, 366–96).

Professor V. Hackman has published a comprehensive work entitled, " Das Rapakiwirandgebiet der Gegend von Lappeenranta (Willmanstrand) " (*Bull. Comm. Géol. de Finlande*, No. 106, 1934, 82 pp.). This is a marginal region, 500 sq. miles in area, of the great Wiborg rapakiwi granite massif. The concluding section of the memoir exhaustively discusses the mechanism of intrusion of the granite. It is concluded that epeirogenic movements have accompanied and caused the emplacement of the magma, tangential orogenic forces not having come into play at all. Hackman inclines to the view that the Wiborg massif is a transgressive bottomless batholith, and not an intrusive sheet of comparatively small thickness, a view which is supported to some extent by gravity observations.

The " Bottenmeerporphyr," described by Professor P. Eskola (*C.R. Soc. Géol. de Finlande*, No. 8, 1934, 111–27), consists mainly of "albite-felsite-porphyry," and is relegated to the "Helsinkitic" mineral facies. It is found as boulders on the Finnish shores of the Bothnian Gulf, and is believed to be derived from submarine outcrops.

A study of the forms, mechanics of intrusion, and the petrology of the late-Mesozoic dolerites of Spitsbergen has been made by Dr. K. S. Sandford and the writer (*Proc Roy. Soc. Edin.*, LIII, Pt. 3, 1933, 284–321). In the Carboniferous and Permian strata the sills are uniform, non-transgressive and of wide extension; but in the Mesozoic formations they ramify as a network through the broken strata. This contrast is regarded as due to injection under deep and shallow covers respectively.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

Ecological.—Comparatively little actual data is available respecting the internal carriage of seeds by animals, so that the recent observations of P. Müller (*Berichte d. Schweiz. Bot. Ges.* **43,** 241, 1934) are of considerable interest. He finds that achenes of the Wild Strawberry, *Fragaria vesca,* from the excrement of the snail *Arion empiricornum* yielded no less than 94 per cent. of seedlings as compared with 74 per cent. with achenes direct from the plant. Seeds of *Vaccinium myrtillus* showed 89 per cent. germination, those from the snail excrement 81 per cent. Fresh seeds of *Ribes alpinum*, which showed 99 per cent. germination, did not germinate so rapidly as seeds which had passed through the gut of the Blackbird. The latter sown in autumn gave 98 per cent. germination by the following January, when only 83 per cent. of the fresh seeds had germinated. Cranberry seeds from the droppings of the same bird showed 74 per cent. germination, as compared with only 40 per cent. germination of seeds which had not passed through the bird.

An investigation of the symbiotic relations of *Monotropa hypopitys* is the subject of a detailed study by H. L. Francke (*Flora,* **129,** 1, 1935). He concludes that the symbiosis with the fungus is an obligate one, since sterile seeds fail to germinate, although germination, albeit a low one (0·3 to 0·5 per cent.) is obtained in the presence of the fungus. The development of haustoria in the host attains a maximum in September and October, the period of most vigorous growth of the host, and is least from December to April. The fungus is probably a *Boletus*, and appears to attain optimum conditions at a reaction of about *p*H 5. The seeds, which weigh only 0·003 milligram, contain an endosperm of only nine cells and an embryo of only three cells.

Attention should be called to two recent accounts of vegetation both of which contain valuable data, but not of a character that could be usefully summarised in brief. One is a study by Waser, Husmann and Blöchliger of the chemical, bacteriological and biological features of the River Glatt. This contains data regarding the variations in the nature of the river water from stations over a length of some 36 kilometres, together with details of the animal and plant life (*Berichte d. Schw. Bet. Ges.*, **43**, 253–388). The second is an account by E. Barre of the vegetation of Madagascar (*Annales des Sci. Nat.*, XVI, 95, 1935) in which the various communities are described and a general map is furnished.

In the October and November numbers of the *New Phytologist* (XXXIII, 1934), Dr. Godwin contributes a useful summary of the technique and interpretation of the methods of pollen analysis in peats in which he rightly stresses the difficulty of establishing direct parallels between the stages of forest development on the Continent and those in Britain.

FLORAL MORPHOLOGY.—The occurrence of male flowers on female plants of *Mercurialis annua*, and of female flowers on male plants, is well known, as also the occurrence of hermaphrodite flowers of diverse types. Yampolsky describes (*Amer. Jour. Bot.*, **21**, 651, 1934) some remarkable flowers of this species, in which carpels bore fertile anthers and others in which fertile stamens developed stigmas, besides which various other types of transformation into carpels were encountered. In some examples both staminody and pistillody occurred within the same flower. The examples described indicate that what may be termed the drift of differentiation towards carpel or stamen formation may proceed apparently almost to completion, and yet permit partial or complete differentiation in another direction. Such abnormalities, of which similar examples have been recorded from the Ranunculaceae and other families, must clearly be taken into consideration in arriving at any generalised concept of the morphogenetic processes involved in floral differentiation.

ANATOMY.—The histology of the wood in the trunk proper, the swollen stem base, and the knee roots of *Nyssa aquatica*, has been studied by Penfound (*Amer. Jour. Bot.*, **21**, 623, 1934). The most marked distinction between elements in the three regions was shown by the structure of the fibres, which were shortest, widest and thinnest-walled in the knee-roots and increased in length with increase in height above the ground of the wood from which they are derived. In the trunk the fibres were both longest and thickest-walled whilst in the swollen basal region, which may attain more than three times the diameter of the stem above, the length and wall thickness are intermediate, the vessels are both shorter and broader in the knee-roots than in the stem.

From a study of the distribution and development of ray tissue in the secondary system of Gymnosperms, Bannan (*Bot. Gaz.*, **96**, 210) concludes that the primitive ray was parenchymatous, and that ray tracheids have arisen at the expense of parenchyma cells.

CYTOLOGY AND GENETICS.—An extensive investigation of the chromosomes of species of the genus *Iris* is the subject of a paper by M. Simonet (*Ann. des Sci. Nat.*, XVI, 229, 1934). No less than twenty-two haploid numbers have now been recorded for different species. These include the numbers from 8 to 24, 36 and 42, whilst 41–2, 43–4, 54–6, characterise other species. A noteworthy feature upon which the author lays stress is that the well-defined and homogenous sections are characterised by a uniform haploid number, whereas the ill-defined taxonomic groups exhibit great variation in respect to the haploid number. The average size of the chromosomes is inversely proportional to the number. In some species even different varieties possess different haploid numbers of chromosomes.

From a cytological and morphological study of the North American roses of the group Cinnamomae, Miss Erlanson has reduced the 113 species that have been described to sixteen Linnean but very variable species. Seven ecotype species are also tentatively recognised and four "species" due to hybridisation (*Bot. Gaz.*, **96**, 197–259, 1934).

CRYPTOGAMIC.—The presence of Water Moulds (Saprolegniaceæ) in the soils of California has been known for some years. W. R. Ivimy-Cook and E. Morgan have now found them to be present in various soils from Glamorganshire. They record twenty-six species belonging to six genera and, from the number and frequency of species in the soil, together with the fact that fifteen of these have not been so far recorded from water samples, suggest that the group may in reality be primarily a terrestrial one, the frequency in aquatic conditions being due to a washing out from the soil (*Jour. Bot.*, LXXII, 345, 1935). It should, however, be noted that the authors do in fact establish aquatic conditions for developing these fungi from the soil, so that evidence of their active growth and reproduction in the soil itself will be awaited with interest.

Experiments by C. Potts on "Finger and Toe" disease confirm the view that acidity encourages infection by *Plasmodiophora brassicæ* whilst alkalinity checks it. Repeated dressings with quicklime of the order of a ton per acre completely prevented infection. Calcium as such also appears to have an inhibiting effect, but the disease can nevertheless occur in soils containing a high proportion of chalk provided the soil be sufficiently moist. The favourable influence of organic matter is attributed solely to its effect upon the moisture relations (*Trans. British Mycol. Soc.*, XIX, 174, 1935).

In the same Journal (p. 145) F. N. Carter records species belonging to some thirty different genera of fungi as occurring in the air over apple orchards, of which about half are known to be associated with disease in stored apple fruit. Evidence is afforded that the disease of stored fruit is chiefly due to infection from the air before the apples are gathered, and in this respect different localities would appear to present marked differences.

PLANT PHYSIOLOGY.

By Professor Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Permeability.—A number of publications dealing with the permeability of plant cells have appeared during the last two years, of which only some of the more important are mentioned below.

B. Huber and H. Schmidt have published a preliminary statement regarding the influence of plasmolysis on permeability, the method consisting in determining the increase in the osmotic value of the epidermal cells of *Rhœo discolor* and other material, after a certain time, in solutions of glycerol and urea of different concentrations. They report ("Plasmolyse und Permeabilität," *Protoplasma*, **20**, 203–8, 1933) that in many objects, including epidermal cells of *Rhœo discolor*, the permeability of the cells is scarcely affected by plasmolysis, in others there is, as previously suggested by Ruhland, a definite decrease in permeability as a result of plasmolysis ; this is the case in the epidermal cells of the stem of *Gentiana macrophylla*. Some other points brought out by their investigation are that the permeability of different plant cells to urea may be very different, and may be very different for different tissues of the same plant, as, for example, the epidermal and hypodermal cells of *Gentiana macrophylla*, and also that the order of permeability of urea and other substances, particularly glycerol, is not necessarily the same in all cases.

Some observations on plasmolysis and permeability of the epidermal cells of *Rhœo discolor* have also been recently described by R. Collander ("Plasmolytische Beobachtungen an den Epidermiszellen von Rhœo discolor," *Protoplasma*, **21**, 226–34, 1934). He records that when a strip of the epidermis of *Rhœo discolor* is transferred from a plasmolysing solution of sucrose + ethyl alcohol to a sucrose solution without alcohol the protoplast moves from one end of the cell to the opposite side, and if the tissue is then transferred back to the alcohol-containing sucrose solution the protoplast moves back. This behaviour is probably related in some way to osmotic processes in the cell.

There has been some controversy as to whether cells are more permeable to water than to alcohol. Collander believes that the epidermal cells of *Rhœo* are less permeable to alcohol than to water, for a notable rapid contraction of the protoplast can be observed when the cells are placed in strong alcohol solution, indicating an osmotic withdrawal of water. But it is difficult to distinguish between an osmotic withdrawal of water depending on the concentration of alcohol outside the cell and an exosmosis due to the breaking down of the plasmatic layer as a result of the action of alcohol.

The permeability of the cells of *Chara ceratophylla* for non-electrolytes forms the subject of a paper by R. Collander and H. Bärlund (" Permeabilitätsstudien an Chara ceratophylla. II. Die Permeabilität für Nichtelektrolyte," *Acta Bot. Fennica*, **11**, 114 pp., 1933). A number of organic compounds were used in their experiments; these substances included, among others, various alcohols, amides and urea derivatives. The entry of the substances into the cells was followed by microchemical analysis of the cell sap. With these non-electrolytes equilibrium in intake occurs with equality of concentration inside and outside the *Chara* cells, and the entrance of the substance follows Fick's law of diffusion. This being so, a value for the permeability of the limiting layers of the cells for a substance is given by the quantity P in the equation

$$\frac{dc}{dt} = \mathrm{PA}\left(\frac{\mathrm{C} - c}{v}\right)$$

where v is the volume of the cell, $\frac{\mathrm{C}}{v}$ the concentration of the external solution, supposed to remain constant and therefore equal to the internal concentration at equilibrium, $\frac{c}{v}$ the internal concentration at any time, $\frac{dc}{dt}$ the rate of entry of the substance at that time and A the area of the cell surface. On integration this equation becomes

$$\mathrm{P} = \frac{v}{\mathrm{A}t} \log \frac{\mathrm{C}}{\mathrm{C} - c}$$

The permeability of the cell wall appears to be so great in comparison with that of the plasmatic layer, that the determined permeability values can be regarded as referring to the protoplasm. Some idea of the range in penetrability of different substances may be gathered from a few of the values found by Collander and Bärlund. Thus the value of P for methyl alcohol was found to be of the order 0·99, that for formamide 0·077, that for urea 0·0040 and that for glucose and some other sugars less than 0·00003. It may be noted that in all cases the permeability to each substance was the same in both directions, that is, whether the substance was passing into, or out from, the cell. Comparing the properties of the various substances with their penetrabilities, it would appear that adsorption processes cannot play any considerable part in determining permeability. On the other hand, there appears to be a direct proportionality between the penetrability for the various substances and their partition coefficients in the system olive oil—water, a strong piece of evidence in favour of the lipoid theory of cell permeability. But substances with a low molecular refraction, that is, with molecules possessing a small volume, penetrate into the cells more rapidly than their relative solubility in olive oil would suggest. The plasma-membrane would thus seem to act as an ultra-filter through the pores of which particles with a diameter of less than about 0 4 $\mu\mu$ can pass whether they are soluble in the lipoid phase or not.

While the results of this work on *Chara* are in the main in agreement with earlier work on the permeability of epidermal cells of *Rhœo*, the cells of the former appear to be more permeable to amides than the latter. This difference is attributed to a difference in the lipoids in the plasmatic layers of the two kinds of cells, the *Chara* lipoids possessing a greater solution capacity for weak basic compounds.

Another comparative investigation on the permeability of a variety of organic substances, in this case on the cells of a liverwort (*Hookeria lucens*), an alga (*Hydrodictyon utriculatum*) and a fungus (*Basidiobolus ranarum*) has been attempted by Helene Bonte (*Protoplasma*, **22**, 209–42, 1934). Her method of determining permeability was to measure the molecular concentration of each substance necessary just to produce plasmolysis and compare it with the concentration of a non-penetrating substance, sucrose, which just produced plasmolysis. In some cases, where the rate of plasmolysis by sucrose was slow, Bärlund's partial pressure method was used, in which the cells are first immersed in a non-plasmolysing solution of sucrose and then transferred to a solution containing the same concentration of sucrose with a certain amount of the substance under investigation added. From the measurements made permeability coefficients were calculated according to the formula μ (the coefficient) $= 1 - \frac{k}{k'}$ where k is the limiting plasmolytic concentration of the non-penetrating substance and k' that of the penetrating substance. It must be confessed it is rather remarkable that at this date an attempt should be made to evaluate permeability in terms of these "coefficients" Many years ago I pointed out that such "coefficients" could not give an exact value of permeability since the observed limiting plasmolytic concentration of the penetrating substance must depend on the time which elapses between immersion of the tissue in the plasmolyte and the moment of observation, for with continued entrance of the substance the osmotic pressure of the cells continually rises. However, we may assume the results obtained to give some evidence of the different degrees of penetrability of the various substances examined. These substances included, with sucrose, 23 organic compounds among which were methyl and ethyl alcohols, methyl and ethyl urethanes, urea, glycerol and some amino acids. The permeability of the various substances is considered in relation to their lipoid solubility, of which a measure is supposed to be given by the partition coefficient between ether and water. It is concluded that substances insoluble, or slightly soluble, in lipoids enter the cells in relation to their molecular dimensions, whereas the rate of entry of lipoid-soluble substances into the cells is greater than that corresponding to their molecular dimensions. Certain complications in the action of some of the substances on the different materials used are noted. Thus a plasmolytic-like contraction of the protoplast in *Hydrodictyon* attributed to changes in the adhesion of protoplasm to cell wall was observed, while in the same material methylurethane renders the cells unplasmolysable owing to the cell wall and protoplasm adhering together. Again, the plasmolysis of the mycelium cells of *Basidiobolus* is increasingly hindered with increasing acidity.

An observed difference in the staining of the stomatal guard cells and the epidermal cells of the leaves of the daisy (*Bellis perennis*) is recorded by F. Weber ("Zur Permeabilität der Schliesszellen," *Protoplasma*, **19**, 452–4, 1933). When a piece of the lower epidermis is treated with a solution containing both neutral red and methyl green, the cell sap of the guard cells becomes stained with neutral red, while the ordinary epidermal cells are stained with methyl green. This difference in staining is attributed to a difference in the permeability of the two kinds of cells, although there is also a possibility that the permeability of the epidermal cells was altered in preparing the material while that of the guard cells was not.

The last paper to which reference will be made here is one by T. Laine ("On the Absorption of Electrolytes by the Cut Roots of Plants and the Chemistry of Plant Exudation Sap," *Acta Bot. Fennica*, **16**, 64 pp., 1934), who worked with the scarlet runner (*Phaseolus multiflorus* var. *coccineus*). The amount of potassium, calcium and manganese in the sap of the cut roots immersed in solutions of potassium chloride, calcium chloride and manganese chloride respectively was found in each case to be given by the equation

$$s = kc^{\frac{1}{n}}$$

where s is the concentration of the element in the sap, c the concentration of the external solution and k and n are constants. The accumulation of both manganese and thallium in the cell sap was examined and was found to obey the same rule. The relationship between internal and external concentrations in the case of the roots used by Laine is thus similar to that recorded by Stiles and Kidd for storage tissue sixteen years ago.

ZOOLOGY.

By G. R. de Beer, M A , D.Sc.; E. B. Ford, M.A., B.Sc.; and J. A. Moy-Thomas, B.A.; the University, Oxford.

General.—A phenomenon of considerable interest has been discovered and described by G. A. Schmidt (*Zool. Jahrb. Abt. Anat.*, **58**, 1934, 607). He has found that in addition to the normal process of development by means of a Desor's larva, the Nemertine *Lineus ruber* in the White Sea and in the English Channel may undergo a different type of development. In the Desor type, the eggs, ·5 mm. in diameter, are laid about six in a capsule, with soft membranes. All the eggs develop, at the expense of the contained yolk, into inert embryos, with a thick epidermis and a small blastopore. In the second type the eggs are smaller, about ·25 mm. in diameter, contain little yolk, and are laid about fifteen in a capsule surrounded by stiff membranes. Only some 30 per cent. of these eggs develop, and they bodily ingest their brethren which have remained at early cleavage stages. They give rise to highly mobile embryos with a thin epidermis and a large blastopore.

Both types of development apparently produce the same morphological end result, the typical *Lineus ruber*, differing however in colour, for the Desor type of development is followed by eggs laid by green adults, while red adults lay eggs which develop according to the second type. There is therefore a divergence in the species consequent upon a variation affecting early stages of development.

A. F. W. Hughes has contributed a study on the development of the blood vessels of the head of the chick, which is remarkable alike for its completeness, the beauty of its illustrations, and for the inferences which the author has been able to draw from a comparative study with a bearing on the causal analysis of blood-vessel development (*Phil. Trans. Roy. Soc. Lond.*, B, **224**, 1934, 75, Plates 1–16).

In general, blood-vessels are preceded by a capillary network, the elaboration of which into definite channels may permit of a certain amount of variation. It is shown that arteries, veins, and nerves, in the course of their differentiation and outgrowth are subjected to similar influences and may influence one another. Thus the sympathetic chain serves as a guide for the subsequent development around it of a capillary network ; the blood-vessels of the limbs serve as guides for the outgrowing nerves ; a vein (the vena carotis) may develop along the line of a pre-existing artery (carotid artery) and nerve (sympathetic) ; or an artery (stapedial) may develop alongside a vein (capitis lateralis). In many cases the directive influence seems to be of the nature of contact reaction.

Morphological work of this kind is an essential prerequisite for the successful continuation of experimental analysis of the differentiation of blood-vessels along the lines laid down by Roux and Oppel.

In a series of interesting papers, J. Brachet has investigated some important problems relating to the methods of obtaining energy used by frogs' eggs and embryos at different stages of development, and the bearing of these methods on the developmental processes themselves. The first paper (*Arch. de Biol*, **45**, 1934, 611) is concerned with the study of eggs which have been subjected to one of the following treatments : anærobiosis, KCN to inhibit the oxidases, phenylurethane to inhibit the dehydrogenases, monoiodoacetic acid to inhibit glycolysis. Measurements of respiratory quotients were also made.

Brachet finds that in the frog, neither atmospheric oxygen, nor oxygenases, nor glycolysis are essential for cell-division and cleavage, which continues up to the early neurula stage. This is quite different from the sea-urchin, whose eggs immediately cease cleaving when deprived of oxygen. On the other hand, phenylurethane used at early stages stops cleavage at once, the effect decreasing with age.

Brachet's tentative conclusion is that while growth is connected with glycolysis and differentiation with oxidative respiration, cleavage in anærobiosis is dependent on a high *p*H value (oxidation-reduction potential).

Brachet's second paper (*Arch. de Biol.*, **46**, 1935, 1) is concerned with the change in rate of oxygen-consumption by frogs' eggs at fertilisation, which he finds to be non-existent. (Again, differing from the sea-urchin). On the other hand, there is a cyclical variation in oxygen consumption synchronised with the mitosis rhythm.

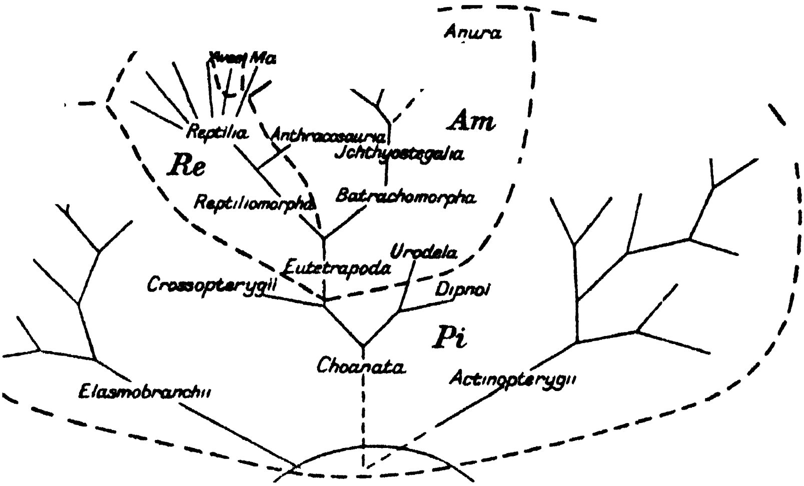
Lastly (*Arch. de Biol.*, **46**, 1935, 25) Brachet has looked to see whether the energy-output of the organiser region of the egg differs from that of other regions. The experiments (estimations of oxygen consumption and CO_2 output of isolated organiser regions and of eggs with the organiser region destroyed) show that the respiratory rate of the organiser region is markedly greater than that of the rest of the egg. This fact has an important bearing on the theory of axial gradients.

GENETICS.—There have been many summaries and detailed accounts of Professor Goldschmidt's celebrated studies on intersexuality in *Lymantria*. In these, new facts have from time to time been added, and the interpretations placed upon some previous observations have now and then been revised. The whole subject has at last been brought together, with a complete bibliography, in a splendid monograph by Goldschmidt (*Bibl. Gen.*, **11**, 1934, 1–186), and he tells us that it is now closed so far as he personally is concerned. Indeed, most of the problems which he set himself to solve have been carried to a successful conclusion. Not only does this latest work cover the whole field of his researches on *Lymantria*, but it contains some important information previously unpublished. Reference may especially be made to the demonstration of the remarkable fact that the female sex-determinant is inherited cytoplasmically. The comparison of intersexuality in *Lymantria* and *Drosophila* will also be read with interest. It may be added that the physiology of intersexuality in the latter species has recently been expounded in a short and helpful summary by J. Kerkis (*C.R. Acad. Sci. U.S.S.R.*, 1934, 291–4).

Some important facts bearing upon dominance theory have recently been published. E. F. Barrows (*J. Gen.*, **29**, 1934, 9–15) has studied the well-known recessive gene "non-agouti," (*a*) in mice, which produces black coat-colour. He has shown that it behaves as a semi-dominant in the presence of one or more semi-dominant "modifiers." These interact with the gene-complex containing *Aa* to express in some degree the effect of the non-agouti gene in the heterozygous state. Furthermore, L. C. Dunn and W. Landauer have obtained important results from their study of the "rumpless" fowl (*ibid.*, pp. 217–43). These birds present several abnormalities: they lack certain vertebræ from the synsacrum and pygostyle, and they are without an oil-gland. These effects are all under the control of a single gene, which behaved as a dominant in the stock in which it appeared, and in some crosses. In others, however, the F1 heterozygotes were modified towards the normal type. By inbreeding and selection from these, a stock was obtained in which the rumpless gene behaved as a recessive. Not all the characters were equally affected in the modification of the heterozygote towards the normal. Thus they provide another instance of the principle noticed by Ford in 1930, that the multiple effects of a single gene, when undergoing dominance modification according to the theory of Fisher, should be capable of independent change. In the present instance the alteration in dominance appears to be due to the action of genes brought in from certain normal fowls, and their accumulation within the rumpless stock by selection.

T. S. Painter (*Genetics*, **19**, 1934, 448–69) has studied the chromosomes in the salivary glands of the larvæ of *Drosophila melanogaster*. He finds that when stained with aceto-carmine they are marked by numerous recognisable bands and lines whose position is accurately maintained. By means of these it is possible to follow translocations and to determine the length of deletions. On this basis he has made a new map of the X-chromosome, using breakages produced by X-rays to find the position of the genes. It is a surprising fact that the inert regions of neither the X-chromosome nor the Y-chromosome, except perhaps a fragment which may carry the locus of "bobbed," are visible in these salivary gland preparations. It seems then that only the genetically active regions can be identified in them. References to the previous work on this new technique are given in the present paper.

PALÆONTOLOGY.—Säve-Söderbergh (*Arkiv för Zoologi*, **26a**, 1934, no. 17) has reviewed the classification and phylogeny of the Vertebrates in the light of recent researches. The main difference in his classification from previous ones is the emphasis laid on the division of the fish into three groups: Elasmobranchii, Choanata, and Actinopterygii, arising from a common ancestor; and the division of the primitive Amphibia into two groups: those which gave rise to all subsequent Amphibia and those which gave rise to the Reptilia. The Dipnoi, Crossopterygii, and Amphibia form a more uniformly natural group, the Choanata, than do the Crossopterygii Actinopterygii, and Dipnoi as in previous classifications. The Urodeles he suggests have an independent origin from other Tetrapods, being derived from the Dipnoi and not from the Crossopterygii. This view of the phylogeny of the Urodeles, however, is less likely to meet with acceptance.



Watson (*Proc. Zool. Soc.*, 1934, 437) working independently of Stensiö (reviewed in the last number) has come to the conclusion that the Arthrodira are true gnathostomes, possessing a jaw mechanism similar to any other fish, both a palatoquadrate and Meckel's cartilages being present. The teeth of the Arthrodira at first showed Elasmobranch succession but later in evolution this succession was abolished and the teeth became permanently fused to the jaws. There is therefore nothing very remarkable in their jaw mechanism, which must have functioned as in other fish and not in the peculiar ways suggested by Jaekel, Adams, and Heintz, who supposed that the upper jaw had to be moved relatively to the lower to open the mouth.

PHYSICAL ANTHROPOLOGY. By L. H. Dudley Buxton, M.A., D.Sc., Exeter College, Oxford.

It would not be fitting to let an account of the progress in anthropology pass without paying a tribute to the work done by Professor Arthur Thomson, who retired this summer from the Chair of Anatomy in Oxford.[1] Professor Thomson has contributed to anthropological studies in this country not only by his published works but also by his long series of lectures in the subject which have been delivered annually since 1895. His successor, Professor Le Gros Clark, is also an anthropologist as well as an anatomist, and in the January number of *Man* he contributes an important statement on his views on "Man's Place among the Primates." He believes that the lemurs and their allies branched off from the basal Primates at an extremely early stage, probably the very beginning of the Tertiary Period. A second branch from the basal Primate stem was that of Tarsius, who possessed features which approached the higher Primates but like the lemurs showed specialised tendencies.

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[1] Since the above was written we regret to have to record that Professor Thomson has died at Oxford.

Soon after the branching off of Tarsius the monkeys separated from the group which was eventually to produce the apes and man. There are to-day two separate branches of monkeys, old world and new world, but in this case there has evidently been parallel evolution.

In regard to the apes the Professor says : " A searching analysis of human morphology leads to the inevitable conclusion that in the structure of the brain, skull and other features the human stem must have passed through a phase of evolution in which it so closely resembled the known anthropoid apes that it is necessary to postulate an anthropomorph ancestor for modern man."

The difficulties in the way of deciding at which stage the human stem branched off are discussed at length. He suggests that the human stem diverged from the anthropoids when the common ancestor was a comparatively small animal. A point which needs special attention is whether this divergence did not take place at a level corresponding to that of the gibbons, rather than a later stage represented by the gorilla and the chimpanzee. This is especially necessary because the two fossil specimens, the Java Ape-Man *Pithecanthropus* and the Peking Man *Sinanthropus*, resemble the gibbons in certain respects and indeed have been classified as gibbons by some writers, although it is doubtful whether they really bear any close relation to the evolutionary development of Man as we understand him.

The Professor suggests that we shall find the direct ancestors of Man among the Miocene forms of *Dryopithecus*. Unfortunately these animals are at present only known to us from remains of jaws and teeth, and evidence is badly needed about their general skeletal form.

A more specialised paper on the evolution of man is that by Dr. Charles Midlo (*Amer. Journ. Phys. Anthrop.*, XIX, 3, 1934). He discusses the external appearance of the hand and foot in Primates, a subject whose importance and interest is well known especially from the writings of Dr. Wood Jones. It is very interesting to compare Dr. Midlo's findings with those of Professor Clark given above. Dr. Midlo suggests that the Prosimian (that is Tarsius, lemurs and their allies) hand and foot are markedly specialised structures, with a greater deviation in their external form from the primitive mammalian form than those of the higher Primates.

Both the Old and New World monkeys have rather generalised hands and feet but various families, especially of the New World monkeys, show varying degrees of specialisation. In the next higher group we find that the gibbons have rather unspecialised feet, but markedly specialised hands, owing no doubt to their arboreal habits. Among the higher apes we get varying degrees of specialisation side by side with the retention of primitive characters and it is especially interesting to note that Dr. Midlo confirms the generally held view that Man has a more primitive hand and a more specialised foot.

George H. Hansen (*American Anthropologist N.S.*, **36**, No. 3, 1934) describes a skull cap found in Utah Lake. The shore of the lake was formerly a favourite place of habitation of ancient man as is clearly shown by archæological evidence. Owing to the drought it was possible to explore points normally far out in the lake. This skull cap was found at a position usually quite inaccessible. Morphologically it contrasts strongly with local Indians and the mound builders of the lake shores. The skull should prove of considerable interest in the study of the distribution of the early populations of America. The *Zeitschrift für Ethnologie*, **66**, Heft 1/3, 1934, contains an account by Herta Busse on the old Slavonic skulls from Eastern Germany. This extremely careful paper, much of which is devoted to a necessarily technical description of the crania, is important as adding a further piece of evidence to our knowledge of the contrast between the culture of the region which was Slavonic and the physical type of the inhabitants which was closer to their more western neighbours speaking Teutonic languages.

There are two American papers which investigate subjects to which anthropologists have normally given less attention. First B. B. Gardner and D. L. MacAdam (*Amer. Journ. Phys. Anthrop.*, XIX, 2) discuss the question of using the technique of physics for testing hair-colour by a colorimetric analysis. They used Professor Hardy's recording spectrophotometer and attempted to analyse hair samples. As they themselves say, the number of examples used (53) was really too few, but the results obtained do suggest that you could get a scientifically graded hair-colour series which would have the advantage of standardising the work of all observers and also of making the material available, if required, for statistical study.

Frederick G. Murray has a paper of exceptional general interest (*American Anthropologist N.S.*, **36**, No. 3). He calls attention to the importance which medical research, especially on rickets, may well have to anthropologists. He postulates two factors, sunlight and such food as fish, oil and meat, and ascribes the whiteness of the White Race as being due to an adaptation which allows as much sunlight as possible to penetrate the body. But the Eskimo, who have black hair and a pigmented skin, should on the same theory also have become white to get sunlight, to which in fact they are even less subjected than the White man. But Mr. Murray draws attention to the fact that their diet—fish, oil and meat—is just as antirachitic as sunlight, and believes that such a diet has not made a specialisation of skin colour necessary. On the other hand, in the tropics there is excessive light and the author suggests that this superabundance of sunlight, combined with a nutritional disease factor, militates against the wellbeing of the white man in the tropics.

Comparatively few of the papers read at the International Congress of Ethnology and Anthropology have yet appeared in more than brief summaries. It is impossible therefore to do more than speak very briefly on the subject. It would appear, however, that the main interests of the physical anthropologists were tending towards two lines at the moment, first the evolution of man (Professor Le Gros Clark's paper has already been noted) and secondly the systematic classification of racial differences especially in relation to their geographical distribution. On the second subject a fundamental difference of opinion seemed to manifest itself. Some anthropologists preferred to take their racial or cranial series as a whole and to study them in relation to their neighbours and to other similar groups. Other speakers again, notably Professor Czanowski, were equally definite that this was hardly a satisfactory line of approach. They considered that any series should be divided into its component parts and that any particular group should be analysed as containing, say, so much per cent. Nordic, so much Alpine and so much Mediterranean, or whatever the components may be. The various papers will no doubt be published in due course, but there can be little doubt that the extremely friendly spirit in which these discussions and those on standardisation of anthropometric data were discussed, although they may appear to be as evanescent as a broadcast programme, will really contribute considerably to the progress of physical anthropology which in the past has suffered very much from the difficulty of different schools seeing each other's point of view. The demonstration by Professor Schlauginhaufen and others who were trained in Professor Martin's school on the actual methods described in Martin's *Lehrbuch der Anthropologie* may be also mentioned as particularly valuable. Skulls, calipers, and skilful fingers cleared away many difficulties which mere words had only tended to confuse. These things can hardly appear as published transactions, but do in fact contribute more to the progress of the subject than a good many published papers.

NOTES

Piano Touch and Tone (S. K. L.)

Most pianists are inclined to the view that the tone quality obtainable from a piano is under the control of the player, whereas to the scientific mind such a conception is not readily acceptable. Many investigators have studied this problem, usually with results that indicate that the control of tone is beyond the capabilities of the player. An important contribution to this study is a paper on " A Precision Study of Piano Touch and Tone," by Hart, Fuller, and Lusby, in the *Journal of the Acoustical Society of America* for October 1934, pp. 80–94. The authors consider that the earlier evidence is not sufficiently conclusive, and have attempted to settle the question once for all. The experiments involved a considerable amount of detail work in removing or analysing various sources of error.

Theoretically, the loudness and tone quality of a piano note are determined by the velocity, acceleration, and higher time derivatives of the hammer when striking the string, but in practice, due to the great rigidity of the hammer shank, the influence of the acceleration and the higher time derivatives is negligible. The duration of contact is an important factor and depends, theoretically at least, not only on the angular velocity, but on a vibratory or whipping motion due to the finite mass and elasticity, and a slight change in the force applied to the key might well reverse the phase of the vibrational motion of the hammer at the moment of impact. It was therefore decided to record the motion of the hammer and the emitted sound.

The hammer motion was recorded optically, the apparatus being designed to fit into the piano with the least possible derangement. A specially designed mechanical striker was substituted for the player's finger to give a calculable performance variable between wide limits. This striker was arranged to have positive, zero, or negative acceleration, with controlled free period of oscillation and controlled velocity. A concert grand piano was used with a condenser microphone, amplifier, and moving-coil oscillograph in an acoustic-deadened room. On account of the limited space inside the piano, the greater part of the work was confined to middle C, but preliminary experiments showed that these results would be typical of other notes taken from any octave.

In all the tests made with various adjustments of the mechanical striker, identical sound pictures were obtained with any acceleration or any free period, provided that the final velocity of the hammer just before striking was the same. Records were made of the touch and tone of reputed pianists, and it was found that the differences of tone were obtained only by alterations of the amplitude, and in every case the tone could be matched exactly by the mechanical striker, even when it was set for different forms of striking. The trained pianist can, of course, make use of various combinations of tones and dynamic emphasis, but he cannot be distinguished from the novice when striking only one single note Actually the pianist can do nothing to vary the tone except to vary the final hammer velocity, and in so doing he inevitably varies the loudness too. The quality varies, of course, with the loudness and register.

Fisheries Notes [1] (M. V. L.)

The Herring Industry of our coasts is steadily declining. The first report of the commission appointed for its investigation contains far-reaching proposals for its re-organisation. The fall of trade is largely responsible, but even more important is the fact that changing circumstances have placed the fishing fleet as a whole in acute financial distress. It is the same with the curing section of the Industry, and the home market for herring in all forms is steadily declining. Under modern conditions it is impossible for the old system of individual ownership to work, for even if the catches of the last years had reached pre-war dimensions the number of boats is far too large. It is proposed that a Herring Board be established with jurisdiction over all the Herring fishings of the United Kingdom, assisted by eight Area Committees and by Panels representative of the respective sections of the Industry. Primarily the function of the Board would be to create a framework of organisation within the Industry which would present scope for individual initiative but at the same time enrol all sections in a concerted effort to restore economic stability as a whole. This would rest largely on a system of licensing of boats, fishermen, salesmen, curers and kipperers with thorough supervising of the fishing fleet.

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[1] (1) First Report, *The Herring Industry*, Sea-Fish Commission for the United Kingdom (London : H M. Stationery Office, 1934. 9*d*. net.) (2) *The Value of Herring as Food* (Ministry of Agriculture and Fisheries. Fisheries Notice No. 4). (3) *Size Limits for Fish and Regulations of the Meshes of Fishing Nets* (Cons. Perm. p. l'Expl. de la Mer. Rapports et procèsverbaux des Réunions, vol. XC, Copenhagen, 1934 (Kr. 3.00)). (4) Report of Delegates of the United Kingdom of the 27th Meeting of the International Council for the Exploration of the Sea, held in Copenhagen from June 4–11, 1934. (5) *Sea Trout of the Montrose District*. By G. Herbert Nall (Fisheries, Scotland, Salmon Fish., 1934, No. III. (H.M. Stationery Office, 1934. 3*s*. 6*d*. net.)

To increase home consumption special attention is given to kippering and the proposed establishment of a National Mark, thus enabling the consumer to be certain of the best, and a leaflet on the value of Herring as food is issued with many recipes for cooking this excellent fish. The cost of 1 lb. of first-class protein is obtained from Herring for 3*s.* 5*d.* whilst the same amount from beef, eggs and mutton is 5*s.* 4*d.* to 6*s* , 7*s.* 6*d.* and 8*s.* 1*d.* respectively. A substantial increase in home consumption would be of the greatest possible help to this hard-pressed Industry. On the whole the report has been favourably received by the Industry, though certain sections of it—notably the curers—do not favour the recommendations put forward.

The meeting of the International Council in June was specially interesting, the most important subjects raised being the regulation of size of mesh in the trawl nets, and the imposition of a size limit for food-fishes which may be landed for sale. Delegates from nearly all countries concerned took part in the discussion. It is found that with an increase of size of mesh of the bag or cod-end of the trawl not only do a large number of small food-fishes, particularly round-fishes, escape but a larger proportion of marketable size are caught. Already in Great Britain the size of the mesh has been enlarged, and also in some other countries, and in most there is a fixed minimum size for the capture of the more important food-fishes. The conclusions of the Committee are that it is important for the maintenance of the stock that measures to prevent waste be taken by all countries fishing in the waters included in the Council's investigations, and the most useful measure of control will take the form of prevention, as far as possible, of the capture of young fish below the size at which they can be sold at a remunerative price for the food of man. Appreciable protection of young round-fishes, in the case of the trawl net and Danish seine, can be secured by regulating the size of mesh of the cod-end. It is recommended that the minimum mesh be employed that is already enforced by the British Government, and that size limits should also be imposed both for flat-fishes and round-fishes, there being no way of regulating the mesh for flat-fishes which would not also release round-fishes of a larger size. These proposals do not apply to Herrings, Sprat, Pilchard and Mackerel. As to the question of the destruction of fish-fry by the nets of small mesh used for prawns and shrimps, it was agreed that when the nets are shown to be unduly destructive they should be regulated by each country in the light of local conditions.

In conclusion the Council stated that it was convinced that, with a view to ultimate solution of these questions, it is essential that each Government should arrange for further observations to be carried out by competent observers, not only in research vessels, but also in commercial vessels and in the fish-markets.

In his paper on Sea-Trout, Mr. Nall continues his series of researches. The present work deals with the Sea-Trout of the Montrose District, which seem to form a natural group distinguished by definite characteristics from those north of Aberdeen, and those of rivers to the north such as the Tay. Most of them migrate after three years of parr life, the sea-growth being good though less rapid than in the Scottish types. Most of the fish begin to spawn in the second winter of migration. There are indications that the Sea-Trout kelts may sometimes start growth whilst in fresh water. Old Sea-Trout are rare on the east coast of Scotland, but one, the oldest in the collection, had survived 10 winters and was in its eleventh summer since hatching.

Tropical Disease (P. J.)

In the Annual Report of the Calcutta School of Tropical Medicine and the Carmichael Hospital for Tropical Diseases, 1933, details of the year's work are set out and offer most interesting reading. It is not possible in a short notice to review the activities of all these twenty-five departments, but special notice may be given to the bacteriological investigations on *Vibrio choleræ*, of which 704 strains were isolated from 430 cases admitted to the Carmichael Hospital. These strains were examined for smoothness and roughness, nitroso-indol reaction, hæmolysis of human red cells, lysability by cholera-phage, and for agglutinability or non-agglutinability by standard cholera high-titre sera. A full report of this valuable investigation is being prepared for publication ; the chief result has been the establishing of the ætiological relationship to cholera of the so-called non-agglutinating vibrios. Of equal importance is the account by Lieut.-Col. Knowles from the protozoological section, where further research has been made on monkey (*Silenus irus*) malaria (*P. knowlesi*). This organism is very fatal for monkeys and can be readily transmitted to man, whereas it is the rule that human malaria cannot be transmitted to the lower apes. Studying the relative toxicity and curative doses of different drugs in monkey malaria it was found that monkeys of 3–4 kilos could withstand 1½ gr. of quinine intravenously but 2 gr. usually caused death. If quinine is given intramuscularly only small doses (2 gr. in 1½ c.c. of water) should be given ; large doses so damage the tissues that less quinine is absorbed. Atebrin is more efficacious than quinine, the parasite count falling to negligible values in twenty-four hours ; relapses are the rule, but these are readily amenable to a second treatment with atebrin. With experimental infections in animals there is a very rapid recovery of the erythrocyte count. Much valuable information was obtained by the use of an air-conditioning chamber, whereby the various Calcutta seasons could be artificially reproduced and the infection rates of experimentally infected mosquitoes could be studied, by serial sectioning and dissection. It was found that at 50° F. and all humidities between 50 and 100 per cent. no infection was obtained with any species. The kala azar inquiry is full of great interest, especially with regard to the anæmia produced and the values of various therapeutic agents used in the treatment. The bionomics of *Phlebotomus argentipes* and the effects of larvicides forms another valuable section. The whole report is a noteworthy acquisition to the knowledge of tropical medicine in India and reflects great credit on the organisation and industry of the Calcutta school.

In his " Essay-Review of the Work done by the Calcutta School of Tropical Medicine during the years 1920 to 1933," and published as an appendix to the 1933 report—Lieut.-Col. Knowles presents a most interesting and valuable survey of the activities of this school. In an historical résumé which follows on the earlier report of Sir L. Rogers, its remarkable growth in importance is traced in detail and makes a wonderful record of a great hygienic institution, centre of medical research and postgraduate school. In the 168 pages are condensed the results of ten years' work in the many different departments, investigations on kala azar, Oriental sore, malaria, bacterial and amœbic dysentery, sprue, cholera and bacteriophage, intestinal helminths, hookworm, filariasis, leprosy, etc. Each subject is studied from epidemiological and climatical points of view, especially in relation to transmitting hosts ; there are also discussions on the relative values of different drugs used in the treatment of these conditions and their reactions on experimental animals. This crowding together of such a mass of facts approaches the form of a catalogue and does not make easy reading ; many points would greatly benefit by expansion, and short summaries at the ends of sections would be useful. The general medical reader is not well versed in Indian geography and would be much helped by maps of country showing the districts where the diseases prevail. For students of tropical medicine the report will be essential. An index would be most acceptable for reference purposes.

Miscellanea

The New Year Honours list included the following names: *G.C.B.:* Sir Josiah Stamp. *K.C.M.G.:* Dr. A. C. D. Rivett, deputy-chairman of the Council of Scientific and Industrial Research, Commonwealth of Australia. *Knights:* Dr C V. Boys—physicist; Dr. Cyril Fox, director of the National Museum of Wales; Dr. J. B. Orr, director of the Rowett Institute of Research in Animal Nutrition, Aberdeen; Prof. E. B. Poulton, emeritus professor of zoology in the University of Oxford, Dr. J. D. Sutherland, lately assistant forestry commissioner for Scotland. *C M G.:* Prof. F. L. Engledow, professor of agriculture in the University of Cambridge. *C.B.E.:* Dr. E. J. Allen, director of the Plymouth Laboratory; Mr. C. C. Hawkins, lately superintendent of the Department of Technology, City and Guilds Institute; Dr. J. S. Plaskett, director of the Astrophysical Observatory, Victoria, Canada. *O B.E.:* Mr. G. W. Austin, principal scientific officer, R N. Torpedo Factory, Greenock.

The Nobel prize for chemistry for 1934 has been awarded to Prof. H. C. Urey of Columbia University, New York, who first detected the existence of "heavy" hydrogen by means of its spectrum.

Prof. E. V. Appleton has been elected to be a corresponding member of the Prussian Academy of Sciences.

The Gold Medal of the Royal Astronomical Society has been awarded to Professor E. A. Milne, Rouse Ball professor of mathematics in the University of Oxford.

The Wollaston Medal of the Geological Society has been awarded to Sir John Flett, director of H M. Geological Survey, the Murchison Medal to Prof. E. B. Bailey of the University of Glasgow and the Lyell Medal to Prof. D. M. S. Watson of University College, London. The Duddell Medal of the Physical Society has been awarded to Dr. W. E. Williams and the Buchan Prize of the Royal Meteorological Society to Dr. F. J. W. Whipple.

We have noted with regret the announcement of the deaths of the following well-known scientific workers during the past quarter: Dr. N. E. Brown, botanist; the Rev. A. H. Cooke, zoologist; Prof. O. D. Chwolson, formerly professor of mathematics at St. Petersburg; Dr. F. A. Dixey, F.R.S., entomologist; Prof. R. B. Dixon of Harvard, anthropologist; Sir Alfred Ewing; Sir Horace Lamb; Dr. J. W. Leather, agricultural chemist; Prof. K. von Linde of Munich, famous for his work on refrigeration and the liquefaction of gases; Rev. S. A. McDowall, senior science master at Winchester; Prof. S. P. Mulliken of Massachusetts, organic chemist; Prof. Santiago Ramon-y-Cajal, For. Mem. R.S., anatomist; Prof. W. de Sitter, director of the Leiden Observatory; Prof. T. Smith, For. Mem. R S., pathologist.

Prof. W. W. Watts has been elected president of the British Association for the year 1935. The Association will meet at Norwich during the week Sept. 4–11. The following is a list of those who will preside over some of the Sections: A, Dr. F. W. Aston; B, Prof. W. H. Haworth; C (Geology), Prof. G. Hickling; D (Zoology), Prof. F. Balfour Browne; E (Geography), Prof. F. Debenham; H (Anthropology), Dr. Cyril Fox; I (Physiology), Prof. P. T. Herring, K (Botany), Mr. F. T. Brooks; M (Agriculture), Dr. J. A. Venn.

An Imperial Botanical Conference, commencing on August 28 and lasting two to three days, will be held in London this year. The subjects set down for discussion are of general interest to Empire botanists and include such topics as pasture research within the Empire, the ecology of tropical forests, the application of ecological methods to the study of native agriculture, problems of fruit storage and transport with special reference to tropical conditions, the furtherance of schemes for the closer co-ordination of botanical research within the Empire, etc. The Chairman of the Organising Committee of the Conference is Sir Arthur W. Hill, K.C.M.G., Director of the Royal Botanic Gardens, Kew, and the Hon. Secretary is Prof. W. Brown, Imperial College of Science and Technology, South Kensington, London, S.W.7, from whom further particulars may be obtained.

The Annual Congress of the South-Eastern Union of Scientific Societies will be held at Bournemouth during the period June 26–29 under the presidency of Prof. A. C. Seward.

The Report of the Water Pollution Research Board for the year ending on June 30, 1934 (H.M. Stationery Office, 9*d.* net), refers to the serious effect of the drought, not merely on the quantity, but also on the quality of the water in rivers and streams into which trade effluents and sewage are discharged. It is pointed out that the increasing demands of the community in future years are likely to render it necessary to draw more and more water from polluted sources and that it is therefore urgently necessary that means should be discovered for the treatment of various trade effluents at a reasonable cost. For example, the effluents from dairies and milk product factories may seriously affect rivers and streams into which they are discharged and may be many times as strong in polluting character as domestic sewage. This problem is of particular importance at the present time owing to the expansion of the milk industry and the increase in the number of large centralised factories and milk collecting and distributing depots. The Board has investigated the matter and believes that means have been discovered whereby the effluent can be purified before it is discharged. The work has reached the stage at which large-scale tests are necessary, and the industry has been given the opportunity of co-operating both technically and financially in the further investigations which are desirable.

The *Journal of Research* of the National Bureau of Standards (Washington, D.C.) for November contains a paper by Hoffman and Roeser on the determination of the freezing-point of gallium. The metal appeared to have a low thermal conductivity and undercooled very readily (sometimes remaining liquid at temperatures as much as 15° C. below the F.P.) so that the usual heating and cooling curve method could not be used. Consistent results were, however, obtained by observing the steady temperature of a mass of the metal, half solid, half liquid, when surrounded by a water bath maintained at various constant temperatures within a range of 0·1° C. higher and lower than the F.P. Their final value was 29·780 ± 0·005° C. for samples of the metal purified by J. I. Hoffmann by methods described in the *Journal* and containing less than 0·001 per cent. of impurity. The F.P. was not affected by the presence of the oxide which is therefore probably insoluble in the pure metal.

The January number of the *Bell Laboratories Record* contains an interesting paper by Dr. Harvey Fletcher on the relationship between the pitch and frequency of a musical tone. It is usually stated that the pitch of a sound, *i.e.* its position on the musical scale, is determined only by the frequency of the vibrations producing it (or by the predominating frequency in the case of a complex tone) although it is obvious that experiments with a Savart toothed wheel or a siren give only a very approximate indication of the truth of the hypothesis. Dr. Harvey Fletcher has investigated the relationship between the two quantities for pure and complex tones of varying degrees of loudness with the highly developed technique of sound production and measurement available in the Bell Laboratories and has shown that pitch is a function of loudness as well as of frequency. In making the comparison a loudness level of 40 decibels was taken as standard and it was found, for example, that a pure tone of frequency 262 v.p.s. and loudness level 80 decibels gives a note of the same pitch as another of frequency 238 7 v.p.s. and loudness level 40 decibels.

The effect varies with the frequency and reaches a maximum for frequencies in the neighbourhood of 185 vibrations per second (about F sharp below the middle C), in which part of the scale an increase of loudness from 40 to 90 decibels lowers the pitch almost a full tone (0·14 octave). For frequencies near the lower audible limit (16–32 v.p.s.) and within the octave 1050–2100 v.p.s. the variation is negligible ; for higher frequencies increase of loudness increases the pitch very slightly. For complex tones the effect is not so well marked, *e.g.* while a pure tone of frequency 200 v p.s. is lowered 0·15 octave when the loudness level rises from 40 to 100 decibels another built up of frequencies 200, 400, 600, 800 and 1000, each of the same intensity, dropped only 0·03 octave for the same change of loudness. During the course of the investigation it was found that the addition of overtones to a pure tone increases the loudness level much more than would be expected from the increase in the intensity (rate of flow of energy) produced.

The discovery that pitch depends on loudness makes it necessary to define precisely what is meant by the pitch level of a tone. This Dr. Fletcher takes as the frequency level of a tone of 40 decibels loudness which is judged equal to it in pitch. For frequency *levels* the octave is taken as the unit and the frequency level F of a pure tone of frequency n is defined by the expression $2^F = n/16{\cdot}35$, *i.e.* $F = 3{\cdot}321(\log_{10} n - \log_{10} 16{\cdot}35)$. The frequency 16·35 is taken as the datum level because it is that of the C nearest the audible limit of frequency. It is, in fact, four octaves below middle C (262 v.p.s.).

ESSAY REVIEW

THE LIMITS OF SCIENCE. By G. BURNISTON BROWN, M Sc., Ph.D., University College, London. Being a review of **Scientific Theory and Religion,** Gifford Lectures, by ERNEST WILLIAM BARNES, Sc.D., F.R.S., Bishop of Birmingham, [pp. xxiv + 685] (Cambridge: at the University Press, 1933. 25*s.* net); **God and the Astronomers,** by WILLIAM RALPH INGE, Dean of St Paul's, [pp xi + 308] (London: Longmans, Green & Co, 1933. 12*s.* 6*d.* net); **Limitations of Science,** by J. W. N. SULLIVAN, [pp vi + 303] (London: Chatto & Windus, 1933. 7*s.* 6*d.* net.)

"And have not the processes of measuring and counting and weighing made their appearance most agreeably to aid us in dispelling these tricks of fancy, and to overthrow within us the power of vague notions of degrees of magnitude, quantity, and weight, and establish the control of the principle which has calculated or measured or weighed?"
"Undoubtedly." PLATO, *Republic.*

"But I am only too well aware that, if we admit the existence of realms which science has not conquered, we give to religious quacks and obscurantists domains where only too probably they will house superstition."
THE BISHOP OF BIRMINGHAM.

THE attainment of spiritual grace has at all times been an arduous and difficult task. It has involved isolation, fasting, living on the tops of pillars, and other even more peculiar methods of mortifying the flesh. But all these processes were ancillary to the main activity which was inner contemplation. The demands made on the knowledge of the neophyte were few: familiarity with the writings and sayings of certain persons held to have had a more than ordinary share of human wisdom; an acquaintance with those processes of doubtful logic which medieval scholastics regarded as proofs of theism; and for such problems as were not either purely imaginary or purely grammatical, a recourse to the all but omniscient Aristotle whose disquisitions on the external world were held as hardly less infallible than those of the saints on the internal world of the soul.

But all this has changed; a pious ignorance is not sufficient. A new horror has been added to a path already steep and thorny enough. And this because the modernists among our churchmen are beginning to agree that the knowledge provided in ever-increasing amounts by the prosecution of the scientific method can neither be ignored nor refuted and must therefore be assimilated with as good grace as possible. In fact, Dr. Barnes, to whose enlightened views the Church in this country owes so much, quotes with evident satisfaction a statement by another distinguished modernist, the former Dean of St. Paul's, to the effect that "The right starting-point for theology is to examine the conception of the world as known to science."

The full implication of this dictum can be realised by a glance at some of the headings to paragraphs in the Bishop's Gifford Lectures in which he sets out to perform this examination, as far, he says modestly, as "my ignorance will permit." Paragraph 33 is "Consideration of Lobatchewskyan or hyperbolic plane geometry"; § 105 is "Minkowski's world. Time and proper-time. The 'four-velocity' and 'four-acceleration' vectors'"; § 186 "The nature of entropy"; § 209 Wave-mechanics and the Born-Dirac calculus"; § 275 "Star-streaming, and differential galactic rotation"; § 340 "Dicotyledons and monocotyledons"; § 349 "Parthogenesis: haploid males and diploid females. Artificial parthenogenesis"; § 632 "Religious ecstasy must be subject to morality and reason", § 490 "The Self as Ego," and so on. Truly the way has become steeper and narrower and many there are that will fall by the wayside! And yet these lectures "set forth only such new knowledge as ought (apart of course from the mathematical reasoning by which it is reached) to be familiar to, and a background for the belief of, a well-educated man." Dr. Barnes is, however, sufficiently practical to add "but it is not probable that the ideas here presented will be absorbed into the general consciousness of the community before several generations have passed."

This great treatise of nearly 700 pages is, then, an attempt to understand, in the words of its sub-title, "The World described by Science and its Spiritual Interpretation." The Bishop's own standpoint is that of moderate realism: he believes in a physical world existing independently of any human mind, but this external world is not necessarily the world as conceived by humanity: "Though it is not constructed by my mind my supposed knowledge of it is the construction of the general human mind. We can only believe that we have some, not entirely inaccurate, knowledge of the external world as it really is, as it is known to God, if we make the further assumption that our minds are akin to the Divine Mind; or, in other words, that there is some ground of unity between man and God" (p. 7). And later "The extent to which we frame true ideas is a measure of our capacity to 'think the thoughts of God'" (p. 9). Clearly Dr. Barnes holds very complex views in epistemology, but as these are only introductory remarks, the startled scientist must push on and hope for further enlightenment.

The emphasis on geometry in the first two hundred pages is essential to the Bishop's scheme, since he attempts to use geometry as an example of the limitations of scientific knowledge in two ways. First to show that we are never free in science from possible errors of perception, *i.e.* although Euclid's geometry was taken as true for so long, there are really many self-consistent systems of geometry, and we are at pains to discover which fits the real world most successfully. And secondly, he is anxious to make it clear that even in an almost perfect science like geometry we do not get complete insight into the nature of space. The reader will, however, probably feel that these points could be treated more generally (and much more happily). For instance, if instead of two hundred pages of abstruse geometry, twenty pages were given to the explanation of the difference between a *fact* and a *hypothesis* and the criteria used in choosing between different *theories*, the reader would then understand the difficulties and limitations of science and all clear thinking.

Dr. Barnes's second point is also rather unfortunate, since space is not an objective entity and not therefore directly observable or measurable. The fact that our knowledge of "no-thing" is nothing is hardly a convincing argument for the limitations of science!

However, let us now embark with the Bishop on a voyage through the vast sea of knowledge which has accumulated up to the present time. The Bishop being, as he says, a moderate realist we start naturally with matter. In defining matter he dwells only on its chief metrical characteristic—inertia. And here at once we find confusion between facts and hypotheses. He states that the mathematical physicist, following Galileo and Newton, says that "A body (*i.e.* a piece of matter) removed away from all other bodies would continue in a state of rest or of uniform (*i.e.* steady) motion in a straight line." This, Dr. Barnes says, "must be regarded as an experimental fact" (p. 15). But clearly this is an extrapolation from experience and not a very obvious one [1]: it has not been, and by its nature is hardly likely to be, confirmed by experiment and is therefore a *hypothesis* and a very complicated one. Since, however, the whole of Newtonian mechanics is built on it, it is usually dignified by being called the *Principle* of Inertia: but it may be false nevertheless.

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[1] Sir Arthur Eddington shows this very delightfully in *The Nature of the Physical World*, p. 123.

In continuing his account of matter the author presents very lucidly the salient features, and references to Poincaré and Mach show that he has not accepted the Newtonian system uncritically. In dealing with the definition of time, however, he says, "The beats of an ideally perfect pendulum are of equal duration. This is a fact which can be verified experimentally by comparing the motions of pendulums of different lengths placed near together: it can also be deduced from physical theory." This seems to be another over-simplification, and confusion, this time, between experimental facts and *definitions* or conventions. The experiment with the pendulums unfortunately proves nothing at all about duration until we have defined *equal intervals*. It is true that a pendulum under certain conditions should beat equal intervals of time according to Newtonian theory; but since the theory is founded on experiments which did, in fact, necessitate this assumption (Galileo used his pulse !) for their interpretation, this is not very satisfactory, and it seems preferable to put it frankly and simply in the form of a definition—"Equal durations of time are given by the beats of a pendulum" (with certain specified conditions).[1] Upon such a definition a consistent scheme can be evolved uniting both stellar and 'local' motions.

It is on 'space,' however, that Dr. Barnes really lets himself go. As a distinguished mathematician, he is able to deal fully with the different geometries that result from the three possibilities of Playfair's Axiom, *viz.* that from a point outside a straight line, any number of coplanar straight lines, only one, or none, can be drawn so as not to cut the first line. This forms an excellent introduction to the theory of relativity in the development of which the author follows Levi-Civita. It is noticeable that the problem of absolute rotation is not avoided as it is by most relativists, and the author agrees with Mach's suggestion that the distant stars are the causes of the bulging at the equator of rotating bodies. At any rate this is a preferable hypothesis, since if we must have 'causes' (and it seems that a dynamical view of nature is not humanly possible without it), it is better to have ones that we can keep an eye on, *i.e.* ones which can be observed and measured rather than ones for ever unobservable like Newton's absolute space. This epistemological criterion applies, of course, equally well to the use of space-time as a cause, but the Bishop fails to avoid this pitfall—a common error among relativists with the great exception of Einstein himself.[1] Space-time is no more an objective entity than Newton's absolute space—" space and time are merely the observer's dissection of that reality which is the change in the observed mutual relationships of observed material particles " as Milne has pointed out.[2]

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[1] Sir Arthur Eddington is, I believe, in agreement with this view.

In turning to the experimental ' proofs ' of the theory of relativity it is refreshing to find a critical account which does not make it appear that its predictions have been confirmed exactly. The author points out that the problem of two bodies in relativity is as difficult as the problem of three bodies in Newtonian mechanics, and consequently in the calculation relating to the precession of the perihelion of Mercury the sun has to be treated as a particle and the planet must have no mass at all. If this is done the discrepancy existing between Newtonian theory and fact is " almost accurately eliminated." Similarly the bending of light round the sun is " in fairly close agreement " with calculation. The spectral-shift test is " so far not decisive." The difficulties in connection with the conservation of energy and momentum are also well brought out. Finally the Bishop ends a masterly survey with a note of caution—" The qualitative distinction between space and time which is always present in common thought may correspond to something in the character of the Universe which eludes the physicist's measurements. In fact, the limitations of purely quantitative analysis must never be forgotten " (p. 192).

The next two hundred pages are devoted to an exposition of the electrical theory of matter, heat and light, quantum theory, X-rays, and astronomy. In discussing the second law of thermodynamics and the probable end of the universe, Dr. Barnes insists that, so far, we have no evidence against such a conclusion, although there may be places in the cosmos where this law is reversed, possibly in cycles of time. He is moved by this, and by the difficulty in accounting for the original state of ' wound-up-ness ' to conclude that " God lies behind phenomena."

The apparent loop-hole for free-will supplied by quantum theory and the uncertainty principle, is discussed, and use is made of Lindemann's suggestive approach to the subject: the Bishop, however, does not place much reliance upon it—" mind and spirit belong to the psychical realm " (p. 310). The habitability of the planets and other portions of the universe are examined in the chapter devoted to astronomy, and the conclusion arrived at is that Mars and Venus may have some form of life, and that life probably exists on other planets elsewhere in the universe; but even, " if it should in time be demonstrated that the solar system is probably unique in the cosmos, I think that such uniqueness would be no satisfaction to the religious man who was told that the origin of the system lay in the chance encounter of two stars. To ascribe the source of this earth and, consequently, of life upon it to mere chance would undoubtedly offend religious sentiment."

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[1] Until recently, at any rate. [2] E. A. Milne, *Nature*, 1932, p. 701.

In connection with cosmic rays Dr. Barnes considers that it is not "wholly fanciful" to suppose that perhaps among the rays we receive, are messages from beings in a higher stage of evolution than ourselves.

This great survey of the world as known to the physicist concludes with a reluctance to bring in God in order to pull the trigger or to fill gaps in deficient human knowledge—"It is because throughout the physical universe I find thought and plan and power that behind it I see God as creator. Mere sequences are ultimately unintelligible: causation, apart from the will of God is, as Hume found it, obscure. The laws which the physicists discover may be mainly statistical: but to me they disclose the operation of the Divine will" (p. 410).

On looking back, the reader will probably be surprised that nearly two-thirds of these lectures are given up to physical science, especially as the author himself says (p. 180), "let us not forget that the only foundation for any belief in God worth preserving must be discovered, not in inanimate nature but in that moral order, emergent through animate nature, from which man's spiritual longings have been derived."

Let us turn now to the facts and theories of biology. Dr. Barnes starts with a fascinating outline of our present speculation regarding the origin of life and of our inferences from the record of the rocks. He rejects a sudden creation of primitive forms of living matter because such an occurrence "does not accord with what we know of Divine action in the cosmos. We do not meet with what we may call visible creative acts: *new things rather emerge within and through the natural order*" (p. 412). Nevertheless, this "must be regarded as a sign of creative activity" (p. 419). The extinction of species, often highly specialised—that "unnecessary—even scandalous—waste" the Bishop finds perplexing. The scientist notes that it is a difficulty created by the type of hypothesis employed, *i.e.* that the universe was created for a purpose.

Early in the next chapter on "The Evolution of Plants and Sex" a similar self-made difficulty appears. Referring to fungi which apparently swarmed in the Early Devonian period, the author observes: "The fungi remain among the most troublesome of our parasites in field and garden: as parasitic pests they can boast a pedigree of more than 300 million years! The problem of evil, in fact, is much older than the Garden of Eden." And yet they are doing no more than does the Bishop when he orders a cup of tea—trying to keep body and soul together. And this is evil!

In discussing the ways of ants and bees and their social organisation, Dr. Barnes disagrees with Bergson and the generally accepted view that we are here dealing with instinct and not intelligence. But in his argument he uses the word instinct in a way with which no biologist or psychologist would agree. He says that, "Instinct develops when we have thoroughly learned a lesson, *e.g.* to drive a motor-car; and also when, in our hereditary make-up, we carry knowledge which our ancestors have been able to transmit. But we need intelligence to learn a lesson, and without intelligence we can hardly acquire new knowledge to hand on to our descendants" (p. 456). The handing-on of acquired characteristics is still a matter of hot dispute, to say nothing of knowledge! After the careful presentation of the world of physics the looseness of this phraseology is very striking. The chapter ends with a very unusual treatment of the 'problem' of the Virgin Birth which must be quoted fully:

> "I have, personally, little doubt that biological research will in due course prove a human virgin birth to be possible. Probably the individual so produced would be haploid, with but half the normal number of chromosomes, and the chances are that its sex would be male. But whether haploid or normal, male or female, it would vary little from the normal mental and emotional make-up of the human race. What exactly is meant 'by breaking the entail of sin' I do not know. my imagination fails when a metaphor derived from our land laws is applied to a mixture of biology and theology. But there is no reason to believe that a human being produced by parthenogenesis would lack normal appetites and passions. Biological parallels indicate that, if haploid, such an individual would probably be of sub-normal physical development. I do not personally think that such development could be 'congruous with' the Incarnation, though Rendel Harris in an ingenious investigation has adduced some evidence to show that Jesus of Nazareth was below normal height. It may be added that, if the story of the Virgin Birth be rejected, Jesus will remain, as St. Mark implies, the son of Joseph and Mary. We should then know of nothing in His ancestry to explain the moral ascendancy and religious genius which were undoubtedly His. These qualities might be held to result from a dominant mutation: and if all mutations are to be regarded as manifestations of the creative activity of God, the spiritual excellence of Jesus would be from God" (p. 458).

The lectures continue with a brilliant account of the evolution of animals and of the Mendelian theory. Most readers will find this lecture, which is illustrated with many fascinating examples, the most interesting and convincing in the whole series. In dealing with the machinery of evolution, Fisher's views resulting from statistical analysis of mutation frequencies, are emphasised, *viz.* that although the occurrence of mutations is essential, the effective causes of evolutionary change lie in the interaction of the organism and its environment. But needless to say, it is the obvious non-moral character of the evolutionary process and mechanism of inheritance which is,- for the Bishop, a fundamental source of perplexity—" They have led to odious parasitism, to the carnage of the jungle, to the microbic diseases which cause such suffering to humanity, to those animal appetites which are useful in the struggle for survival, and are the basis of sin in man. This, the immoral, brutal, lustful side of creation is as characteristic as the parental self-sacrifice, the adventurous curiosity, the instinct for truth, the enthusiasm for righteousness, the beauty of form and the physical well-being which equally result from the evolutionary process." In the end, he can only conclude that " Its apparently non-moral character must be with His permission."

And here again, the scientist notes how hypotheses have to be multiplied in order to support a complex theory when a simpler one would suffice. The absolute minimum of instincts necessary for the behaviour of living things are those of self-preservation and reproduction. Any theory of the universe which is forced to label one ' selfishness ' and the other ' lust ', and thus make the most outstanding, universal, and obvious facts ' problems ' is clearly not satisfactory. The object of a theory is to account for the main facts—not to turn them into ' perplexities.'

In discussing the origin of man himself and his early beliefs, Dr. Barnes stresses the great importance of speech in the development of man's superiority over other animals, and insists that " since psychology was emancipated from theological prejudice, the barrier formerly thought to exist between the mental processes of man and those of ' the beasts ' has largely vanished " (p. 542).

The reader now prepares for a lecture on psychology and looks forward to seeing in what way the Bishop will manage to absorb the facts and theories of this science. It is now nearly four centuries since Galileo and Newton drove the theistic hypothesis out of the science of non-living bodies ; it is not quite a century since Darwin drove the theistic hypothesis out of the science of living bodies (the Bishop, as we have seen, would not quite agree, but admits " fundamental perplexity "), and the opposition to it has hardly died away yet ; the result has been that this hypothesis has been driven for its chief support into the recesses of the human mind. How will the Bishop treat this section of the scientific outlook ?

But the next lecture is " Scientific Theory and the Real World," followed by " God and Our Belief in His Existence," " Religious Experience," " Immortality," " Conclusion," and an appendix proving that if $a = k$ and b is a constant, the partial differential equations

$$u_{jk} = g_{jk}(au + b), \qquad j, k = 1, 2, \ldots n$$

when the coefficients g_{jk} belong to the metric of a space of constant Riemannian curvature K, admit an integral which also satisfies the equation

$$au + b = u/2\left\{-K + \frac{1}{u}\Sigma_l u^l u_l\right\}$$

Psychology, in fact, is hardly dealt with at all. The events of the external world have been submitted to 550 pages of scrutiny, analysis, and criticism, but the events of the ' internal world ' are to be taken at their face value, apparently. And yet the real nature of our motivation is just as unknown to us as the reality of the external world, and it is just as imperfectly reported to us through the data of consciousness as the external world through the indication of our sensory organs, as Freud has pointed out.

If the Bishop limits scientific experiment and theory to the external world, the well-educated man for whom these lectures are written is entitled to ask why : especially as Dr. Barnes agrees with Hume's criticisms of realism, *viz.* that what we know is actually limited to the phenomena of consciousness. Why some of the events of consciousness which seem to be connected with the external world should be singled out for careful, critical, and dispassionate analysis and the rest accepted without investigation seems to require some justification. This justification is not explicitly given, but can, in part, be found lurking in a paragraph in the portion dealing with the worth of mystical experience. " Experiment is impossible save with pathological mysticism induced by extravagant asceticism or drugs. With what we may call uncontaminated mysticism there is no possibility of creating a " uniform repetition of likenesses," to use Hort's phrase ; and such uniformity is the basis of the synthetic knowledge which science exists to organise " (p. 624).

Apart, however, from the difficulty of finding a criterion to decide when mystical experiences are " uncontaminated," this phrase of Hort's seems to limit the activities of science quite unnecessarily. Ball-Lightning is a luminous sphere which occurs somewhat rarely during thunderstorms and travels about slowly, often following the edges of objects and entering windows, etc. It usually bursts with a loud report. Its nature and formation is not completely understood, but it is a legitimate object of science, and yet there is not the least possibility at present of "creating a uniform repetition of likenesses." Indeed, this is a difficulty with which meteorologists are constantly faced, but we do not give up the application of the scientific method in meteorology just because it is difficult. Similarly, many of the facts upon which the theory of evolution is based are not even capable of repetition at all.

The fact remains that careful, critical, dispassionate observation necessary to establish facts, and theories based on the minimum number of hypotheses and constantly checked by more experimentation; in other words, the proper application of the scientific method, cannot be limited by catch-phrases such as "Knowledge is measurement" (p. 13) and Hort's arbitrary criterion.

As regards science and the 'real world,' the Bishop is, it seems, a phenomenalist, and for the full implication of this view we are referred to Kirchhoff, Mach, and Karl Pearson. Briefly, the view is that science deals only with observable sequences and statistical laws. He is led to agree with this view by the study of Hume's treatment of causality, but his attitude to Hume is a curious one, in that he is convinced by many of his devastating criticisms of causation in the physical world, and yet ignores their equal applicability to psychical causation. ("Whenever conscious agents are active, efficient causes exist.") Hume was not so naïvely anthropomorphic as to find causation by the Will of God any less obscure than physical causation.

The chapter on "God and Our Belief in His Existence" is one of the most interesting in the book. Dr. Barnes deals with some of the older proofs once held to be convincing, and then develops the Berkeleian view upon which he relies most strongly. "We pleaded in favour of a monistic view of the cosmos: it is rightly to be regarded as a unity, but as a duality in unity in which Universal Mind or consciousness is the subject and the world of our intermittent perception is the object in the fundamental subject-object relation. This position admittedly rests upon a postulate, an affirmation of faith, that such a relation of the world to Universal Mind is necessary, inasmuch as an object without a subject is unthinkable" (p. 588).

It is curious to find Dr. Barnes placing any confidence in an argument which is related much more to the structure of grammar than to that of the real world. Let us pass on to the Bishop's inferences from the fact that we make ethical judgments. How are we to eliminate erroneous judgments? "We must answer that the critical examination of its moral experience will in the end enable humanity to discover the moral law. In accordance with this postulate we construct an increasingly perfect system of ethical principles by a process of critical analysis which continuously removes the contradictions which arise from the different judgments of individuals. In the end we shall reach a system of moral principles which is objectively valid" (p. 603).

To the scientist this looks more like a pious hope than a system that compels acquiescence. Critical analysis does not necessarily remove contradictions; it may enhance them. Dr. Barnes tries hard to show that moral judgments can have objectivity, and tries to liken this to the objectivity of the laws of nature; but in this he fails signally. "It does not *purport* to be the private opinion of the subject who forms the judgment. It arises, of course, in his mind; but it has a universal nature because all who judge correctly will, *it is assumed*, reach the same conclusion" (p. 604, italics ours). Here, of course, the Bishop assumes what he sets out to prove; but this is a minor fault compared with the following paragraphs which Schoolmen of the Middle Ages could hardly have bettered:

> "Our conclusion that moral values are objectively valid is strengthened by the fact that truth has a similar character. Thus neither truth nor moral value is merely subjective. Furthermore, our belief that truth is absolute can readily be seen to be fundamental. For if it could be proved that truth had only significance for some individual, the proof would itself be a truth and its significance would therefore be similarly limited. Thus the assertion that truth has no objective validity carries within itself its own denial. From such considerations there arises the situation, embarrassing to the sceptic, that if all truth be relative the scepticism which denies the existence of absolute truth is itself relative. In brief, doubt of the absolute value of truth destroys itself. We cannot use a similar argument to demonstrate that goodness is absolute; but goodness and truth are so allied in their nature that belief in the absolute value of the one predisposes to belief in the absolute value of the other" (p. 604).

We will only remark that, before even beginning to analyse this, we should require a definition of truth, goodness, value and existing. Needless to say, in spite of the immense importance attached to these notions by the author, he nowhere attempts this. Yet definition is the starting-point of all the sciences, which is another way of saying that it is the *sine qua non* of all clear thought. That a brain that can master the first five hundred pages of this book, should be in such matters as this, in bondage to words and entangled by the chains of syntax, is sad indeed. The Bishop has failed, as all the greatest philosophers and theologians have failed before him, to make goodness and beauty objective. Opinions have varied from place to place and from time to time and show no signs of unanimity, so that to say that there is anything absolute about them seems to be unwarranted. Truth, however, is a very different question, and as the scientist is directly concerned with it, it may, perhaps, not be out of place to attempt a short defence of the attitude taken by most scientific men on this difficult subject.

The words true and false clearly cannot apply to the world itself. Giraffes, as has been profoundly observed, just *are*. It is only our knowledge about giraffes that can be true or false. Of course, there is the story of the man who, on seeing a giraffe for the first time, said, "I don't believe it!"; but such persons are not common, and we are discussing common knowledge. Our knowledge can be dissected into simple forms which are called propositions, and with care in the definition of the words making up the propositions, they can be put in such a form that they are capable of verification, *i.e.* a normal human being can adjust himself in such a way (*e.g.* by looking down a microscope, or by taking a drug, or by consulting documents, or by hearing testimony) that he will experience such events in his consciousness as the definition of the words in the propositions indicate. All definitions must ultimately refer back to events of consciousness, since it is only of these that we can be at all sure: if they do not, they cannot define objects of knowledge and have no *meaning*.

It will readily be apparent from examination of this criterion that it will not yield complete certainty. This is chiefly because it involves *inferences*, *i.e.* beliefs which are held instinctively (unconsciously) or by conditioned reflex and may never have been formulated in words at all (*e.g.* that memory is not always delusive). Nevertheless, they may be false. A *logical* inference involves words and is a proposition which is true if the propositions from which it follows logically are true. It follows logically if it is involved directly or indirectly in the definition of the other propositions, *i.e.* in their meaning. Mathematics is a branch of logic and can tell us nothing more about the world than is implicit in the definitions of the original assumptions.

Facts are propositions that have had a very high degree of verification, *e.g.* water consists of hydrogen and oxygen. Such propositions may be said to be 'true' and the degree of their 'truth' varies with the degree of verification.

Hypotheses are propositions which have so far not been verified, but might conceivably be. Propositions which cannot conceivably be subjected to verification are called Definitions or Conventions, *e.g.* a yard is the distance between two marks on a bar of platinum, etc. Hypotheses upon which important theories have been built are usually dignified by being called Principles, *e.g.* the principle of inertia, the principle of the uniformity of nature, and may, of course, be false.

Theories are sets of propositions involving hypotheses and definitions which are invented to exhibit simple causal relations between facts. The practical value of theories is that they suggest further possible propositions which, on verification, become additional facts and increase our 'true' knowledge of the world. But since theories rest on hypotheses their truth or falsity depends on whether all the hypotheses are true or not. Theories which require the least number of hypotheses are preferable, since each hypothesis, in view of the obvious limitations of human experiment and the immense complexity of the universe, is more likely to be false than true. This is the Principle of Parsimony or Occam's Razor.

Another consideration which throws great doubt on the truth of a theory, is the possibility that perhaps the same facts could be accounted for equally well, or better, by some other theory. If such a theory can be produced, use is made of its heuristic properties in suggesting new propositions which can be verified by experiment. Such propositions are chosen, as will, on verification, decide between it and former theories. If they are verified, the former theories are shown to be false or incomplete, but the new theory may still have other rivals, for the reasons given above. It is for this reason that we have to admit that *facts do not prove theories to be true*, although they may show them to be untrue or in need of modification. Nevertheless, as the difficulty in thinking of any alternative theory increases, we are led to believe more and more in the truth of a given theory. The theory of evolution is a good example of a theory which can hardly be doubted for this reason: we are much more convinced by it than by the theory of relativity or the quantum theory.

It is in such a way as this that the scientist deals with the difficult question "What is truth?" It will be noticed that clear definition of words is essential, and it is just because this has not been done in the case of goodness and beauty, that the attempt to establish something absolute about them has always attracted philosophers and theologians. None the less, a great deal of light has been thrown on how we form ideas of good and evil acts by anthropology, by the study of children, and by psycho-analysis; but this is usually ignored by philosophers and theologians. Further, it is not beyond the limits of imagination to suppose that beautiful sights and sounds may one day be shown to depend, to a large extent, on certain spatial and temporal relations. At any rate, there is no doubt that a distinction exists, for the scientist, between truth, and goodness and beauty.

Concerning religious experience, Dr. Barnes holds that religious emotion, "if it is not to be dangerous, must be severely disciplined by the reason" (p. 628). This enables him to overcome difficulties with regard to inferences from mystical experience (which he admits are most often pantheistic, or, in the highest form, mere 'blank trances'), and also to denounce, in no uncertain manner, the revival of "pagan sacramentalism." As regards immortality, he reaffirms the view that "Belief in God and belief in immortality must in the long run stand or fall together." These considerations, together with the highly mathematical appendix, bring this great treatise to an end. And if it seems that in this essay we have been unduly critical, it must be pleaded that, at any rate, Dr. Barnes does give the scientist something to criticise. Would that the same could be said of other pillars of the Church, whose works consist almost entirely of arguments of the kind into which, as we believe, the Bishop occasionally lapses, which depend on syntax and lack of definition for their effect.

Turning now to Dean Inge's Warburton Lectures, we find him defining his position as nearer to realism than to the idealists: "Reality, as I understand it, is constituted by the unity in duality of the mind and its objects. Mind does not create the intelligible world, nor is the intelligible world independent of the intelligence. The real world is a kingdom of objective "forms," which in the mind of God are the perfect counterpart of His own thought" (p. 7). To the scientist it seems doubtful whether this view is nearer to realism than idealism; in fact, he wonders if it enjoys proximity to anything intelligible. Here again, careful definition of the words is required to give meaning.

With regard to the "New Götterdämmerung" which will be brought about relentlessly by the second law of thermodynamics, the Dean is not anxious for Christian traditional philosophy, for he is convinced that "the fate of its own God is not involved." . . . "Nothing that really is can ever perish" (a very comforting phrase—but is it a definition ?—or a truism ?—or . . . ?). Recent attempts by certain astronomers and mathematicians to convince the general public that science leads to subjective idealism, are referred to as "not altogether happy incursions into metaphysics," with which dictum, scientists in general will be in hearty agreement.

And now what about the most persistent red herring that has ever been dragged across the path of clear thinking ? What, in other words, about 'value' ? It soon appears : it has a capital V and also italics. That is a brave attempt to make it look like a 'thing,' *i.e.* something which endures. "We must recognise that reality is ultimately a kingdom of Values, and that the ultimate and eternal Values, which it has been agreed to classify as Goodness (or Perfection), Truth (or Wisdom), and Beauty, are *given* to us as much as the facts of what we call existence. These Values . . . are, we may say, the contents of the divine mind as knowable by man" (p. 12). What mind, then, are Badness, Untruth and Ugliness the contents of, as knowable by man ? It really is not fair on Darwin, for whom both the Bishop and the Dean have such respect, to push them into the apelike ancestor still dormant within us. We cannot verify the hypothesis about the divine mind, but we can note that observation of the animal world does not reveal anything approaching the refined cruelty which has been observed in men. All arguments leading from goodness to one kind of universal mind lead equally from badness to another kind. The Devil has been much neglected of late.

Does the Dean attempt to define value, truth, goodness, or beauty ? Can he get any farther in this matter than the Bishop ? It must be admitted with regret that although he gives a whole lecture to "The World of Values" he fails to be convincing. For instance, we have to contend with such statements as "Existence apart from value is an abstraction" (p, 175), "the presence of the ideal in human experience is as much a fact as any other" (p. 182), "Disinterestedness is a fact which the pragmatists seek in vain to explain away : and disinterestedness implies that life itself is not the highest value" (p. 188). Failing any definition of the terms used in these *ex cathedra* propositions, criticism is impossible ; but we may remark that disinterestedness would not be considered a fact by most psychologists, or indeed, by anyone who is a careful observer of his own behaviour and that of his fellow-men. But the psychologists are put in their places, and very humble ones too, by another *ex cathedra* pronouncement, "Psychology is an abstract study, like natural science [*sic*]. The word should be confined to the science which *describes* inner experience. For it, desire and rejection are part of the neutral content of consciousness" (p. 189).

So here is the limit of science ! and for obvious reasons. Desires and rejections must not be looked into ! They must be taken uncritically, although everything else entering consciousness must be submitted to careful scrutiny.

The Dean's assertions and hypotheses break up, as they must do, on the 'problem of evil.' He admits that he has no adequate solution (p. 187): evil becomes a negative value comparable with "temperatures below freezing-point." A natural philosopher in such an *impasse* would modify his original hypotheses. He would avoid involving indefinite parochial notions of morality in his hypotheses as to the "cause" of the behaviour of living matter, just as he had to do in his hypotheses as to 'causes' in so-called dead matter. It was a 'problem' to account for 'imperfect' (non-circular) orbits in astronomy, until meaningless, irrelevant abstractions like 'perfection' were discarded. The 'problem of evil' can be avoided in a similar manner. Perhaps an analogy will help to make this clear.

Let us consider the 'problem of dirt.' Chemistry and physics have shown that all matter consists of atoms which are neither 'clean' nor 'dirty': these terms are merely relative to our personal outlook. Similarly, biology and psychology have shown that all living matter exhibits the phenomena of self-preservation and reproduction which are neither good nor bad: these terms are merely relative to our individual outlook. Thus an inoffensive atom combined with other equally inoffensive atoms can produce some of the most unpleasant odours by which the human olfactory system may be assailed: on the other hand, combined in a different way, it may produce the smell of a rose. Likewise the instincts mentioned can produce modifications which are extremely unpleasant, but equally they may produce others amongst the most highly praised by mankind. These considerations do not make cleanliness and uncleanliness any less real, but they do prevent us from idealising them and saying that the universe can be explained causally by supposing that it exists for the attainment of perfect cleanliness. We still have to fight against dirt (chiefly because of bodily pain, *i.e.* disease), and scientific knowledge shows us how to set to work. Similarly, these considerations do not make goodness and evil any less real, but they do prevent us from idealising them and saying that the universe can be explained causally by supposing that it exists for the attainment of perfect goodness. We still have to fight against evil (chiefly because of mental pain), and scientific knowledge can help us in our method in this case also. Notions as to what constitutes the criterion of cleanliness have varied from time to time and place to place, but the standard will probably continue to improve with the help of science (measured, say, by the decrease in world total of bodily pain). Ideas of what constitutes goodness have varied just as much, but may also be expected to improve with the help of science (measured, say, by the decrease in world total of mental pain).

This analogy indicates very briefly the attitude of the scientific man to the 'problem of evil,' in so far as he thinks about it impartially at all. But it must be admitted with regret that few scientific men adopt a dispassionate attitude to such matters. After all, the scientific outlook on world problems is only a few hundred years old, and is much more difficult to sustain than the theistic one.

There are several other scholarly essays in this book concerned with "The Problem of Time," "God and the World," "The Eternal World," written with Dean Inge's well-known charm and wit. We have, however, concentrated criticism on his central viewpoint, especially as, in order to maintain it, he has to deny the legitimacy of impartial scientific inquiry into the reasons why we do value some events of consciousness and not others.

We turn now to a very different book, Mr. J. W. N. Sullivan's "Limitations of Science," which purports to give a clear answer to the questions "Why has man pursued scientific adventure?" "How far has he got?" "How far is he likely to get?" This is a most readable book with many apt quotations from pioneers of the scientific method, but the author overestimates the value placed by scientists as a whole in certain theories, *e.g.* Eddington's Expanding Universe and the theory of relativity (which is stated to have been verified "exactly").

Mr. Sullivan deals, as did the Bishop, with Newton's first 'law' of motion. It is unfortunate that this hypothesis should ever have been called a 'law': it causes the writer to fall into the error of saying that scientists chose it because it was most convenient—"what scientific men mean by truth is, in the last resort, convenience. Scientific men are pragmatists in practice, whatever they may think they are in theory" (p. 59). This, of course, is utterly false and in contradiction with the author's view that scientists are influenced by aesthetic considerations. Once again, we must insist that truth can only be strictly applied to facts, and facts are often far from convenient: indeed, some philosophers use the term 'brute facts.'

The author, unlike the Bishop and the Dean, gives us a most interesting chapter on "The Nature of Mind." Here the study of behaviour of chimpanzees, of Pavlov's dogs, of the results of hypnotism and of free-association, are discussed and their implications pointed out. He agrees that the evidence supporting the hypothesis of unconscious mental processes is very convincing, and gives an outline of the Freudian theory. He concludes that psychoanalysis is in an unsatisfactory position regarded as a science, chiefly because the facts can, he says, be equally well accounted for by other theories, *e.g.* those of Jung and Adler. But this has been a defect of all sciences at one time or another, and with the discovery of fresh facts it is usually possible to decide between rival theories. Apart from this, there are general reasons for preferring Freud's central viewpoint to those of Jung and Adler. Freud is in conformity with general scientific procedure in placing his 'causes' in the past and holding that mental events are determined as are physical events in the external world. Jung adopts a vaguer and more mystical standpoint, and, so far as he is consistent, finds the causes of neurosis in the person's attitude to the future, and tends to disregard his past life. If we must invoke 'causes' to explain the observed sequence of phenomena, it is better to place them in the past than the future, for the simple reason that we can have *some* knowledge of the past. As regards Adler, who "sees the driving force of life as the urge to acquire power and superiority over one's fellows," we note that this is not as simple a hypothesis as Freud's. We have noted before that living things must exhibit at least two behaviour-patterns or instincts, viz. self-preservation and reproduction. These are involved implicitly in the definition of the word 'living.' If, then, we can trace human motivation back to these two essentials, a theory built on them will be preferable to one involving an extra, non-essential, and subsidiary 'urge' such as the one used by Adler. Nevertheless, instincts related to herd life may exist, but it seems highly improbable that they will not be modifications of the two fundamental instincts mentioned.

The author gives an interesting account of the different opinions which some pioneers of science held as to what its method should be and what was gained by its pursuit. He is led astray by the writings of some recent spokesmen of science into asserting that all we know about electricity, for instance, is its mathematical structure, and then leads up to Eddington's view that all exact knowledge consists of "pointer-readings." That, at one stage in the life of all vertebrates, the cavity behind the mouth and nose is pierced on both sides by gill-slits, is most important exact knowledge, but it is not a 'pointer-reading.' It is well to remember the robust Dr. Johnson who, attempting to refute idealism, struck his foot against a stone so mightily that he rebounded from it, exclaiming, "I refute it thus!" Whatever events occurred in his consciousness as a result of this vigorous action, we can be certain of one thing; they were not concerned with mathematics or structure. Again, those who say that all we know about electricity is the way it affects our measuring instruments, forget that in early experiments, without which we should have no knowledge at all, the investigator himself was often the instrument. When the Hon. Henry Cavendish was making experiments which demonstrated what was later known as Ohm's Law, the galvanometer *was the Hon. Henry Cavendish.*

Mr. Sullivan goes on to take Eddington's 'world-building' on nothing but indefinable 'point-events,' seriously without noticing that Eddington has regressed into the Dark Ages by adopting the deductive instead of the inductive method. With the demerits of such a method we have attempted to deal elsewhere.[1]

Another general criticism of scientific method is that it commences with analysis. We should, it is advocated, try to look upon complex systems as 'wholes' or 'organisms.' Unfortunately, analysis is forced upon us for the simple reason that we can only concentrate consciousness on one event or series of events at once. We cannot even explain the working of a Rolls Royce as a 'whole'; we are forced to analyse, to deal with it part by part. It may well be that we should understand better certain problems, especially in biology and ones involving the functioning of the brain, if we could look upon these as 'wholes.' But at present we cannot do so, and not the least important knowledge we can have, is that of our own limitations.

Another great limitation of science is often said to be that its theories do not ultimately answer the question 'Why?' But a little consideration will show that the disintegrating effect on theories of the question 'Why?' depends merely on the persistence of the interlocutor. Even in the sphere in which the question has any meaning at all, *viz.* that of the origin of human motivation, it has only to be asked a few times in succession to show us that we do not know, ultimately, why we do or think anything. These facts alone should make us doubt whether the question has any ultimate meaning, *i.e.* whether the notion of 'purpose' applied as a 'cause' of the events of the universe is not possibly a false hypothesis.

In attempting to show, as has been done here, that scientific method is the only method by which we can hope to get any true information about the world inferred from the events in our own consciousness, we have had to admit that facts can never prove theories but only disprove them. Nevertheless, these theories are useful in leading us to the discovery of more facts, and thus our true knowledge of the world is increased and false beliefs are eradicated.

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[1] "Modern Physics and the First Principles of Science," SCIENCE PROGRESS, No. 106, 1932.

"What is thinking and how do we think? We think with words; that alone is sensual and brings us back to nature. Think of it! A metaphysician has only the perfected cry of monkeys and dogs with which to construct the system of the world. That which he calls profound speculation and transcendent method is to put end to end in an arbitrary order the natural sounds which cry out hunger, fear, and love in primitive forests, and to which are attached, little by little, the meanings which one believed to be abstract, when they were only crude. Do not fear that the succession of small cries, feeble and stifled, which compose a book of philosophy will teach us so much regarding the universe, that we can live in it no longer." So said Anatole France (*Le Jardin d'Épicure*) and we can only humbly agree.

Gibbering then, in the fitful sunshine of a second-rate star, burning away in a dying universe, we anthropoids must build our philosophies as best we may. Of the precautions that must be taken in order that the result may not entirely be gibberish, we have here essayed this brief outline.

REVIEWS

MATHEMATICS

Higher Mathematics for Engineers and Physicists. By Ivan S. Sokolnikoff, Ph.D., and Elizabeth S. Sokolnikoff, Ph.D. [Pp. xiii + 482, with 121 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 24*s*. net.)

A course of lectures regularly given in the University of Wisconsin to students of engineering and applied sciences forms the basis of this book. The assumption is that such students are familiar with the elements of the calculus, but not mature enough to be introduced to advanced and specialised works. The authors do not fail to define their purpose as "bridging the gap" and as "a stepping-stone." So far as the engineers are concerned, this purpose may perhaps be achieved in American institutions, but in this country the routine of courses has no place for such refinements, and it is doubtful if much use could be made of this book. For physicists and chemists, on the other hand, from whom a greater knowledge of mathematics is now expected than was the case not more than a generation ago, it contains much useful matter.

The contents of the book may best be described by quoting the chapter headings: Elliptic Integrals, Solution of Equations, Determinants and Matrices, Infinite Series, Partial Differentiation, Fourier Series, Multiple Integrals, Line Integral, Improper Integrals, Ordinary Differential Equations, Partial Differential Equations, Vector Analysis, Probability, Empirical Formulas and Curve Fitting, Conformal Representation. Each chapter was made, perhaps unwisely, as far as possible independent of the others. The arrangement is in general not good; for example, hyperbolic functions do not appear before p. 220, where they are intruded into the chapter on Ordinary Differential Equations. There are numerous and valuable examples in the text, but unfortunately they are in the same type as the rest of the matter, which hinders the use of the book as a work of reference.

Mathematically, the book is as sound as a text of this type need be. Difficulties are not merely ignored. There are a few printer's errors, but none likely to worry the reader.

E. L. I.

Elementary Dynamics for Students of Science and Engineering. By Robert Cochran Gray, M.A., D.Sc., Companion I.E.E. [Pp. vii + 211, with 94 figures]. (London: Macmillan & Co., Ltd., 1934. 5*s*.)

The purpose of this book is to introduce students beginning a University course in engineering or applied science to the elements of dynamics without the use of the calculus. That such an introduction should be necessary is a weighty indictment against an inefficient educational system, which considers a man with no calculus and no dynamics fit material to be turned in three years into a scientific engineer or technologist.

Nevertheless, the book seems admirably designed for its purpose, though if the calculus is to be dodged, its notation should have been avoided. The use of $\dot{x}$ and $\ddot{x}$ as mere symbols for velocity and acceleration may be expected to lead to trouble later on; experience tells that few people are less skilled in symbolism than the "practical man" type of engineering student. Moreover it is doubtful if the mathematical equipment of a reader who cannot differentiate an integral power would fit him (as it is called upon to do on p. 78) to understand the exponential limit.

The four chapters on kinematics, five on particle dynamics and seven on rigid dynamics form a course as exhaustive as is possible with the readers' supposed limitations The arrangement is good and the style everywhere lucid. The examples, numbering some 400, are mainly of a practical or numerical type.

The book contains many things which every schoolboy might know with advantage, and some things of which few are aware. Man as a machine is notoriously inefficient, but that his "output of mechanical power is not worth more than *5d.* per week" (p. 99) will surprise and shock many of us.

E. L. I.

PHYSICS

The Architecture of the Universe. By W. F. G. SWANN, A.R.C.S., M A., D.Sc. [Pp. xii + 428, with 23 figures.] (New York and London: Macmillan & Co., Ltd., 1934. 16*s*. net.)

THIS book is based on a course of lectures on the philosophy of physics given recently by the author in Ohio State University. The subject matter includes the quantum theory and wave mechanics, relativity and cosmology, the nature of life and the relation between science and theology. The book is intended for the educated layman with no special knowledge of physical science, and consequently much space is devoted to the statement and explanation of the results of modern theoretical physics, as well as to their philosophical implications. The author also explains in non-technical language many of the more elementary concepts of classical physics; for instance, he explains the difference between "temperature" and "heat." While some parts of modern physics can be made intelligible to people who do not know such things already, the reviewer very much doubts whether wave mechanics can, and feels that the book would have more value if addressed to the sufficiently large body of readers, comprising technicians, schoolmasters and other people with some scientific training, who alone will be able to understand most of it. The exposition is mainly by analogy, and is often witty; the present reviewer will always remember with pleasure, in connection with the photoelectric effect, that "the atom is like a cat; you may torment it, and it may do nothing, but the chance of getting scratched is proportional to the annoyance of the cat." Delightful as this is, it is to be feared that it is the chief impression of the atom that the entirely lay reader will carry away.

In the philosophical parts of the book the point of view of that unfortunate experimental physicist, who wants to make a mechanical model of the atom and of most other things, is very effectively demolished. The uncertainty principle of Heisenberg is discussed, and the question as to whether the electron really exists; the point is made that electrons and electron waves are both merely convenient terms with which to describe the results of experiments. While agreeing in the main with Dr. Swann's contentions, the reviewer thinks that too much emphasis is laid on the convenience and elegance of the new theories, and the fact that they have been able to predict hitherto unsuspected phenomena is left too much in the background.

N. F. M.

Atomic Theory and the Description of Nature. By NIELS BOHR. [Pp. vi + 119.] (Cambridge: at the University Press, 1934. 6*s*. net.)

TWELVE years ago the Cambridge University Press published three lectures in English by Niels Bohr under the general title *The Theory of Spectra and Atomic Constitution*. The first of them had originally been delivered in 1913. Now, in a companion volume, four lectures are reprinted which belong to the period 1925–29. No attempt has been made to bring these lectures up to date; in their original form they describe successive stages in the gradual development of the quantum theory of the atom. However, a third volume is promised shortly which is to follow this development to more recent times.

It is instructive to compare the essays in the first and second series, respectively. In the earlier essays the emphasis is on the achievements of the theory; the later ones are more philosophical in outlook and in their working out the limitations of the epistemology appear stressed rather than its completeness: "In this respect, the quantum theory may be said to be a disappointment." Yet there is one conclusion, admirably expressed, which qualifies any such disappointment over the failure of a thoroughgoing causal interpretation: "Perhaps the most distinguishing characteristic of the present position in physics is that almost all the ideas which have ever proved to be fruitful in the investigation of nature have found their right place in a common harmony without thereby having diminished their fruitfulness."

This collection of essays abounds in examples of such felicity of phrase; as concerns its subject matter, it would obviously be impertinence to pretend to criticise.

N. F.

Wave Mechanics, Advanced General Theory. By J. FRENKEL. The International Series of Monographs on Physics. [Pp. viii + 525, with 3 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1934. 35*s*. net.)

THIS volume is the second part of a work consisting of three volumes; the last of which is not yet published. Students of modern physics will be familiar with the first volume which was devoted to the general ideas underlying the new theories and was written for the beginner. The present volume, although intended to be complete in itself, requires some previous knowledge and is rather of the character of an advanced text-book. It is devoted to the mathematical development of the general ideas and stresses the connection with the classical theory. In fact, the author appears almost to make the subject emerge from the older theories.

It is probable that the book contains the most complete account of the subject which has yet been published and is therefore of great value to specialists in this part of physics while, as a work of reference, it is valuable to all. It is somewhat of a contrast to another volume with a similar title appearing in this International series of Monographs—the principia of the subject by Dirac. The content and purpose of the two volumes are of course different, but one leaves Dirac's work with a sense of intellectual satisfaction; there is something of a satisfactory philosophy and something clear cut in his presentation of the subject. It is not possible to say this of the volume now under review.

It is interesting to seek the cause of this difference, for Frenkel's book is certainly complete in the subjects with which it deals and it treats them with a faithful attention to detail, sometimes with repetition and with too great a stress on the finer points. The cause is undoubtedly the overstressing of the classical analogy. It is not altogether easy to say what physical relation lies in this analogy, but mathematically the connection is one of form. Dirac recognised this in his earliest writings on the subject, but his greatest success would appear to lie in his ability to break away from the old obsessions to a greater extent than any other writer. Too much analogy with classical theories becomes in the end almost an apologia for the new. This is not to say that this analogy is unimportant; that would, in fact, be historically untrue; but it is important for all who work in this field to try to avoid too many glances backward.

One of the author's contributions to the subject was his demonstration of the analogy between the quantum equations and those of Maxwell. His work is given in this volume under the title "Relativistic Wave Mechanics as a Formal Generalisation of Maxwell's Electromagnetic Theory of Light," and, later in the chapter, Dirac's equations are obtained by an identification with the results of this generalisation. Here again we have an example of classical analogy; but the results, so far, of attempts to generalise Maxwell's equations seem to be negative in character. Without going so far as to say with some authorities that the results of such investigations have contributed "exactly nothing" to the subject, it is doubtful if the method of presentation is an improvement on that of Dirac's original paper. It certainly introduces a reader steeped in the classical doctrine more gently to the basis of the theory, but he probably loses something in the process.

This particular chapter is a long one and could have been further shortened by a different presentation of the matrix theory. This is given in three stages. Pauli's theory, more exact theory, and Dirac's exact theory. It would have made better reading if the treatment had been given under the third of these titles with such references to the earlier attempts as were necessary.

These criticisms are of method of presentation not of the matter presented, which is complete, accurate and given with extreme care. The book is a very important contribution to the literature of the Quantum Theory and has great value as presenting the point of view of one who has made important original contributions to the subject on which he writes and who has remarkably clear insight into the problems of this realm of physics. We must, finally, pay a tribute to the style of the book and to those who helped the author over the difficulties of language.

H. T. F.

Introduction to Modern Physics. By F. K. Richtmyer, Professor of Physics at Cornell University. International Series of Physics. Second edition. [Pp. xviii + 747, with 6 plates and 221 figures.] (New York and London : McGraw-Hill Book Co., Inc., 1934. 30*s*. net.)

The first edition of this book was published six years ago and in the intervening period there can be few teachers who have not recommended it to their advanced students as an ideal approach to an honours course in modern physics. The new edition will make an even greater appeal for, in addition to a thorough revision of the original material, 150 pages have been added to bring it up to date without sacrificing anything of the " historical sketch " which forms so valuable a part of the work. There are two new chapters dealing respectively with the Vector Model of the Atom and with the Wave Theory of Matter, and two new appendices, one of which deals with Group Velocity, while the other contains a simple mathematical treatment of the Special Theory of Relativity leading finally to the fundamental relation between mass and energy.

Unfortunately the author has again found it necessary to omit topics which his readers would have liked him to discuss, and the index contains no mention of Cosmic Rays, Raman or Gamow. The last pages of the chapter on the Atomic Nucleus deal with the neutron and the positron ; it was written before the new views on nuclear structure had crystallised and, in consequence, assumes that the nuclei themselves are built up of protons and electrons. Thus this section already needs revision albeit it still serves as an excellent introduction to the later developments.

D. O. W.

The Physics of Electron Tubes. By L. R. Koller, Ph.D. [Pp. xiii + 205, with 67 figures.] (New York and London : McGraw-Hill Book Co., Inc., 1934. 18*s*. net.)

The phrase " electron tubes " in the title of this work covers thermionic valves (including gas discharge relays) and the various types of light-sensitive cell. The book does not deal with the external circuits used in the application of such devices but with the physical phenomena involved in their internal functioning. It is essentially a summary of known results in their practical aspect rather than a treatment of the methods and problems of the research worker. The first four chapters deal with the thermionic emission from various types of cathode. Methods of measuring the cathode temperature are then summarised and the phenomena of secondary emission and " clean-up " of gases are described. The chapter on space charge which follows might well have been augmented by a fuller treatment of the action of the grid and a discussion of the effect of the shape and dimensions of the electrodes generally upon the characteristics of a valve. A chapter on discharges in gases leads up to a consideration of the grid-controlled arc, and there are three chapters on light-sensitive cells, including the selenium and cuprous oxide types. Three mathematical appendices and a set of numerical problems and answers are included. It is disappointing to find no reference to the cathode ray oscillograph, which is well within the scope of the title.

As an introductory work for students or a summary for those whose main concern is with the technical application of the devices described, the book fulfils a definite function in a satisfactory manner. Its inclusion in the International Series in Physics, however, might lead the prospective reader to expect a much more advanced and comprehensive treatise.

N. L. Y.-F.

Resonance Radiation and Excited Atoms. By A. C. G. Mitchell, Ph D, and M. W. Zemansky, Ph.D. The Cambridge Series of Physical Chemistry. [Pp. xvi + 338, with 84 figures] (Cambridge: at the University Press, 1934. 18*s*. net.)

This book constitutes a valuable compilation and critical presentation of both experimental and theoretical work in the field of resonance radiation and excited atoms. Both the authors have made important original contributions to the progress which has taken place in this branch of physics within the last eight years or so, and are therefore justified in attempting the task of handling the subject matter primarily from a critical point of view rather than from a purely historical one. They have faithfully and successfully carried out their purpose which, as they explain in the preface, was to remove conflicts in notation, experimental method, and evaluation of results, by the adoption of a systematic notation and a unified point of view compatible with modern quantum theory, in the presentation and discussion of the experimental work that has been performed. They have shown clearly the difficulties confronting the experimentalist and the extent to which it has been possible recently to arrange experimental conditions so that the theories which have been developed can, in spite of their well-defined limits of application, be employed in the interpretation of experimental results. The particular directions in which there is scope for yet more experimental work are emphasised.

The tracing of original papers dealing with particular aspects of the subject matter is facilitated by the numbered references throughout each of the five chapters into which the book is divided and by the further detailed subdivision of each chapter under definite headings.

The book is likely to prove valuable to any physicist or chemist whether a specialist in this particular field of investigation or in any other.

With the exception of Table III which can only be found by turning on twenty-four pages from the first reference to it on p. 17 of the text, the various figures and tables are very conveniently arranged in relation to the textual references to them.

A. C. D.

Thermionic Emission. By Arnold L. Reimann, B.Sc., Ph.D., F.Inst.P., a Member of the Research Staff of the General Electric Co., Ltd., and of the M.O. Valve Co., Ltd. [Pp. xi + 324, with 64 figures and 19 tables.] (London: Chapman & Hall, Ltd., 1934. 21*s*. net.)

That branch of physics which deals with the emission of charged particles from hot surfaces has seen, during the past few years, not only a steady increase in variety and precision of experimental data, but a rapid—one might almost say sudden—change of front in their theoretical interpretation. Quantum statistics have suggested that the conduction electrons within a metal, which are the ultimate source of the observed thermionic electron emission, have velocities far greater than had previously been suspected, while the advent of wave-mechanics has revolutionised our ideas as to the manner in which these electrons escape through the intense though localised electric fields which occur at the emitting surface.

A much more satisfactory account of thermionic phenomena has in consequence become possible, and the time was clearly opportune for the appearance of a comprehensive and authoritative discussion of the whole subject. Dr. Reimann has turned the opportunity to very good account and, keeping strictly to the limits of his subject, has given us in three hundred pages, and without any undue compression, a remarkably complete work covering both experiment and theory.

The general survey which constitutes the first chapter may be recommended to any physicist who wishes for a clear outline of the subject; the remaining five chapters, which will appeal more particularly to the specialist, are devoted to electron emission from clean metals, from contaminated metal surfaces, and from oxide coatings; the theory of electron emission and, finally, the thermal emission of ions. The discussion is throughout of a critical nature and is supplemented by valuable tables and curves of numerical data and by plentiful references to original papers.

The general standard of precision and clarity is extremely high, though one may note an exception on p. 58, where the same symbol i_A denotes successively a current, a current per unit length and a current per unit area.

It is a pleasure to congratulate Dr. Reimann on a book which has every prospect of becoming the standard modern work on thermionics.

P. B. M.

The Practice of Absorption Spectrophotometry with Hilger Instruments.

By F. Twyman, F.Inst.P., F.R.S., and C. B. Allsopp, M.A., Ph.D. Second edition. [Pp. 140, with 46 illustrations.] (London: Adam Hilger Ltd., 1934. 12*s*. 6*d*. net.)

Although Mr. Twyman's first edition of this book appeared as recently as 1932, the rapid growth of the subject has rendered necessary this new edition, thoroughly revised and much enlarged. The book is now divided into two parts. In Part I Dr. Allsopp contributes new material relating to the theory and applications of light absorption, a simple description of molecular spectra including predissociation and the Raman effect, and brief accounts of some applications of absorption spectroscopy in theoretical, industrial and biological chemistry. The spectro-photometric nomenclature of the International Critical Tables (1929) is adopted, and the meanings of the terms *density*, *extinction coefficient*, *molecular extinction coefficient*, *absorption coefficient* and *absorption index* are clearly stated; the statement is particularly necessary since lack of precision has led to much confusion and rendered many past investigations meaningless. In Part II Mr. Twyman describes the technique of absorption photometry, the conditions for accuracy of the measurements it involves, and modern Hilger instruments for ultraviolet, visible and infra-red spectrophotometry. Amongst the subjects treated in the appendices are the use of Beer's Law in the analysis of absorbing solutions, particulars concerning photographic plates, spectroscopic investigations of poisons and the control of purity in foodstuffs by absorption and by emission methods, and absorption in a diffusing medium. The book is very well illustrated by spectrograms, photographs of apparatus and line diagrams. The name of *Rasetti*, two of whose beautiful rotation Raman spectrograms are excellently reproduced, appears wrongly as *Rosetti* on p. 37.

The book will, no doubt, be found invaluable to those about to take up absorption work and of great interest to those already engaged in this work and to advanced chemistry students generally.

W. J.

CHEMISTRY

Arsenical and Argentiferous Copper. By J. L. Gregg. American Chemical Society Monograph Series. [Pp. 189, with 56 figures.] (New York: The Chemical Catalog Company, Inc. 1934. $4.00.)

This book is an American Chemical Society Monograph issued by The Chemical Catalog Company, Inc., of New York. The reason for its publication is the desirability of collecting all the available information on the subject of arsenical and argentiferous copper in one volume to be easily accessible to metallurgists and users of copper.

Following a short historical section, the properties and uses of copper are discussed, more attention is rightly given to the physical than to the chemical properties, for the latter are to be found in many standard works, whilst the former are more or less scattered through the literature. A short account is also given of the geology and metallurgy of copper and mention is made of its casting and mechanical working.

The Copper-oxygen, Copper-arsenic, Copper-silver systems and complex alloys are explained in detail.

The most important chapters of the book are those devoted to the electrical, thermal and mechanical properties of arsenical and argentiferous copper. A large number of graphs and tables illustrate these properties and the information given should be of great value to metallurgists interested in or working on the subject.

There is a chapter dealing with the corrosion of arsenical copper and brass in which it is pointed out that much of the best literature pertaining to this subject is to be found in the *Journal of the Institute of Metals*; indeed, a very pleasing feature of the book is that it is not confined to American practice but that full reference is made to investigators in all countries.

In response to requests from fabricators and consumers of copper, the book contains a chapter on the uses of arsenical and argentiferous copper, in which the data stated or implied in earlier chapters is brought together in systematic form.

An appendix gives useful tables concerning production and prices of copper alloys and tables referring to various A.S.T.M. specifications.

The book ends with a complete bibliography and should prove a very useful volume.

E. W. Yeoman.

Conductometric Analysis. By H. T. S. Britton, D.Sc., B.Sc., D.I.C., F.I.C. Vol. VIII of a series of monographs on Applied Chemistry, edited by E. Howard Tripp. [Pp. x + 178, with 49 figures.] (London: Chapman & Hall, Ltd., 1934. 12*s*. 6*d*. net.)

The exceptionally rapid development of potentiometric titration methods in recent years has tended to overshadow the less spectacular progress which has been made in the application of conductometric methods to chemical analysis. Nevertheless, this progress has been considerable, and the work of Dr. Britton in particular has shown the value of such methods as a complement to potentiometric methods in the investigation of a number of physico-chemical problems. The general principles of conductometric titration as applied to the neutralisation of strong acids and bases and to reactions in which sparingly soluble substances are precipitated are well known, but in the present volume these principles are extended to elucidate the forms of the curves obtained with weak acids and bases, polybasic acids and polyacid bases, mixtures of acids and of bases, and with various special types of reaction, such as those in which metallic hydroxides are precipitated, complex salts formed, etc. Modern developments in apparatus and technique are discussed in a clear and practical manner, and the various types of titration are illustrated by reference to actual experimental data and curves. Two final chapters deal with some examples of the application of conductometric methods in the investigation of physico-chemical problems and in the control of industrial processes.

Dr. Britton has produced a very readable, interesting and practical guide to the principles, methods and applications of conductometric analysis which should prove of great value to workers in many fields. The numerous diagrams are informative and clearly reproduced, and full references are given to the original literature.

H. J. T. E.

The Fundamentals of Chemical Thermodynamics. Part II. By J. A. V. Butler, D.Sc. [Pp. ix + 271, with 43 figures.] (London: Macmillan & Co., Ltd., 1934. 8*s*. 6*d*.)

A TEXT-BOOK which can be carried in the pocket is especially valuable, for it has a better chance of being read, and in these circumstances of close mutual contact may be unconsciously assimilated. This compact little volume should therefore commend itself to students. Part I appeared six years ago but is not an indispensable companion since Part II can be read by anybody possessing an elementary acquaintance with the Second Law. Three quarters of the book is occupied with the thermodynamical properties of aqueous salt solutions. There is a long account of surface-equilibria and an introduction to the Phase-Rule. Nothing seems to call for special comment in the early chapters (Functions; Gas Reactions; Third Law) unless it be the omission on p. 48 to refer to the important monograph by Parks and Huffman entitled *The Free Energies of Some Organic Compounds*. Exercises are set at the end of each section.

The Debye-Hückel theory is dealt with in the usual manner, though at relatively greater length than its limitations would seem to warrant, and it is a little disappointing that a more adequate account of the recent ramifications of the theory is not forthcoming. This is not a general criticism, however, for most of the material is up to date and numerous references are made to the literature of the past two or three years. The author does not much concern himself with statistical mechanics nor at all with the alliance of thermodynamics and spectroscopy. It is to be hoped that he will be tempted to do so in Part III.

C. H. J.

Free Radicals. A General Discussion. [Pp. iv + 248, with 5 plates and numerous figures.] (London: Gurney & Jackson, for the Faraday Society, 1934. 12*s.* 6*d.* net.)

THIS volume contains the 29 papers and the discussions which took place at the meeting of the Faraday Society in Cambridge last year. The papers are divided into three groups: (1) Free radicals of relatively long life, (2) Free radicals of short life, chemical aspects, and (3) Free radicals of short life, physical aspects, and the topics considered covered a wide range. Thus Part I contains *inter alia* papers on the chemical factors which stabilise free radicals and a wave-mechanical theory of the triphenyl methyl type of radical. Part II includes contributions on the formation of methyl and ethyl and the part they play in thermal and photochemical changes, whilst Part III is concerned with the production of radicals in electric discharges and the spectroscopic phenomena to which they give rise.

There is no need at the present time to emphasise the valuable service to physics and chemistry which the Faraday Society performs in organising and publishing its well-known general discussions. It is sufficient to say that the present volume is a worthy addition to the series of discussions already published.

S. S.

Electrokinetic Phenomena and their Application to Biology and Medicine. By HAROLD A. ABRAMSON, M.D. American Chemical Society Monograph Series. [Pp. 331, with frontispiece and 106 figures.] (New York: The Chemical Catalog Company, Inc., 1934. $7.50.)

DR. ABRAMSON's book is divided into the following chapters: I, Historical. II, Early Theory and Related Experiments. III, Methods. IV, Recent Theory and Related Experiments. V, Proteins and some Related Compounds. VI, General Effect of Salts on " Inert " Surfaces. VII, Inorganic Surfaces. VIII, Organic Surfaces. IX, Gases. X, Blood Cells, Spermatozoa, Tissues, etc. XI, Bacteria, Antibodies, Viruses and Related Systems.

The treatment of the whole is very consistent and in so far in every way superior to that of the monograph by Prausnitz and Reitstötter, published a few years ago. Abramson uses the conception of an electrokinetic potential, a ζ-potential, throughout. In a future edition it might be advisable to discuss antagonistic views like those of McBain more thoroughly, although they do not seem to be very well founded. The electrical charge of the surface, calculated on the ground of the Debye-Hückel theory, is taken into account too. In some cases it leads to simpler correlations, yet the stability of colloidal systems appears mainly to depend upon the ζ-potential, not upon the electrical charge.

The book is rich in well-selected details which make it interesting and most suggestive reading. It is somewhat surprising that the behaviour of proteins and especially of surfaces covered with layers of protein is often more easily explained than that of inorganic surfaces. A good review is given of the greatly increased literature concerning the electrokinetic phenomena found with blood cells, spermatozoa, bacteria, etc. These phenomena are very complicated; it is nevertheless obvious that the investigation of the ζ-potential belongs to the most important methods of attacking the problems concerning the structure and chemical composition of surfaces. For it is not only the sign of the electrical charge which is characteristic, but also a number of other factors: the sensitiveness of the ζ-potential towards the *p*H, the existence of well-defined isoelectric points, the slope and shape of the ζ-*p*H-curves; the ease with which the surface is covered by a protein; the stability of suspensions and its correlation with the electrokinetic potential, etc.

All who are interested in capillary chemistry and biology will find Abramson's monograph most valuable.

H. FREUNDLICH.

Manometric Methods as applied to the Measurement of Cell Respiration and Other Processes. By MALCOLM DIXON, M.A., Ph.D., Sc.D. With a foreword by SIR F. G. HOPKINS. [Pp. xii + 122, with 19 figures.] (Cambridge: at the University Press, 1934. 5*s*. net.)

QUANTITATIVE study of chemical and biological reactions in which a gas is either absorbed or evolved, by causing the reaction to take place in a closed vessel attached to a gauge tube containing liquid, has become common. The method is essentially simple, but accuracy depends on practical details, of which Dr. Dixon has had twelve years' experience. He has written this small handbook to help beginners in the technique, and though essentially practical in character, the theory underlying the methods is described in the first part. The book is clearly illustrated, and should fulfil well its limited purpose.

P. M. T. KERRIDGE.

Annual Review of Biochemistry. Vol. III. Edited by JAMES MURRAY LUCK. [Pp. viii + 558.] (Stanford University, California: Stanford University Press; London: H. K. Lewis & Co., Ltd., 1934. 25*s*. net.)

THIS is the third of a series of annual reviews of the progress of biochemistry and it fully maintains the high standard set by the two preceding volumes. The policy has been continued of inviting contributions on various sections of the subject from those scientific workers who, on account of their own original contributions and their intimate knowledge, are best fitted to deal with the subject and so the review has a truly international flavour. Amongst the many valuable contributions, it would be invidious to single out one or two as particularly outstanding, but, as an interesting example of how an exceedingly complex subject may be presented clearly and simply, by skilful selections of the essentials, the article by Rosenheim and King on the structure of the sterols and the bile acids may be cited. The sensational developments during the past two or three years, for which these two workers have themselves been largely responsible, must have interested, and at the same time perplexed, many chemists whose knowledge of this branch of the subject had only been of a general character, and the lucid presentation now before us will be welcomed by many.

In passing, it is interesting to note that fully one-sixth of the book, which contains twenty-six separate contributions, deals with what may be called structural organic chemistry and this is no doubt as it should be. Amino-acids and proteins, fats, animal pigments, and the terpenes are amongst the subjects which are dealt with from this point of view. Naturally, the majority of the contributions deal with the separate biochemical processes which form the basis of the life of plants and animals, and most of the topics which one would naturally expect to be included will be found in one or other of these various articles. It is not surprising to find that enzymes and vitamins, which have received so much attention of recent years, are referred to by quite a number of the contributors, for these two classes of substances play a very important role in vital processes of almost all living things.

W. O. K.

Introduction to Physiological Chemistry. By Meyer Bodansky, Ph.D Third edition. [Pp. x + 662, with 39 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 25s. net.)

The popularity of Professor Bodansky's book is attested by the fact that it has already reached its third edition and that both previous editions underwent several reprints. The last few years, however, have witnessed numerous advances in Biochemistry of first-rate importance and so it has been found necessary, not only to incorporate new subject matter, but to rewrite a considerable portion of the whole book. The general plan and style of treatment has been preserved and the author aims at giving a fairly comprehensive but not unduly profound survey of the subject, the student being referred, for fuller information on particular branches of the subject, to various monographs and original papers. It is almost impossible in a book of this nature to satisfy all critics. Some might regret, for instance, that there is no chapter or section devoted to the biochemistry of muscular contraction, the various aspects of the subject being found in chapters on carbohydrate metabolism, fat metabolism, creatine, etc. Most of the facts seem to be given, but the ordinary student might have some difficulty in forming a comprehensive picture. However, the best type of arrangement is, no doubt, a matter for individual judgment. The fastidious critic might likewise find fault with certain points of detail. For example, whatever may be the relative merits of phosphocreatine and creatine phosphoric acid to denote the compound of creatine and phosphoric acid in muscle it would seem to be as well in a short section on the subject to use the one name consistently and not to employ both, but still more reprehensible would seem to be the replacement of those two names by "creatine phosphate" (apparently a hybrid between creatine phosphoric acid and sodium creatine phosphate), which the intelligent student might well imagine to denote a simple salt of creatine and phosphoric acid. In spite of these details the book gives a very useful and readable survey of Biochemistry and no effort seems to have been spared to bring it up to date.

W. O. K.

GEOLOGY

Tertiary Faunas. A text-book for oilfield palæontologists and students of geology. Vol. II: The Sequence of Tertiary Faunas. By A. Morley Davies, D.Sc., A.R.C.S., F.G.S. (Pp. x + 252, with frontispiece and 28 figures.) (London: Thomas Murby & Co., 1934. 15s. net.)

Since William Smith's original discovery that strata could be identified by the organised fossils they contained, the division of geological time into Eras, Periods and Epochs has been carried on to a more and more detailed subdivision, primarily on a palæontological basis.

These subdivisions, first made in some small area, have often been shown to be recognisable beyond its borders and have gradually been extended until in some cases they have become world-wide.

The possibility of such extensive correlations depends on the occurrence in the rocks under consideration of animals whose characters rapidly change by evolution, and which have a very extensive geographical range. In Palæozoic time the graptolites met these requirements admirably, and the goniatites and ammonites are as satisfactory for late palæozoic and mesozoic times. But their disappearance at the end of the Cretaceous left no group of animals which combined these qualities.

In consequence, the subdivision of the Tertiary rocks into small groups has always been unsatisfactory; the still-continuing uncertainty of the ages of the marine Tertiary deposits of Patagonia, Australia and New Zealand stands in sharp contrast to our relative assurance of the correlation of the Palæozoic and Mesozoic deposits of these areas.

The difficulties of Tertiary palæontology arise from many factors. The period probably covers a much shorter period of time than the Mesozoic, so that its subdivisions are correspondingly shorter, and a group of animals to be of value for zonal purposes in Tertiary rocks must have a very rapid evolutionary rate. Experience has shown that the mollusca, which are the most abundant of all Tertiary fossils, are unsatisfactory, in part because of the relatively long continuance of the species, in part because their occurrence is largely determined by ecological conditions and because their geographical range is commonly small. The echinoids are better, but whilst locally abundant they are usually rare, each species being restricted to rocks of a definite facies; very unexpectedly the foraminifera prove to be amongst the best time-markers for those regions where the large members of the group lived.

For land deposits the mammals provide excellent evidence, the more so because so much of their evolutionary history is known; but the marine mammals, whales, seals and sirenia are too rare to be of use, and can in any case only be identified from nearly complete skulls. The ideal groups of marine animals such as Copepods for example, are not capable of preservation as fossils.

The study of Tertiary palæontology is thus one of great difficulty because, to a greater extent than that of any other era, it demands very careful consideration of the effects of "facies," and an evaluation of the changes in fauna produced by migration from other zoogeographical provinces.

Dr. Morley Davies' book includes a short but admirable account, founded on the work of Peterson and his followers, of the variations in fauna, depending on differences in depth and in the character of the bottom, which may occur within very short distances in the European seas. Then he passes on to discuss the Zoogeographical regions of the present day as they exist on land and in the sea; giving us by far the best short account of the facts which exists.

The bulk of the book consists of a systematic treatment of the Tertiary rocks of all parts of the world, to which a discussion of the end of the Cretaceous period and of the rocks which have been regarded as transitional between the secondary and tertiary eras acts as an introduction.

For each sub-division of Tertiary time we are given a condensed, but very intelligible, account of the fauna of each important region, with an indication of the facies represented and a discussion of its zoogeographical significance. These accounts deal with the mammal faunas of land deposits as well as with marine invertebrate faunas. Each section is followed by a very extensive bibliography.

The work as a whole is far more than a text-book, it is really an original treatise of the first rank, founded on an immense range of first-hand knowledge of facts and a familiarity with a singularly unprepossessing and extensive literature.

It gives for the first time a readable and comprehensive discussion of the Tertiary period as a whole so far as it is represented by fossiliferous rocks, and is distinguished by its sanity and judicious outlook as much as by the wide range of its correlations and interpretations.

It will be of the very greatest value, not only to those students for whom it is intended but to all who need a guide through the tangled literature of the Tertiary period, for such purpose as the discussion of the past and present distributions of any groups of animals.

D. M. S. WATSON.

Useful Aspects of Geology. By S. J. SHAND, D.Sc., Ph.D., F.G.S. Second edition. [Pp. x + 183, with 30 figures.] (London: Thomas Murby & Co., 1934. 6*s*. net.)

THERE are many small introductory works available which deal with the study of elementary geology, but none of them treat the subject in quite the same way as does Prof. Shand in this little book; and with the ever-increasing applications of geological science to everyday problems such as water supply, the finding of oil and the choice of roadmaking and building stones, there is no doubt that *Useful Aspects of Geology* will appeal to an even wider circle of readers in its second edition than in its first.

As might be expected, the portion dealing with palæontology, that branch of the science which becomes more and more the province of the specialist, is kept within wisely narrow limits, while the study of rocks proper is set out in a way which is simple and yet adequate for elementary purposes. The difficult subject of metamorphism is also well covered, a special feature being the avoidance of long names which only succeed in terrifying the beginner.

The diagrams are exceedingly clear, being almost equal in this respect to the solid models of the lecture-room table, and are far more effective for their purpose than photographs. The bibliography which is appended at the end of each chapter is a feature which commends itself, the books given being evidently chosen with care.

Besides its special use for students of applied geology, civil engineering and surveying, mining, oil technology, etc. the book would form a welcome addition to the shelves of a geographical library. For clarity of expression and sustained interest the work would be hard to surpass.

B. H. KNIGHT.

BOTANY

An Introduction to Plant Biochemistry. By CATHARINE CASSELS STEELE, M.A., B.Sc., Ph.D. [Pp. viii + 356, with 12 figures.] (London: G. Bell & Sons, Ltd., 1934. 15*s*. net.)

THIS book is intended to provide students of Botany with an introductory account of the chemical nature and relationships of the substances elaborated by plants. The subject matter is divided into seven parts, the first of which is an introduction and contains a brief account of the colloidal State; the second deals with alcohols, fats and oils and the third with aldehydes and ketones and carbohydrates; Part 4 is devoted to plant acids and the remaining three parts to proteins, cyclic compounds and plant metabolism. As stated in the preface, the material is an elaboration of a series of about thirty lectures to students in biochemistry, and the accompanying experiments form a parallel elementary course of practical instruction. Throughout the text there is liberal reference to the botanical source of occurrence of the substances discussed and a separate index of botanical names bears witness to this. The physiological aspect of the subject has also been kept well to the fore and wherever possible it has been introduced into the general text so that the student may be continually reminded of the significance to the plant of the substances discussed. In the last part of the book, under the heading of Plant Metabolism, there are to be found some clear and concise accounts of such subjects as Photosynthesis, Nitrogen metabolism, Respiration, Growth, Fruit ripening and storage and the chemical effects of cold and frost on plants. The book is thoroughly up to date and the author may be congratulated upon having produced a readable book which contains a very considerable amount of information in a comparatively small compass.

P. H.

Transactions of the Bose Research Institute, Calcutta. Vol. VIII, 1932–33. Biological and Physical Researches. Edited by SIR JAGADIS CHUNDER BOSE, M.A., D.Sc., LL.D., F.R.S., C.S.I., C.I.E. [Pp. vi + 266, with 131 figures.] (London, New York, Toronto: Longmans, Green & Co., 1934. 21*s*. net.)

THIS volume of the *Transactions of the Bose Institute* contains thirteen contributions in addition to an Introduction by the Editor which is a summary of the various investigations.

The Table of Contents shows that these investigations cover a wider field than hitherto; naturally botanical work is most represented; but there are also papers on zoology, biochemistry, physics and anthropology.

In addition to the Introduction, Sir Jagadis Bose contributes some observations on the reactions of fish to sound and water disturbance. In the same paper he describes the effects of extracts of *Millettia pachycarpa* and *Cajanus indicus* on various fish. *Millettia* is known as a fish poison and contains saponin; it is, therefore, not remarkable that it proved fatal to all the fish. The extract of *Cajanus* also proved toxic, but to a lesser degree, and a dilution was found that was fatal to one kind of fish but not to the other three experimented on. It would be interesting to know what is the toxic principle in *Cajanus*, a food and fodder plant; no authority, as far as has been ascertained, describes it as having poisonous properties.

Das and Palit describe the sleep movements of *Averrhoa* in darkness and in strong light. They also record their observations on the excitatory impulse in plants which, unlike *Mimosa*, show no obvious response to tactile stimulation. They find that the motor mechanism is essentially similar in sensitive and in ordinary plants.

Guha-Thakurta and Dutt contribute two papers: one on the effect of external stimulation on the diametric growth of stems, and the second on the automatic recording of the elongation of roots and variations in the rate induced by light, electric shock, ether, carbon dioxide and ammonia.

G. P. Das describes the effect of adrenalin and other drugs on the movements of the frog's heart and the leaflets of *Desmodium gyrans*, and N. N. Das describes the action of extracts of various plants on the peristalsis of different regions of the alimentary canal of the frog.

Nag records his observations on the manganese content of the leaves of various plants, mostly Coniferæ. He finds the amount varies according to habitat. The female cones have a relatively large amount and the leaves of the fertile branches show an increased content during cone production. This does not obtain in the male cones. The value of the results would have been enhanced by estimations of the manganese content of the soils of the various habitats.

Nag and K. N. Bose describe an organic principle isolated from *Eupatorium ayapana*.

Banerjee characterises the fats of *Phaseolus radiatus* and *Cicer arietinum*.

Basu considers the racial affinities of the Mundas, a primitive Indian people. Whilst the cranial measurements indicate a general affinity with the Veddahs and Australians, they show marked differences from the Proto-australoid type characteristic of the prehistoric folk of the Indus valley.

Finally, there are two papers on the absorption spectra of silver and lead halides by Datta and Deb respectively.

T. G. H.

Identification of the Commercial Timbers of the United States. By H. P. Brown, Ph.D., and A. J. Panshin, Ph.D. [Pp. xxvi + 223, with 60 plates and 6 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 18*s*. net.)

Identification of the Timbers of Temperate North America: including Anatomy and Certain Physical Properties of Wood. By Samuel J. Record, M.A., M.F., Sc.D. [Pp. x + 196, with frontispiece, 6 plates and 47 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1934. 18*s*. 6*d*. net.)

With the appearance of these two works it is not too much to claim that the commercial timbers of the North American continent are adequately monographed as to their anatomy; the identification of the commoner woods of that part of the world should now prove a relatively simple matter, at least generically, for in both works one is impressed with the difficulty or even impossibility of identifying closely related species on anatomical features.

The treatise of Brown and Panshin forms the first volume of an ambitious undertaking—The American Forestry Series—a work which, it is hoped, will ultimately provide a set of text-books and works of reference for the whole field of forestry. In the present volume there are introductory chapters on the structure and physical properties of wood; but these have been reduced to a minimum, and the remainder of the book is occupied by two keys and a section upon the specific anatomy of the woods dealt with in the keys. The first key is based upon gross features, such as are visible to the unaided eye or with a × 10 lens. In the second key the diagnoses involve minute features. The use of the keys is greatly facilitated by nearly 300 photomicrographs depicting mostly transverse and tangential longitudinal sections of the timbers dealt with; those intended for use with the first key are × 5, those with the second × 75. Both authors and publishers are to be congratulated upon the uniform excellence of these illustrations; further, the magnifications selected are neither too high nor too low, a not uncommon failing with illustrations of this type. It is to be regretted that the authors have followed the usual practice of figuring transverse and tangential longitudinal sections, while radial longitudinal sections are almost completely ignored; perhaps this practice may be defended as far as dicotyledonous woods are concerned, but it is surely desirable that radial longitudinal sections of coniferous timbers should be figured; this would, it is true, greatly have increased the number of illustrations, unless with coniferous woods, the radial section had replaced the transverse, surely a gain rather than a loss in the value of the illustrations.

Prof. Record's book has been written to replace his well-known *Identification of the Economic Woods of the United States.* It adopts the same general lines as Brown and Panshin, but the first part—"Anatomy and Certain Physical Properties of Wood"—covers a much wider field: the author deals with the anatomy of woods generally, and his text covers the ground more completely than any former treatment of this subject. This section of the book should prove valuable as a reference book to students of plant anatomy on all points concerning the anatomy of the secondary xylem. The second part of the book is a key to the North American timbers, followed by descriptions of the woods dealt with in the key; the descriptions concern the distribution, abundance and uses of the timbers: this section of the book is illustrated by some 36 photomicrographs, but some of the transverse sections are disappointing in depicting far too small an area to be of value in identification. In the reviewer's opinion these illustrations do not reach the same standard of usefulness as those—mostly photomicrographs—which illustrate the first part of the book.

Although covering much the same ground these two books are complementary; for general wood anatomy Record's work is indispensable, while the identification of the North American timbers receives fuller treatment in the hands of Brown and Panshin.

F. W. J.

ZOOLOGY

Introduction to Cytology. By Lester W. Sharp, Cornell University. Third edition. [Pp. xiv + 567, with 230 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1934. 30s. net.)

The second edition of this deservedly well-known text-book appeared in 1926. Since then a considerable amount of important cytological work has appeared either in new fields or in old attacked by newer or more precise methods, with the result that many undecided or controversial points that then seemed important have either been solved or fallen into comparative insignificance and other aspects are warmly discussed or have become the centres of interest.

The ultra-microscopic nature of the cell wall, the observation of living cells and nuclei, especially in connection with cell formation, nuclear and chromosome structure, the morphology and structure of the chromosomes, have proved especially fertile fields and with the study of meiosis, including unusual chromosomal behaviour such as fragmentation and translocation, of heteroploidy and cytogenetics, have been very productive of results and connect up better than ever before with results obtained in genetics.

The work has been brought up to date in figures, text and bibliography, so as adequately to deal with such matters and has been pruned severely and rearranged. While setting out to treat chiefly of the structural and genetical aspects of cytology it by no means neglects other aspects, *e.g.* the physical and chemical; indeed, the present edition presents an excellent account of the present state of cytology.

E. M. C.

The Natural History of the Herring in Scottish Waters. By HENRY WOOD, M.A., Ph.D. The Buckland Lectures for 1932, Third Series. [Pp. 36, with 7 figures and 2 charts.] (Aberdeen: The Fishing News, 1934. 6*d.*)

FRANCIS TREVELYAN BUCKLAND died in 1880, and under his will he left to the nation a sum with which to endow a Professorship of Economic Fish Culture. The Buckland Foundation (1926) administers the income derived from the Trust Fund, and three trustees are appointed to carry into effect a scheme for the application of the bequest. This scheme provides for the appointment annually of a Professor of Economic Fish Culture, whose duty shall be to deliver lectures at such places, in the United Kingdom or Ireland, as the Trustees shall from time to time determine. Dr. Henry Wood, Senior Naturalist of the Fishery Board for Scotland, held the appointment of Buckland Professor for 1932 and delivered lectures in Scotland on the herring and its fishery in Scottish waters. The full text of the lectures was first printed in current issues of the *Fishing News* and afterwards re-issued in booklet form. To those wishing to become informed of the progress made in the scientific study of herring fishing, nothing superior to Dr. Wood's account could be recommended. In it will be found a clear and concise description of the methods by which salient facts in the natural history of the herring have been determined and the knowledge applied to the problems of the commercial fisheries. Dr. Wood shows that there are still gaps in our knowledge, but he remains calmly confident that they will eventually be filled. Judging by the happy note on which he concludes, we may find assurance that his future investigations will be as successful as those already completed: "The scientist is aware more than anyone else of the work remaining to be done, and in tackling this work there is nothing more appreciated and encouraging than the sympathetic interest and helpful criticism of the men who handle the herring, whether at sea on a drifter or on land in the curing yard." That, surely, is a statement which would have met with the full approval of Buckland himself.

E. F.

The Natural History of the Herring of the Southern North Sea. Being the Buckland Lectures for 1933. By WILLIAM C. HODGSON, D.Sc., Naturalist on the Staff of the Ministry of Agriculture and Fisheries. [Pp. 120, with frontispiece and 10 figures.] (London: Edward Arnold & Co., 1934. 3*s*. 6*d*. net.)

THE Buckland Foundation administrates a fund left to the nation by Francis Buckland for the endowment of a Professorship of Economic Fish Culture. The scheme provides for an annual appointment of a " Buckland Professor " who shall deliver certain lectures—usually three in the year—in connection with the Economics of the Fisheries. The present work contains the " Buckland Lectures " for 1933. The subject is wisely chosen, for there is no fish more discussed at the present moment than the herring, and there is nobody better qualified to deal with it than Dr. Hodgson who has devoted his energies to the elucidation of herring problems. The lectures were addressed to members of the Herring Industry at North Shields, Grimsby and Yarmouth, but they should be read by all, for the matter is so clearly worded that anyone could understand it and there is much important information brought together in a small space which should be known by the general public and not only by the herring specialist and general scientific man.

The tale of the herring, its life, migrations and fluctuation is a romance. Its interpretation represents years of hard working and hard thinking on the part of an enormous band of enthusiastic scientific experts. One of the results of this combined work is that given certain conditions it is possible to forecast the quality of the herring fishery for some years ahead. The methods by which these results are achieved are fully described in this book. One of the most interesting parts is the description in the last chapter of the failure of the North Sea Fishery for the season of 1933 when the ordinary movements of the herring were interfered with by huge barriers of phytoplankton—minute plants proliferating to such an extent that the herrings were actually prevented from passing through and were obliged to make wide détours, resulting in their being found in quite different places from where they should have been.

The size of net mesh is a very important subject just now, when so much depends on the fishing and selling of fish of only the best quality. All this is fully discussed, and the book ends on a cheerful note urging the fishermen in their own interests to concentrate on the fishing of adult stock only—a question of vital importance.

M. V. L.

MEDICINE

Veterinary Helminthology and Entomology: The Diseases of Domesticated Animals caused by Helminth and Arthropod Parasites. By H. O. MÖNNIG, B.A., Dr. PHIL., B.V.Sc. [Pp. xvi + 402, with 264 figures, including 12 plates.] (London: Baillière, Tindall & Cox, 1934. 30*s*. net.)

THIS book deals with the most important diseases of domesticated and fur-bearing animals caused by helminth and arthropod parasites in all parts of the world. It is divided into four main sections as follows: 1, *General* (pp. 2–10) treats briefly of parasitism, pathogenicity, host organ specificity, resistance and immunity, epizootology, geographical distribution and historical. 2, *Technique* (pp. 13–22). Under this head short accounts are given of special methods for collecting and preserving parasites, making permanent preparations, special diagnostic methods including examination of excreta, egg-counting technique and cultural methods for larvæ. 3, *Helminth Parasites* (pp. 22–259). This large section deals with parasites belonging to the classes Trematoda, Cestoda, Nematoda, Acanthocephala and Annelida. Each class is introduced with a brief general account and is then dealt with under orders and families whilst the chief genera and species within each family are succinctly described. 4, *Arthropod Parasites* (pp. 259–381). After a general introduction this section is divided into two parts A and B. Under (A) the directly poisonous forms of Insects and Myriapods are discussed, whilst under (B) the Arthropods as parasites or as transmitters of disease are considered, including Insects, Arachnids and Pentastomids. The same arrangement of the subject matter is followed as in the helminth section; the classes being dealt with under orders, families and genera.

The morphological descriptions of the various organisms throughout are reduced to minimal dimensions but the author has aimed at giving sufficient data to enable each parasite to be identified. For most of the forms considered besides the description of the parasite, there are included particulars on life-cycle, pathogenicity, symptoms, diagnosis, treatment and prophylaxis; all of which are set out in admirably brief form. The practical aspects of medication are adequately described.

In addition to several plates, the book is illustrated with a large number of excellent line drawings the majority of which are from originals by the author. It is a pity, however, that most of these lack scales of magnification or any indication in the accompanying legend of the enlargement. The author is to be congratulated on the lucid, concise and practical manner in which he has presented his subject, and the publishers on the excellent appearance of the book, which should prove very valuable to veterinary students and practitioners.

T. G.

Essentials of Histology. Descriptive and Practical. By Sir E. SHARPEY-SCHAFER, F.R.S. Thirteenth edition, edited by H. M. CARLETON. [Pp. x + 618, with 721 figures and 2 plates.] (London, New York, Toronto: Longmans, Green & Co., 1934. 15*s*. net.)

IT would be superfluous to praise a text-book which, now forty-nine years old, has been revised twelve times by its original author and once by Dr. H. M. Carleton, and has sold ninety-two thousand copies. This history speaks for itself. In the present (thirteenth) edition, certain portions, such as the sections on the development of blood corpuscles and on ossification, have been rewritten, and a number of the older illustrations have been replaced by new figures, mainly photographs of microscopic preparations. But the changes are not considerable, and one has the feeling, as one turns the pages fresh from the press, of welcoming again an old and trusted friend.

We suggest that in future editions the statements that the nuclear membrane is made out of chromatin (p. 11), and that the chromosomes form a spireme during mitosis (p. 15 and elsewhere) be dropped. Also, although the book is essentially a treatise on mammalian histology, the occasional excursions into the histology of other forms might perhaps be expanded and increased in number. The cilia of the blue mussel and the muscle fibres of insects afford admirable material for practical study and are already included; space might perhaps be found for the beautiful and easily observed amœbocytes of the garden snail, for the egg cells in the ovary of the cockroach, and for the sperms and eggs of the sea-urchin, in which animal fertilisation and early segmentation can be so easily observed.

G. P. Wells.

Psychology and Psychotherapy. By William Brown, M.D. (Oxon.), D.Sc. (Lond.) F.R.C.P. Third Edition. [Pp. viii + 252.] (London: Edward Arnold & Co., 1934. 12*s.* 6*d.* net.)

The third edition of Dr. Brown's *Psychology and Psychotherapy* has been so revised and enlarged that it has the appearance of a new work, and should replace the earlier editions on the shelves of those already possessing one; while those who are interested in the many aspects of modern psychology, and have not yet read the book, should find in its pages much that will arrest them and prove of the greatest interest. For Dr. Brown looks upon the problems of psychology from many angles, both theoretical and practical; and indeed he is singularly equipped to deal with them from as many points of view. His own personal approach has been by way of philosophy, physiology, mathematics, statistics, laboratory research, and medicine; in which last he has specialised now for many years as a psychotherapist. This latter practical interest, however, has never led him away from his earlier ones. He is still Wilde Reader in the University of Oxford; he is still Director of the Psychological Department at Bethlem; and no less than three Appendices to the present work show how keenly he is still elaborating the statistical analysis of tests of mental ability.

Such a varied outlook to a large extent explains the aim of the book and the plan on which it has been written; though for the most part Dr. Brown confines himself to topics of practical usefulness and, in particular, to those which have a definite bearing upon human life in health and in disease. He shows how the forces that make for or militate against the wellbeing of the individual may also play their part for or against the wellbeing of communities, and points out how psychology itself ramifies into the problems of ethics, politics, economics, and the like, with which the world is faced—with which we are attempting to grapple—at the present day. To do this successfully it is not enough to study the normal mental processes of human beings. The abnormal must be studied as well; for it is in the abnormal that the misdirected mental forces may be seen at work. Accordingly, we have chapters on Mental Dissociation, Multiple Personality, Dream Theory, the Unconscious, Theories of Emotion, War Psychoneuroses, Alcoholism, Adolescence, Suggestion and Faith, Personal Influence, and the Psychology of Peace and War, following one another, and presenting a growing picture of cultural problems sketched in over the background of personal ones. Not every reader will agree in detail with Dr. Brown in respect of all the subjects that are touched upon in this book; but everyone will be impressed by the general thesis. Two chapters of a more philosophical and speculative character follow the topics cited. One is on the Relation of Mind to Brain, and the other on Psychical Research, to which is added a short passage on The Eternal Values. Besides the three Appendices noted above—a Statistical Examination of Mental Ability, the Two-Factor Theory, and the Sampling Theory—which will not perhaps so greatly interest the general reader, there are two others that certainly will. In the first, three cases of War Neurosis are described ; and the second consists in an account of a deep mental analysis that covered several years, written by one of Dr. Brown's patients himself. These two Appendices show, by way of samples, the kind of data upon which psychopathological theory and psychotherapeutic practice are based. But such samples, and even the data derived from his own large practice in medical psychology, are not the only sources of Dr. Brown's wide knowledge in this sphere. He is obviously acquainted with the extensive literature concerning it ; and, by way of balanced criticism, he has reached a position for himself which, as he modestly says in his Preface, is "synthetic rather than eclectic, and (he hopes) free from at least glaring contradictions."

F. A.

The Circulation of the Blood. By WINIFRED PARSONS, M.A. Sheldon Books of Popular Science. [Pp. 204, with frontispiece and 30 figures.] (London : The Sheldon Press ; New York : The Macmillan Co., 1934. 5*s*. net.)

THE general editors of the Sheldon Books of popular science write : "In the middle ages of western Europe, the everyday life of the vast majority of men and women was dominated by Religion and Theology, but now, and in the immediate future, this dominance belongs and will belong, for better or worse, to Science. It is important that those whose lives have to be spent in such a period should have some understanding, not so much of the results of scientific work as of its methods, its way of thought, its presuppositions, its world outlook. We hope that this series of books, written in untechnical language by scientific workers actively engaged in research, will contribute to such an understanding." Rarely does a book so aptly fit the publisher's advertisment as *The Circulation of the Blood* by Winifred Parsons. The hopes of the editors should surely not be vain. The easy almost conversational style and clear expression of this most important—really vital—subject will enable the uninformed reader to realise many facts about his or her personal machinery which hitherto were only vaguely felt, and may introduce the inquiring amateur to the elementary methods of physiology and research. It would perhaps be useful if the key letters in Figs. 5, 6 and 7 were included in the text.

The chapter on the electrocardiograph strikes a less elementary note and will appeal to more advanced readers, to physiological and medical students. The final chapter on the history of the subject from Aristotle to Harvey and Stephen Hales is well arranged and forms a fitting appendix to this in every way successful little book.

P J.

PHILOSOPHY AND THE HISTORY OF SCIENCE.

Science in an Irrational Society. By H. LEVY. Conway Memorial Lecture, 1934. [Pp. viii + 82.] (London : Watts & Co., 1934. 2*s*. net.)

IT is always interesting to hear the views of men of science other than professional psychologists upon the laws that govern conscious human beings considered as individuals in relative isolation from one another and also as members of social groups. This small volume, the importance of which is certainly not commensurate with its size, shows the precise thought that a mathematician has given to the subject. It shows, too, how that thought, like any other exact thinking directed towards the solution of a scientific problem, is motivated by a practical and very human aim. To discover the laws of social change, and to understand them, means that we are on the high road towards making use of them, just as we are able to make use of many of the laws of physical Nature to secure our material and cultural ends. Even if the laws differ, as Prof. Levy shows they do, in that subjectivity, purpose and feeling enter into human activity; even if history teaches us that "the types of society mankind has forged in the past have never been conscious of their future"; it does not follow that those laws cannot be consciously and purposively applied to the moulding of a better and a nobler social type, in which the constituent individuals will enjoy a fuller development and a truer freedom. Prof. Levy distinguishes between physical and social laws on the ground that the former are "brought to light," whereas the latter are "brought into being" by experimental methods. "A social group creates its laws, and in certain circumstances may assert that it proposes to make a certain event occur—to make a certain prediction *come true*." We cannot, perhaps, read into this all the implications of organismal theory; for the distinction made between properties inheritent in isolates, whether physical or social, and dependent upon environment, would rule this out. But if the whole should, in fact, determine the characters and activities, including the growth and development, of its every part; if the isolates we study in order to discover laws are artifacts, and the real laws are laws of wholes and of relatively segregated wholes; then we come very close to his final conclusion that "the irrationalist . . . resorts to barbaric methods in striving to attain his irrational end"; for he vainly tries to set himself in opposition to the whole of which he is a part. "But the laws of matter, the human laws of individuals and of social life, are expressive of necessities that ultimately compel the reason of man to adjust itself to their nature. Thus irrationality in the universe works out its own destruction."

F. A.

Newton and the Origin of Colours. A Study of one of the earliest examples of scientific method. By MICHAEL ROBERTS and E. R. THOMAS, M.A., M.Sc. Classics of Scientific Method. [Pp. viii + 133, with 8 plates and 13 figures.] (London: G. Bell & Sons, Ltd., 1934. 3*s*. 6*d*. net.)

THE *clou* of this book is a reprint of the paper which Newton sent to the Royal Society on February 6, 1671/2, containing an account of his investigations of the prismatic spectrum carried out some five years earlier and first made public in the lectures which he delivered at Cambridge in the years 1669 and 1670. The reprint occupies 20 pages; it is preceded by a most attractive exposition of the development of scientific method in the seventeenth century, together with an account of Newton's early life and work, of the optical work of his predecessors and contemporaries and of the controversies into which he was drawn by the publication of the paper.

The authors, while doing no more than justice to Newton's genius, are perhaps a little over-generous in their estimate of his personal character. Thus on p. 65 they state ". . . Newton, though he was aware of the importance of his discovery [relating to colours] was modest about his own share in it," and quote the letter he wrote to Oldenburg on January 6, 1671/2, in support of their statement. In this letter he said "'at the reading of your letter I was surprised to see so much care taken about securing an invention to me, of which I have hitherto had so little value'," but here he is referring to his invention of the reflecting telescope. He first refers to his paper on Colours in a letter to Oldenburg dated January 18, 1671/2, and describes his work as "being in my judgment the oddest, if not the most considerable detection, which hath hitherto been made in the operations of nature"—modesty indeed! Again, on p. 93, Newton is quoted as having written with reference to Hooke's comments on his paper that he was glad "that so acute an *observer* has said nothing that can enervate any part" whereas, according to Birch, in his letter to Oldenburg on February 20 he described Hooke as an acute *objector*—a very different though possibly quite inoffensive expression.

The last chapter of the book contains a brief account of post-Newtonian developments in Optics. It is perhaps less excellent than the rest of the work, and there are several statements which would seem to require alteration or qualification. Thus, on p. 124, we read that Huygens "did not see that darkness would be produced by the constant arrival of two wave trains of equal strength, but in opposite phases" which, of course, is true for the very good reason that Huygens did not contemplate the existence of wave trains at all. On p. 127 the introduction of the conception of transverse waves is attributed to Fresnel and dated 1814. In fact Fresnel himself agreed that "Dr. Young is the first who has announced positively the possibility of such a property of an elastic fluid"; and Young first put forward the suggestion in a letter to Arago dated January 12, 1817 (Peacock, I, 390–2). The matters dealt with in the two preceding paragraphs are, however, of small significance when the quality of the book as a whole is considered. The selection of the plates, which include an engraving of Newton's birthplace and Loggan's print of Trinity College in 1690, form a better indication of the care taken in its preparation. It forms a most useful addition to the literature of the history and method of science and can be recommended unreservedly to the attention of all interested in those subjects.

D. O. W.

Faraday. By THOMAS MARTIN. Great Lives Series. [Pp. 144.] (London: Gerald Duckworth & Co., Ltd., 1934. 2*s.* net.)

THE story of Faraday's life is remarkable even among the many remarkable life histories revealed by the study of the careers of famous men of science. His parents were of good Yorkshire stock but very poor and it is said that when he was 10 years old they were in receipt of public relief. His boyhood was spent in the mews off Manchester Square and at 13 he went to work as errand boy. At 21 he had completed his service as bookbinder's apprentice and had acquired some knowledge of science by reading books in his master's shop and by attendance at Mr. Tatum's lectures, each of which cost him a shilling, a shilling often provided by his elder brother Robert, the gas-fitter. Next year he became Davy's laboratory assistant at the Royal Institution—a post soon relinquished in favour of an eighteen months' tour through Europe as Davy's scientific assistant. Back at the Royal Institution, his reputation grew apace so that at 32 he was elected first secretary of the Athenæum, at 33 Fellow of the Royal Society, at 34 Director of the Laboratory at the Royal Institution and fully launched on the career which was to establish his fame for all time.

The details of this career are sketched in a masterly way by Mr. Thomas Martin the editor of the *Diary* now in course of publication. Much of the scientific work which he describes will be familiar to all readers of this review, but even those who have read Bence Jones's volumes will enjoy the very vivid pictures given to us of Faraday's private life—of his love of music and childish games, of his work as an elder of the Sandemanian Chapel and of his loss of that office when, being commanded to dine with the Queen one Sunday, he was absent from the meeting. A few details of Faraday's income are available. His salary from the Royal Institution was always wretchedly small—only £300 in 1853; but once he became known he was able to augment his income by consulting fees, and he held several posts, so that from his fortieth year onwards his income cannot well have been less than £1,000 a year. He had also his rooms in Albemarle Street and so could live in comfort in the manner he desired. He had one serious period of ill-health, due to overwork, and as a result did but little research during the period 1840–44. His last paper—on regelation—was published in 1860, and the experiment he performed on March 12, 1862, in his seventy-first year, concluded the long record in his diary. He died, after two years of gradually increasing weakness, on August 26, 1867.

Mr. Martin's book forms one of Messrs. Duckworth's series of Great Lives and we must count ourselves fortunate that they succeeded in persuading him to write it for our very great pleasure.

D. O. W.

MISCELLANEOUS

Ocean Waves: and Kindred Geophysical Phenomena. By VAUGHAN CORNISH, D.Sc. With additional notes by HAROLD JEFFREYS, M.A., D.Sc., F.R.S. [Pp. xvi + 164, with 26 plates.] (Cambridge: at the University Press, 1934. 10*s*. net.)

DR. VAUGHAN CORNISH has been well known as a student of waves since his first paper on the topic appeared in the *Geographical Journal* in 1897. He has been fortunate in having both freedom to travel widely and a purpose for his journeys—to study snow waves in a Canadian winter; sand waves on the sea beaches of England and Wales, and in the desert borders of Egypt; waves of the lakes, the narrow seas, and the oceans, as well as those of more confined waters; and, not least, the tidal bores of several rivers. This book is essentially a record of his observations all of which include, as an essential part, measurements as accurate as the conditions of observation allowed. To these he has added many reports of comparable observations by others. The book is a record which will be of great value in the further study of waves and wave-motion. In some cases he has found that individual observations are necessarily incomplete; and he makes an interesting suggestion for team-work to secure a series of records of the movements along the course of the Trent "eagre" which may be commended. The twenty-six pictures are excellent examples of the author's skill as a photographer and of well-selected illustrations.

Dr. Harold Jeffrey has contributed, in the final fifth of the volume, a note on the mathematical theory of waves in so far as it applies to those of Dr. Cornish's results which can be explained at present; and he draws attention to those which are as yet unexplained by theoretical mathematics.

C. B. F.

Africa : A Social, Economic and Political Geography of its Major Regions. By WALTER FITZGERALD, M.A. [Pp. xvi + 462, with 90 maps and diagrams.] (London : Methuen & Co., Ltd., 1934. 16s. net.)

THIS is the latest volume in Methuen's series of advanced geographies ; and it goes far to fill one of the more awkward gaps in the literature of the subject. Before its appearance there was no comprehensive work on the geography of Africa, except for some elementary texts. Doubtless one of the reasons for this lack was the difficulty of producing a homogeneous work which arises from the great variety, in both kind and quantity, of the material available for different areas. Egypt and Abyssinia, the Union of South Africa and the Portuguese African possessions, and even the Katanga and Northern Rhodesia provide instances of this disparity. If, then, some parts of the present work look rather thin as compared with others, the blame should not be laid at the author's door.

The work is divided into three parts, of which the first two, covering about a third of the volume, are concerned with the physical and human aspects of the continent as a whole. Part III is devoted to a regional treatment of the broader geographical divisions of the continent. This is the essential work ; and it is well done. The author has avoided the temptation to think only in terms of economic geography, and of economic geography only in terms of the exploitation of the natural wealth by European industrial and commercial interests. In particular his analysis of the interrelations of White and Black under a variety of geographical settings and political régimes gives ample evidence of his willingness to use his geographical studies as bases on which to form judgments. His particular conclusions may be accepted or rejected according to the prejudice of the reader, but will be widely regarded as sound. In any case it is the author's readiness to weigh and evaluate the material available that lifts the work above the level of a mere text-book. Good select bibliographies and an adequate index are included.

R. O. B.

The Handbook of Palestine and Trans-Jordan. Edited by Sir HARRY LUKE, C.M.G., B.Litt., M.A., and EDWARD KEITH-ROACH, O.B.E. Third edition. [Pp. xvi + 549, with frontispiece and map in end pocket.] (London : Macmillan & Co., Ltd., 1934. 16s. net.)

THE chief characteristic of Palestine is its variety, human and physical ; but it may be news to some readers that no less than sixty languages are spoken in that small country, whose land area is less than ten thousand square miles and population only just over one million (census of 1931). The religious complexity is almost as great as the linguistic.

The seven sections, or chapters, on Palestine deal respectively with (I) Geography and History—very little geography, in contrast to a good summary of the history ; though both aspects are similarly complex and important. (II) Peoples and Religions—a clear and useful account ; though the reader may wonder whether the so-called "racial" strife is not in fact religious strife. (III) Archæology—with a full treatment of ancient and medieval architectural remains. (IV) Communications and Information for Tourists. (V) Government and Government Activities—which indicates the manifold nature of administrative work in a land which until recently entirely unequipped for modern civilised life. It may be noted that Railways and Posts and Telegraphs are described in section V rather than in IV. (VI) Commerce, Industry and Miscellaneous. This includes accounts of the three calendars in use by Jews, Muslims and Orthodox Christians; and of the exploitation of the salts of the Dead Sea. (VII) on Geology, Mining and Natural History, a section which suffers even more than most of the others from the absence of diagrams and other illustrations.

Trans-Jordan is described, more briefly, on the same plan.

The book is a careful and authoritative compilation; it includes lists of the Government publications, and of other references.

The map is that by the Survey of Palestine on the scale of 1 : 750,000. It leaves very much to be desired. On this scale, and given a survey of the type described on pp. 275–8, it should be possible to show contours at much closer intervals than 300 metres, with less generalisation, and perhaps with a distinction between surveyed and sketched contours. Also the occasional figures of altitude given in the text would be more useful on the map. The boundaries, and names, of districts and sub-districts should be shown on the inset. The "unending sinuosities" of the Jordan (p. 8) are not in any way suggested on the map; and no other stream is even named, though the text names four other rivers and many wadis whose position a reader may wish to know. The map generally uses the spelling of names adopted by the Government, and referred to on p. 383; but the text spelling differs in many places (e.g. on pp. 6, 7, 77, 278, etc.). Apparently this map was not used by the editors. It is to be hoped that in future editions a much better map can be supplied, and that the text and the map will be in agreement.

More detailed climatic data would also be of value. In this edition "Climate" is disposed of in four paragraphs, on pp. 283 and 299–300, with precise but inadequate statistics for Jerusalem only.

The fullness and authoritative character of the accounts of the history and archæology, of the peoples and religions, and of the administrative services, have established this as the best reference book on those aspects of Palestine and Trans-Jordan. Its treatment of other aspects of the lands should be raised to the same level.

C. B. F.

Dust. By S. Cyril Blacktin, M.Sc., Ph.D., A.T.C. [Pp. xii + 296, with 4 figures, including 2 plates.] (London: Chapman & Hall, Ltd., 1934. 18*s*. net.)

The term "dust" conveys to the ordinary person a powdery something of no value and a nuisance, which it is the business of the good housewife to remove. But a glance at the headings of the various chapters, which make up the contents of this book will enlarge his view of the real meaning of the word in its varied applications. One has only to note references to dust storms, coal dust in its physical and industrial aspects, cosmic dust, etc. to realise the wider significance of the term; and indeed the author considers that the very existence of the human race is bound up with dust. At the same time it should be pointed out that he gives the term a very generous interpretation, for it embraces particles of very diverse sizes. The book is divided into chapters on Dust in nature, in experimental science, in industry and in physiology and each is developed in considerable detail and with very complete references.

The opening chapter on cosmic dust gives an account of the meteorites falling on our earth from Heaven knows where. It is followed by one on earth dust movements promoted by the combined action of wind and gravity. The chapter which will probably appeal to the general reader will be that on "dust in everyday experience." Here we read of the effect of aerial dust on visibility. "A very clear air," so the author says, "may contain 30,000 to 60,000 particles in a cubic centimetre" yet "a breeze raising visible road dust may have added but 1,000 particles" to those already present. Referring to smoke pollution, he points out that the larger suspended particles of smoke and dust may cut off a large proportion of sunlight and 37 per cent. loss has actually been recorded in London. Besides its effect on light the smoky atmosphere in industrial areas may be the cause, as some authorities maintain, of pulmonary and bronchial disease as well as heavy monetary loss for cleansing and repair of buildings, estimated for the West Riding of Yorkshire at about £6 million per annum.

Various distances are given by different authorities which smoke particles may travel, and the writer of this review has observed the occasional formation of films of soot on Coniston Lake when the wind (S. or S.E.) has been from an industrial quarter, possibly Leeds or Bradford, a distance of nearly 90 miles. The loss from soot of domestic and industrial fuel has been computed at 3 million tons per annum in this country, and the further monetary loss from the additional cost of cleansing must be considerable; for Messrs. Simon & Fitzgerald estimate that household washing in Manchester would be reduced by £250,000 a year if the air were as clean as at Harrogate. How far the nuisance can be controlled is exemplified by Salt Lake City, which consumes annually half a million tons of bituminous coal and where, by strict inspection, the air pollution has been reduced by 85 per cent.

A new source of dust pollution indicated by the author is from increasing motor and air traffic by the introduction of rubber dust and waste carbonaceous products, and he estimates that the 8 million tyres on the 2 million cars licensed in Great Britain will produce 6×10^{18} particles of 20 microns diameter per annum. Full details of this interesting calculation are carefully recorded; but need not be discussed.

To turn now to the scientific views of dust, it may be a matter of surprise to some that dust is a colloid, or, as it is termed, an "aerosol," or in some cases an "aerogel," and may be any size below the ultramicroscopic, its chief characteristic being the tendency to further disintegration. "Smokes" are regarded as the forerunners of dusts and share with them this property of disintegration. Perhaps the most visible effect of the dust layer surrounding the globe, or as the author terms it the "staubosphere" (though why he should borrow a German word is not obvious), is that of scattering the sun's rays and producing, under different conditions, diffuse daylight, the blue of the sky and the sunset colours, that is, light of every degree of refrangibility by particles of different sizes. A further property of dust particles is the formation of rain; for it is these particles which form the nucleus for vapour condensation when the temperature falls. Were it otherwise there would be no rain, but water would stream down every solid object with which the vapour was in contact.

The ingenious apparatus devised by Aitken and known as Aitken's dust-counter is based on the sudden cooling by expansion of saturated air. It reveals the number of dust particles in a small chamber of known dimensions by the number of drops of water deposited on a squared illuminated field. It should be added that other dust counters have been invented and are fully discussed by the author, notably that by Whytlaw-Gray and Patterson.

The section on dust in Geology deals generally with abrasion in wind-swept areas, effects of rain and rivers in transporting finely divided material and dust from volcanic and meteoric sources. The mighty explosion of Krakatoa in 1883 produced a cubic mile of dust forming a layer 5 to 15 miles high which travelled three and a quarter times round the earth and continued for years to illumine the evening sunset sky with brilliant colour. Other sources of widespread atmospheric dust are the dust storms, which in total masses of many tons may travel thousands of miles. We must pass over the vegetable dust in the form of spores, seeds, moulds and algæ which may be carried, like inert dust particles, many miles and those more minute forms of living matter which as pathogenic organisms are only too well known as disseminators of disease.

Of industrial dusts by far the most important is that from coal which is responsible for all-too-frequent colliery explosions. The subject has naturally created a very widespread interest and attracted many scientists whose object has been to prevent these disasters. Other dusts, such as those from flour, sugar and starch are liable to explode under certain conditions, but none are so sensitive to ignition as that from coal. The results of research with full references form the principal subject of this section but cannot be discussed in a short review.

From another point of view pulverised coal is a useful combustible which has many advantages over the raw material and has been the subject of much technical study and now of widespread practical application. One interesting phenomenon, which from being a scientific curiosity has developed into an industrial process, is the removal of dust by electrical precipitation whereby valuable by-products have been regained and prevented from otherwise contaminating the atmosphere. The above review will convey an idea of the scope and character of the book; and it may be added that as far as can be judged by one who does not profess to be able to appraise the value of each section of the book, it has every appearance of having been conscientiously compiled, and, what is of great importance, contains, in addition to the ordinary indexes, a reference number in the text to an index containing the names and publications of the various authorities.

J. B. C.

Rebel, Destiny. Among the Bush Negroes of Dutch Guiana. By Melville and Frances Herskovits. [Pp. xviii + 366, with 15 plates and maps as end-papers.] (New York and London: Whittlesey House, McGraw-Hill Book Co., Inc., 1934. 12*s*. 6*d*. net.)

This is an anthropological book of an unusual type. It is cast in the form of a travel narrative and the anthropological material is incorporated in conversational form. This has certain great advantages; many anthropological books, though they may be of great scientific interest, are not altogether good reading; in fact one reads them not for themselves but for the information to be dug out of them, and if that is transferred to the notebook or memory the book is thenceforward only of value to fill in gaps in these records. The present book, however, has in itself literary and dramatic value and should appeal to a wider public than the anthropologist alone. At the same time the authors have done their work in that careful and learned spirit of inquiry which is always associated with their studies. The disadvantage of the method is that the narrative is necessarily somewhat discursive and the search for facts might be difficult. This is, however, got over by the excellence of the index and the care with which the cross-headings to the pages have been written. There is a useful appendix and glossary of the language, but it would have been interesting if the authors had rather enlarged what they have to say on its structure. The illustrations are well chosen and informative. The authors have been at particular pains to show the geographic background in which the people live, and this in itself adds considerably to the value of the book.

L. H. D. B.

An Introduction to the Teaching of Science. By PROFESSOR ELLIOT ROWLAND DOWNING. [Pp. viii + 258.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1934. 9*s*. net.)

THIS is a revision of the author's well-known *Teaching Science in Schools*, and is the text on which he bases his lectures in the Department of Education of the University of Chicago. In view of the relative lack of good books dealing specifically with the subject, it will probably find a ready welcome.

Prof. Downing begins by considering what should be the aims of the teacher of science (and this term includes biology) and the various methods available to him. A careful discussion of the merits and demerits of these methods follows and there is also a very valuable chapter on the newer ways of testing results and achievements.

An excellent feature of the book is the care with which the author refers to specific researches on the points he discusses. All too often—especially in England—we argue at great length over such points as to whether individual work or demonstrations are more effective, or whether the heuristic method is superior to didactic lecturing, without ever referring to the fact that a great deal of objective research has already been done on these points. We may doubt whether this research has any real value; but these doubts should act as spurs to further effort, and, in any case, it probably has more value than our (usually groundless) prejudices.

A wider and more precise philosophical outlook would have improved the book. For instance it might have prevented the author from considering as one of the fundamental principles of chemistry the idea "that atoms of all elements are made up of equal numbers of positive and negative electrons arranged, etc."

The author's style is occasionally irritating—there are far too many constructions such as "So that one is enabled to adjust himself to . . ."—and it is sometimes verbose and obscure. However, in spite of these defects, this book contains many good things and will be found of considerable interest and utility both by teachers of Science and by students preparing themselves for a Diploma in Education.

J. A. LAUWERYS.

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ERRATUM

Vol. XXVIII, page 635, line 24, for "Sir Humphry Davy" read "Dr. John Davy, brother of Sir Humphry Davy."